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PROCEEDINGS

OF THE

ROYAL SOCIETY OF MEDICINE

EDITED BY

SIR JOHN Y. W. MACALISTER

UNDER THE DIRECTION OF

THE EDITORIAL COMMITTEE

VOLUME THE FIFTEENTH

SESSION 1921-22

PART III

SECTIONS:—

OBSTETRICS AND GYNÆCOLOGY	ODONTOLOGY	OPHTHALMOLOGY
OTOLOGY	PATHOLOGY	PSYCHIATRY
THERAPEUTICS AND PHARMACOLOGY		
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I

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SECTION OF OBSTETRICS & GYNÆCOLOGY



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1922

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Section of Obstetrics and Gynæcology.

President—Professor HENRY BRIGGS, F.R.C.S.

Green Exudate on the Back of the Pregnant Uterus.

By Professor HENRY BRIGGS, F.R.C.S. (President).

A GREEN intraperitoneal exudate covering the middle third of the back of the uterus was found on June 30, 1920, during a Cæsarean hysterectomy at the thirty-fourth hour after the onset of a full-term labour obstructed by a fibroid in the left lower uterine segment; the foetal head had remained above the brim of the very slightly contracted pelvis: the cervix not fully dilated; the membranes unruptured; the foetus alive with a reduced heart-rate of 100; labour pains powerful; maternal and foetal stress observed.

Mrs. McD., aged 36, two and a quarter years married; the fibroid had been symptomless and unknown until the fifth month of pregnancy; one day's acute abdominal pain subsided so gradually that she was in bed for three weeks; no fever, no vomiting.

Whether or not this abdominal pain corresponded to the early stages of the green exudate affords scope for clinical and pathological opinion. The exudate was not firm; it resisted complete detachment; its texture was organically cohesive; its organization was feeble; its thickness was $\frac{1}{4}$ in.; it lay upon, and covered in patches, the middle third of the posterior surface of the uterus.

Professor Ramsden, from the Bio-Chemical Department, has very kindly reported the absence of bile pigment and the presence of hæmoglobin or hæmatin; he has since remarked upon the green colour that alkaline hæmatin acquires when diluted.

Dr. R. A. Hendry, after an examination of one of the larger patches, describes an organizing blood-clot with a shell of lutein tissue and granules of pigment indicating a condition of some standing consistent with the clinical history; the origin was not inflammatory; the ovaries and tubes were matted and adherent.

Professor Ernest Glynn has examined the sections and agrees with Dr. R. A. Hendry's description.

It is unnecessary to state that the uterine wall was not ruptured. The intrapelvic site of the tubes and ovaries and their adhesions to the lower uterine segment, the supravaginal cervix and adjacent broad ligaments contributed to the obstruction of the labour; the œdematous, softened and friable

2 Donaldson: *Cancer of Cervix associated with Sarcoma*

condition of these tissues prohibited a steady grip by either heavy or light pressure forceps; repeated tearing of tissue delayed the passage of the continuous catgut sutures.

The intrapelvic site of the tubes and ovaries is additional evidence of the early origin of the green exudate.

The child was asphyxiated and collapsed at birth. The mother made a good recovery. The health of both mother and child is now good.

Cancer of Cervix associated with Sarcoma of Omentum.

By MALCOLM DONALDSON, M.D.

THE following case is of interest, both from the point of view of an exact diagnosis of the nature of the tumours, and on account of the rarity of finding two types of malignant growth in one patient.

K. A., a woman, aged 69, first came to St. Bartholomew's Hospital on August 6 of this year, complaining of uterine hæmorrhage. Patient a multipara; five children. Menopause fourteen years ago. History of rheumatic fever twenty-seven years ago, and of gastric ulcer three years ago.

History of Present Condition.—Twelve months ago, patient noticed occasional pain in stomach, and soon afterwards loss of blood *per vaginam* occurring at intervals of a few weeks. Patient states that in February, 1921, she was operated upon at the Samaritan Hospital for a hernia. (At the same time a tumour was said to have been found and pronounced inoperable; unfortunately, I have not had time to inquire about this.) When she came to St. Bartholomew's Out-patients' Department on August 6, 1921, the following note was made: "Mucous membrane somewhat pale; tongue clean; nothing abnormal discovered in thorax; temperature and pulse normal. *Per hypogastrium*: Abdomen moved well on respiration, and was distended in the lower half. On palpation, a tumour was found arising to the umbilicus and appeared to be fixed. No definite fluid thrill could be felt. On percussion, there was impairment of note, but no absolute dullness. *Per vaginam*: The cervix was atrophied, the edges hard, and a polypus was seen protruding from the cervix. This was removed and sent for microscopical examination. The uterus appeared to the right of the middle line, and not markedly enlarged. The cystic tumour can be felt, with difficulty, at the brim of the pelvis."

On August 21, patient admitted to hospital. After a further examination and inspection of section removed, provisional diagnosis made of (1) carcinoma of the uterus; (2) an ovarian cyst, adherent to intestines.

August 25: Laparotomy under general anæsthetic. Abdominal tumour found to be large cyst of great omentum, with transverse colon adherent all round, and anchoring it by adhesions to brim of pelvis. Adhesions separated with difficulty, and whole cyst dissected out, without being opened. Upper part of tumour solid and adherent to lower border of stomach, in region of pylorus.

Considering the patient's age, and the fact that she had already been some time under the anæsthetic, it was decided to do a pan-hysterectomy, with a

[October 6, 1921.]

cuff of vagina, together with a salpingo-öophorectomy, rather than a full Wertheim operation.

Patient made an uninterrupted recovery, and left the hospital on the eighteenth day.

Dr. Spilsbury very kindly gave the following pathological reports:—

(a) "The polypus originally removed in the Out-patients' Department shows an atypical squamous-celled carcinoma. The cells are unequal in shape and size, and irregular mitosis is seen in the nuclei.

(b) "The polypus removed from the uterus after hysterectomy shows simple polypoidal tissue, covered by squamous epithelium at the tip, columnar cell epithelium elsewhere. The sudden change from one to the other is very clearly seen.

(c) "Stump of polypus removed in Out-patients' Department. This shows malignant growth, having the characteristics of squamous-celled carcinoma. The growth does not appear to extend into the walls of the cervix. Fallopian tubes showed nothing abnormal.

(d) "The omental mass has the structure of a polymorphic cell sarcoma. The cells vary very much in shape—rounded, irregular and fusiform varieties are present. Some parts are closely set; there are others in which the arrangement is loose, with much vacuolation, probably due to fat. Some cells have branching processes. Thick-walled blood-vessels are seen traversing the tumour."

Case of Hæmatoma of the Vulva following Labour.

By J. BARRIS, F.R.C.S., and H. J. MCCURRICH.

(From the Maternity Department of St. Bartholomew's Hospital.)

ALTHOUGH small hæmatomata of the vulva following labour have been frequently recorded, this case is of special interest because of the unusual size of the swelling.

E. M. (Case No. 3,181), admitted February 3, 1921, complaining of a swelling of the vulva subsequent to labour. Patient was a married woman, aged 35, who had already borne three children without any known complications. No history of miscarriages or of past illnesses. Labour had occurred before her admission to hospital: membranes had ruptured spontaneously at 1 p.m., February 2, and at 1 a.m., February 3, strong pains had commenced. Her doctor stated that on arrival he had found the fetal head on the perinæum and as far as he could judge the second stage was short. The child was born at 5 a.m. Forceps not used; perinæum not torn; placenta delivered without difficulty soon after birth of child. No abnormal amount of hæmorrhage. The child, a girl, was alive and weighed 7 lb. 7 oz.

One and a half hours after labour was completed patient experienced great soreness between thighs and shortly afterwards bled slightly. Her doctor was summoned and found a large hæmatoma of the vulva. Later in the day she complained of so much pain that he gave her an injection of morphia and sent her to hospital.

At 8 p.m. on same day soon after her admission she looked blanched, her temperature was 97.4° F. and pulse-rate 96 per minute. Height of uterus was

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4 Barris and McCurrich: Case of *Hæmatoma of the Vulva*

5 in. from pubes; no abnormal swelling could be detected within abdomen. Labium majus and minus as well as perinæum on left side widely distended by a purple shiny oval swelling, extending from anterior extremity of left labium minus to anterior margin of anus. Left labium minus stretched over centre of swelling as an œdematous band. Perinæum also very œdematous. In general appearance the swelling resembled the foetal surface of a large placenta in process of extrusion. The antero-posterior diameter measured 9 in. and the transverse $4\frac{1}{2}$ in. In consistence it was tensely elastic, but there was no marked tenderness to touch except at one point on left of perinæum. The swelling bulged so much across the mid-line that the orifices of the vagina and urethra were completely hidden. One finger only could be introduced into the vagina with difficulty and the bulging extended up the left vaginal wall as far as the finger could reach. There was a slight amount of blood-stained fluid, which was escaping from the vagina, but the surface of the swelling appeared intact. No varicose veins were detected around the vulva, but there was a minute superficial laceration of the perinæum in the mid-line. The œdema did not extend into the left thigh or buttock.

It was decided to evacuate the swelling, partly on account of the severity of the pain and also because, owing to the general condition of the patient, it was not possible to be certain that bleeding had ceased.

Operation performed under gas and oxygen anæsthesia, patient being in lithotomy position. Incision 1 in. long was made in antero-posterior direction just internal to labium minus; 18 oz. of blood clot evacuated. Inner wall was extremely friable and tore at edge of incision as if it were a piece of wet blotting-paper. The cavity resulting from evacuation of clot was explored with finger, which could be passed up as high as base of broad ligament but not beyond; uterine artery could not be palpated. Finger could also be passed backwards by side of rectum and internal pudic artery was felt pulsating in Allcock's canal. No bleeding points seen and the surface oozing not great. The cavity was irrigated with saline at temperature of 118° F., which stopped nearly all oozing and it was then lightly packed with sterile gauze which was retained in position by single silk-worm gut suture.

While patient was under anæsthetic, bimanual and rectal examination made and cervix and vaginal walls examined by speculum. Cervix seen to be lacerated on each side. No induration could be felt in either broad ligament. A finger passed into cervix could not detect any uterine laceration; some protruding blood clot and membrane removed from uterus which was irrigated with hot lysol solution; 1 c.c. of pituitrin then given hypodermically. Rectum found undamaged.

Gauze pack removed thirty-six hours later. Catheter required until pack was removed, after which bladder was evacuated normally. Prophylactic doses of sensitized vaccine of type No. 1 *Streptococcus pyogenes* given hypodermically, 125 million immediately after the operation and 250 million on following day: no reaction. Wound irrigated three times daily for ten days with a weak solution of biniodide of mercury and surface washed over after urine or fæces had been voided. No suppuration; wound healed by granulation within twelve days. Supply of breast milk unaffected and the child entirely breast fed. Patient got up on fourteenth day and discharged from hospital on twentieth day.

All that remained of the lesion was a small puckered scar on inner wall of vagina just superior to the labium minus. Three months later patient came up for observation; scar had caused no dyspareunia nor inconvenience.

Case of Prolonged Gestation.

By R. A. HENDRY, M.D.

THE interest in cases of post-maturity aroused by a recent law-suit before the Lord Chancellor is my reason for describing the case of Mrs. B., whom I saw on November 20, 1917, when she attended for the first time at an Ante-natal Clinic in order to learn whether she was pregnant. She was aged 41, and had had four children, the last eighteen years ago, and three abortions scattered between the live births. Her husband went to Australia in 1913, four years before. He returned with his regiment, in which he was a sergeant, and while in Liverpool, his native city, on a few days' leave, met his son, went home with him and there spent the night of May 14-15, 1917, and then rejoined his unit.

On November 20, six months later, Mrs. B. came to the clinic and said that the menses had been regular till April 12 and absent since then except for a flooding in June—the exact date not remembered—and that she was getting stouter and could feel movements.

On abdominal and vaginal examination I noted that the uterus was six months in size, i.e., corresponding with the presumptive period of gestation, and that the foetal heart sounds were not heard.

A month later, December 18, 1917, I again examined her and noted the uterus as of seven months' size, foetal movements present, foetal heart not heard. She attended the clinic on January 15 and February 19, 1918, but was not examined again till March, 19, 1918, a month overdue, when I noted the uterus as full term, foetal heart heard, movements felt, and the head in the pelvic cavity.

On April 2, abdominal examination as on March 19; cervical canal not taken up and internal os admitted a finger.

Patient was delivered of a daughter on April 8, 1918, three hundred and twenty-eight days after coitus and three hundred and sixty-one days after the last menstruation.

The midwife informed me that the labour was quite straightforward, that there was no excess of liquor amnii, and that the baby was of average size; on the seventeenth day it was weighed at an infant welfare centre, and its weight reported to be 7 lb. 4 oz.—certainly not over the average.

Criticism may be directed:—

(1) To errors in observations; (2) to causes of discrepancy between the size of the uterus and its gestation history; (3) to lack of corroboration of observation.

(1) A two-hundred-and-eighty-days' gestation, terminating on April 8, 1918, begins on July 2, 1917; November 20 would be twenty weeks two days later; December 18 twenty-four weeks two days, i.e., on November 20 a twenty-weeks' uterus must have been mistaken for a twenty-seven weeks' uterus, and a month later a twenty-four weeks' uterus for one of thirty-one weeks. A double error of such magnitude is, I think, extremely improbable.

(2) Hydramnios and twins may both cause oversize; there was only one baby and, at the labour, no hydramnios. The requisite amount of hydramnios

6 Nyulasy : *Endometritis Decidualis Polyposa vel Tuberosa*

in the pregnancy would probably be recognized if present, and moreover would probably not be associated with such a normal pregnancy.

No irregularity of the uterine tumour suggestive of fibroid was ever found, and there was only one tumour.

(3) In the circumstances lack of corroboration is practically inevitable; it is only fair to say that a trained nurse and pupil midwives are present at the clinics, and that the cases are demonstrated to them as a routine.

Endometritis Decidualis Polyposa vel Tuberosa.

By FRANK NYULASY, M.D.

This paper, which was read and discussed at the meeting of the Section on October 6, is published in the *Medical Press and Circular*, October 19, 1921, p. 315.

Section of Obstetrics and Gynaecology.

President—Professor HENRY BRIGGS, F.R.C.S.

Specimen consisting of a Uterus showing a Double Ruptured Interstitial Ectopic Gestation.

By A. E. MORTIMER WOOLF.

THE patient from whom this specimen was removed was admitted to hospital on April 19, 1920. She had been married for seven years, and had one child 6 years old; no miscarriages, no pregnancies, since birth of child. There had been no previous illnesses.

Amenorrhœa had existed since February 13, 1920.

On Wednesday, April 14 (i.e., five days previous to admission), at 8.30 a.m., while dressing, she was seized with sudden sharp pain in the left side of the abdomen which caused her to double up. This pain lasted all day, but passed off at about 7 p.m. On the Thursday, Friday and Saturday, she felt quite well except for some slight weakness. On Sunday morning, the 18th, about 10.30 she was again seized with acute abdominal pain which was general throughout the abdomen. She fainted. The pain was continuous until she was admitted to hospital on the following afternoon (Monday, the 19th) about 3 o'clock. Vomiting was frequent during the Sunday and Monday.

On examination the lower half of the abdomen was found distended. The abdomen was fairly rigid all over. Tenderness was present throughout, but was more marked over the epigastrium. Dullness was present in both flanks. She looked very pale. The pulse was thready in character, 144, temperature 99° F. *Per vaginam* nothing abnormal could be detected. She was constantly retching.

Diagnosis: Ruptured ectopic gestation.

Operation Monday evening, at 8.30: Median incision below umbilicus. When the peritoneal cavity was opened, a large quantity of dark-coloured blood escaped. The right tube was seized and clamped. In the intramural portion of both tubes there were gestation sacs which had ruptured. A large hæmatocele was present on the left side. The pelvis contained numerous blood clots.

The patient was in a precarious condition, and the most rapid operative procedure was obviously essential. I considered that excision of both tubes by wedge-shaped incisions, and subsequent suture of uterine walls, would have involved too big a risk owing to (1) further hæmorrhage, and (2) the time which such a procedure would have occupied. I therefore performed a sub-total hysterectomy.

Further progress: The patient stood the operation well, and made an uneventful recovery.

Description of Specimen.—A coronal section of the uterus which was excised. In the wall there is on either side, at the upper angle, a mass of blood about the size of a cherry, which represents an interstitial pregnancy. In the clot

on the left hand side may be recognized one or two uncoloured foci, which microscopic examination shows to consist of chorionic villi. The sac on the right hand side has ruptured, and from it there projects an irregular eminence of coagulum. The uterine mucosa has undergone a simultaneous thickening, and is hyperæmic. The muscular wall is hypertrophied.

An American Gold Button Contraceptor.

Shown by W. J. O'DONOVAN, M.D.

(Introduced by Dr. LUKER.)

I REMOVED this intra-uterine contraceptive device from a lady aged 26 after it had been in place twelve months and she had forgotten its existence. It consists of a gold button-like small plate moulded conversely to fit over the front of the cervix, with a central rod of closely-wound gold wire which fitted like a stem into the cervical canal. At the end of this stem two arms of wire diverge at an angle of 30° and in position reached in the uterus to the internal ostia of the Fallopian tubes. The patient had suffered from no floodings nor from dysmenorrhœa and was sent to me on account of pruritus vulvæ due to a slight, and slightly offensive, vaginal discharge.

Cystic Adenomyoma of Uterus.

By G. CRANSTOUN.

I MUCH regret that, in introducing this case to your notice, I have no specimens to show you, as unfortunately this was destroyed before the nature of the tumour was discovered, and I have only two microscopic sections, one taken from the body of the uterus and one from the wall of the cyst, to produce as evidence of my statements. But as this type of tumour, is, I believe, of very rare occurrence, I thought it right that the case should be recorded. Cullen in his monograph mentions cystic adenomyomata of small size, but none approaching in the least the size of this cyst, which occupied the whole abdomen. The fluid in the larger cavity and in the smaller spaces was similar to retained menstrual blood.

The patient was a single woman, aged 45, with a two-years' history of gradual enlargement of the abdomen and increasing dyspnœa—the only unusual point about the history being that neither her friends nor her relations had suggested pregnancy as the cause of her condition. Menstruation had always been regular and normal up till August, 1920 (i.e., eight months before admission to hospital), when it ceased.

Patient was a thin woman, rather anæmic, with a cheerful placid expression, and despite the enormous distension of the abdomen showed very little sign of distress while lying in bed. The abdomen was immense, and the percussion note was dull everywhere, even in the flanks. A very marked fluid thrill was obtained. No vaginal examination was made, and apart from signs of bronchitis which were present on admission but cleared up in a few days, nothing else abnormal was discovered.

A diagnosis of unilocular broad-ligament cyst was made.

Operation by Dr. Fairbairn, April 13, 1921: As soon as the abdomen was opened the cyst-wall was exposed to view; it was very dark in colour and adherent to the parietal peritoneum, omentum and gut by its anterior surface. It was not tough and elastic like that of an ovarian cyst, but inelastic and tore readily, efforts to separate it soon resulting in rupture and the escape of large quantities of brown muddy-looking fluid, whilst in the most dependent parts of the cyst was found a soft brown spongy material which proved to be old altered blood-clot. Owing to the difficulty of separating the adhesions it was decided to seek the origin of the cyst below and separate it upwards. On exploring the pelvis the uterus was found to be irregularly enlarged and contained what appeared to be several fibro-myomata, whilst the structures in the neighbourhood were adherent and matted into an almost indistinguishable mass. Panhysterectomy was performed commencing with the division of the round ligaments and cervix, and the body of the uterus and the cyst-wall were gradually separated and stripped off the abdominal viscera from below with considerably greater ease than had attended the earlier efforts. The abdominal cavity was washed out with hot saline and the operation concluded in the usual manner.

Description of mass removed: The uterus was enlarged by the presence in its wall of three fibromyoma-like masses each about the size of a tangerine orange, each mass communicating with the others. The largest of these tumours was situate at the upper left corner of the uterus, apparently originating at the junction of the left tube and left uterine cornu. It appeared to be interstitial and composed of uterine tissue but it projected about an inch above the upper surface of the uterus and presented an ovoid flattened surface to the abdomen, forming a kind of plateau: this surface was covered with the soft spongy material found to be degenerated blood-clot. The wall of the cyst was continuous with the edge of this plateau and the point at which the section was cut (about 3 in. from the edge); it was composed of uterine tissue containing many spaces filled with the same degenerated blood-clot.

The explanation apparently suggested is that the degeneration and distension of the tumour had started centrally and, owing to the fact that least resistance was encountered on the abdominal side, dilatation had taken place at the expense of the upper part of the tumour which had gradually stretched from the wall of the immense cyst. It contained between three and four gallons of dark-brown muddy fluid and a large quantity of the same brown, soft degenerated material as covered the flattened plateau of the tumour. All parts of the tumour contained these dilated spaces and all spaces communicated with the cyst cavity.

The right tube and ovary were normal and the left tube and ovary were normal except that the tube could not be traced into the cavity of the uterus and appeared to be lost in the wall of the cyst.

After a good deal of trouble with vomiting and distension the patient made a slow but uninterrupted recovery and left hospital on May 20. She is at present in excellent health and has returned to her situation.

Section of Obstetrics and Gynæcology.

President—Professor HENRY BRIGGS, F.R.C.S.

Case of Squamous-celled Carcinoma of the Cervix Uteri associated with a Nodule of Spheroidal-celled Carcinoma in the Body.

By JOHN ELLISON, F.R.C.S.Ed.

MISS A. T., aged 58; nullipara; menopause at 36. In 1916 she first noticed that the right breast was drawn in. During 1918 and 1919 she was treated for a lump in the right breast by means of applications, and during this time she had some intermittent blood-stained vaginal discharge which she did not mention to anyone. In April, 1920, she came under the notice of Mr. Back with an obvious carcinoma of the breast, which he removed by the radical operation. From this she made an uninterrupted recovery. While in hospital I saw her in consultation, and found a carcinoma of the cervix which I considered just operable. In June I performed a radical operation for its removal; there were no enlarged glands in the pelvis and no metastases in the liver. Recovery was uninterrupted except for slight superficial suppuration. In July the vaginal vault was smooth, and there was only a little thickening above it on the left side, which was mobile and not tender. During August she had a course of radium applied to the pelvis. Though no definite mass could be felt her general appearance made one suspect that there were secondary deposits somewhere. Headaches became more frequent and severe, and in September the left internal rectus became paralysed, causing diplopia. She died in a comatose condition at the end of the month. Up to the time of her death there were no signs of any recurrence in either the breast or the pelvis.

The specimen shows a large carcinomatous ulcer of the cervix, and also two nodules of a different nature in the body. Microscopically the cervical growth is a typical squamous-celled carcinoma. The larger of the two nodules also consists of spreading carcinoma of the squamous type. The smaller of the two nodules however, is composed of closely packed cells of a spheroidal type with very little intercellular tissue. This nodule is just separated from the advancing squamous epithelioma by a band of tissue containing large blood-vessels.

The microscopical appearance of this nodule and the clinical picture presented by the patient appear to warrant the opinion that this nodule is a secondary deposit from the carcinoma of the breast.

Mr. GORDON LEY said he had seen Mr. Ellison's specimen some months ago, soon after its removal, and had then thought that the small nodule in the body of the uterus was probably secondary to the growth in the breast. The growth in the cervix was squamous and polygonal-celled, and showed horn formation. That in the small nodule was spheroidal-celled, and the cells were closely packed in small processes separated by strands of intervening stroma. The further history of the patient was very suggestive

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of a death due to aplastic anæmia. He had never seen a case of carcinoma of the cervix uteri in which there was a generalized carcinomatosis such as appeared to have been present in this case.

An Unusual Case of Ectopic Gestation.

By JOHN ELLISON, F.R.C.S.Ed.

L. D., aged 24, was admitted to St. George's Hospital in 1917, under the care of Dr. Stabb, with the following history: She had married four weeks previously, and her last period was a fortnight before this. For the last week there had been some tenderness of the breasts and some morning sickness. Five days before admission, while turning over in bed she had sudden pain in the right iliac fossa, after which she felt faint. There was no further pain till the morning of admission, when she had a similar but more acute attack, after which she fainted twice. The pain continued and grew worse; she became pale and had frequency of micturition.

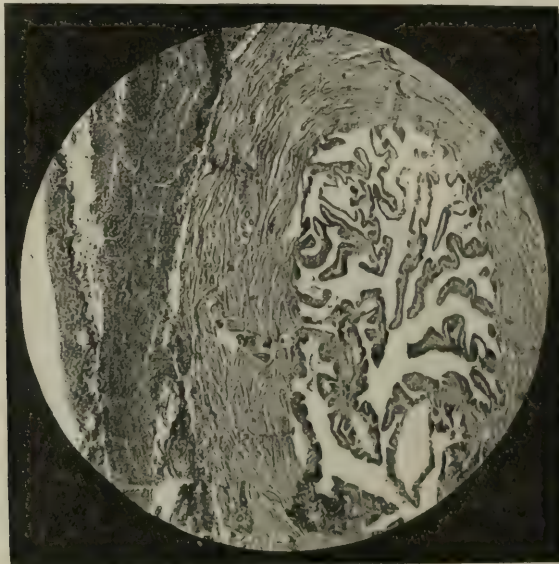
On admission she was pale and restless; temperature 100, pulse 68, soft and compressible; the breasts showed early signs of pregnancy. *Per abdomen*: There was a visible mass in the right iliac fossa which was tender and dull, and the tissues of the inguinal canal appeared to be infiltrated as though some of the contents of the right iliac fossa were trying to escape by that route. There was no sign of free fluid in the peritoneal cavity. *Per vaginam*: The cervix was high up, softened and to the left, the external os was closed, the body of the uterus was a little enlarged and pushed to the left by a large cystic swelling in the right and anterior fornices. Definite fluctuation could be obtained in this mass bimanually. There was a slight blood-stained discharge on the examining finger. There was no change in the signs after passing a catheter.

The abdomen was opened in the middle line; the tissues of the abdominal wall were infiltrated with blood. There was no free blood in the peritoneal cavity. The uterus was somewhat enlarged and lying over to the left. Both appendages were normal. The peritoneum covering the right side of the pelvis and the right iliac fossa was raised in a large mound extending from the pelvis to the right kidney; the right tube, unaltered, was lying on top of this, and was connected with it by some $2\frac{1}{2}$ in. of unobliterated mesosalpinx. On the tube being held up a blood-stained cone could be seen with its apex at the middle of its isthmal portion and its base some $1\frac{1}{2}$ in. in length reaching the above mentioned mound. The right tube was removed and the posterior layer of the peritoneum was incised and a large quantity of blood clot was removed from the retroperitoneal space, exposing the large vessels. A steady stream of blood was noticed coming from an opening in one of these veins, and the hole was closed by a catgut ligature. A tube was inserted into the retroperitoneal space which was also packed with gauze. Two pints of saline were left in the peritoneal cavity, and the abdominal wound closed in layers with continuous catgut reinforced with silkworm-gut. On the third day the plug was removed under gas. The retroperitoneal space began to fill up again and on the ninth day, as the temperature was rising, it was found necessary to reopen the wound and remove more blood-clot. The tube was replaced, and from that time the patient made an uninterrupted recovery. When the patient was discharged the pelvis appeared quite normal, but *per abdomen* some thickening could still

be felt in the right iliac fossa. The first slide shown (*see figure*) is of the right tube, the section being taken at the point where the blood cone touched it. The lumen of the tube and of its muscle wall is quite normal, but there is blood surrounding it. Sections of the rest of the tube were normal. The second slide is of the blood-clot from the retroperitoneal space, and in it there are definite chorionic villi.

An ovum was therefore developing in the retroperitoneal space, and one of the chorionic villi had eroded into one of the pelvic veins causing the acute symptoms.

As the tube is apparently normal throughout it seems probable that the primary implantation of the ovum occurred on the pelvic peritoneum, that it gradually embedded itself into the retroperitoneal space where it continued to



Section cut at the point where the blood-cone in the mesosalpinx touched the right tube. Showing a healthy tube with blood-clot between its muscular wall and the peritoneum.

grow till it eroded first a small vessel (resulting in the first attack of pain and faintness five days before admission); and later a larger vessel with the sequel of urgent symptoms.

In 1920—three years after the operation—the patient was delivered of a full term infant, and when she was examined a few months later the abdomen and pelvis appeared quite normal.

Dr. ARTHUR GILES remarked that this was a most rare as well as most interesting case, and he had never heard of one like it. He asked Mr. Ellison whether serial sections had been made of the tube in the region where the cone of blood came up to it from the broad ligament; although it could not be said that an ovum implanted on the peritoneum could not make its way through into the tissues of the broad ligament, it did not appear to be the most likely thing to happen. *A priori* the likely thing was for the ovum to erode the tube, whence it might pass into the mesosalpinx and give no indications until it opened up a vein in that position. The hole might easily be so small as to escape notice.

Carcinoma of the Cervix in a Virgin, aged 24.

By SIDNEY FORSDIKE, F.R.C.S.

THE patient, Miss E. C., was aged 24 years 10 months when she came under my observation.

History: Catamenia 3/28 had been regular. In February, 1920, when 23 years 3 months old, she first began to have irregular losses of blood apart from the period. Her doctor writes: "I saw her at the end of February, 1920, when she complained of irregular loss. Being so young I took it that it was one of the usual irregularities one gets, and told her if it continued that she ought to go up to one of the hospitals for treatment, which might mean curetting. I did not see her again until I sent her up to you. I have never heard of carcinoma uteri in one so young." From February, 1920, to December, 1920, the loss became more pronounced and alarmed her mother, who consulted her own doctor about it. In December, 1920, she had a flooding, and was taken to see the doctor, who gave her some pills and prescribed douching. From now onwards she had daily losses and a constant brown discharge, with backache and attacks of acute pain in the "privates" which only lasted a few minutes at a time. No pain at night nor interference with sleep. In January, 1921, she was advised to seek hospital treatment, but was only urged to come when much worse, in August, 1921. On November 6, 1921, she had a severe flooding which confined her to bed for several days, and a few days later she came to hospital. November 10: Looks poorly. Abdomen *nil*. *Per vaginam*: Hymen present, ostium vaginae small. *Per rectum*: Cervix enlarged and hard, with indurated tissue around it. She was now bleeding freely from the vagina and further examination was postponed. November 18: Examination under ether. The cervix was represented by a growth which was easily removed by light curettage, leaving a large ulcerated cavity, which easily admitted the largest Hegar's dilator. The growth involved the vaginal wall all round, the latter being quite friable. The bases of both broad ligaments are involved and the uterus is fixed and does not descend on traction. The utero-sacral ligaments are slightly thickened. A piece was removed for section. The patient is being treated by radium.

Microscopic section shows a columnar-celled carcinoma.

**REMARKS ON THE OCCURRENCE OF CARCINOMA OF THE UTERUS
UNDER THE AGE OF 25.**

Carcinoma uteri in nulliparæ below the age of 25 is exceedingly rare. Lewers and Cullen do not show any cases in their tables. Cragin (*American Journal of Obstetrics*, 1913, lxvii, p. 144) reports a case in a girl aged 18, who had symptoms for several months before the diagnosis was established. De Rouville (*Bulletin de la Société d'Obstétrique et de Gynécologie de Paris*, 1912, xv, p. 388) reports a case in a girl aged 18, which was operable after many months' symptomatic treatment. In the reported cases of cancer at this age, the diagnosis has been made when the disease is well advanced, and this appears to be due to the want of examination of the patient. The examination of young girls as a routine for minor disturbances is certainly not necessary, but when the symptoms persist despite treatment, then examination should be

the rule, under anæsthesia if necessary. Carcinoma uteri in multiparæ appears to be coming under observation at an earlier stage of the disease than formerly, a fact which may be attributed to the recent publicity given of the symptoms of the disease, and it would be wise to emphasize the truth that cancer occurs also in young girls.

Eclampsia and its Incidence.

By R. H. PARAMORE, F.R.C.S.Eng. (Rugby).

(ABSTRACT.)

[This Paper is printed *in extenso* in the *Lancet*, December 3, 1921, p. 1147.]

ECLAMPSIA is marked by a profound toxæmia. It is admitted that the toxæmia causes the eclampsia; the question is, what causes the toxæmia? The author rejects the view that the toxæmia results from placental changes; and, indeed, that a strange toxin arises from other parts (breast, ovary, thyroid, intestine, fœtus). He believes the maternal visceral lesions, which obviously precede the eclampsia, are primary to the toxæmia and explain it. Necrosis of the liver and kidneys must cause some symptoms; and in the author's opinion suffice to explain the toxæmia which exists. This view is supported by the rôle food (especially protein food) plays in the rise of the toxæmia; by the considerable increase of non-protein nitrogen in the blood which Killian and Sherwin have recently demonstrated in these cases; and by the urinary changes, conspicuous amongst which is the large proportion of undetermined nitrogen.

The author believes eclampsia is simply a uræmia, distinguishable from other acute uræmias only in the method of its production. The necrosis of the maternal kidney and liver is an ischæmic necrosis—a necrosis due to a shutting off of the blood supply. The cessation of the blood flow is determined by an occlusion, not of the supplying arterioles, but of the capillaries into which these arterioles lead; and the occlusion is produced, not by a thrombosis, but by a pressure. The thrombosis is secondary to the necrosis; and the pressure is the raised intra-abdominal pressure which in certain cases of pregnancy is exaggerated, and to which the rises induced by activity and especially by labour are superadded.

The incidence of eclampsia supports this conception. Thus, eclampsia is most common in primigravidæ—that is, in women in whom the abdominal wall has not suffered by having been previously stretched; and in whom, therefore, the intra-abdominal pressure is likely to be higher than in pregnant multiparæ. Eclampsia is commoner in strong muscular women than in fragile or diseased (e.g., phthisical) subjects—because the better development of the musculature in strong women causes their intra-abdominal pressure to be higher than in fragile women at corresponding periods of pregnancy. The unmarried pregnant woman is more prone to eclampsia than the married, because she constricts her abdomen by corsets, endeavouring to prevent the abdominal distension which the woman in wedlock has no motive to hide. These three types of cases form a class by themselves.

Eclampsia, however, also occurs in other kinds of cases—at the birth of twins, in hydramnios, concealed accidental hæmorrhage, and in hydatidiform mole, and these form a second class. In all these cases the contents of the uterus are distinctly different, and this alone indicates that the cause of

eclampsia does not reside within the uterus. But, in spite of this dissimilarity, the first three types at least have one conspicuous feature in common—that the uterus is much larger than it should be for the period of the pregnancy. This must necessarily affect the intra-abdominal pressure and produce higher pressures than occur in the opposite condition, other things being equal. The condition of affairs in the case of hydatidiform mole, when the pre-eclamptic toxæmia arises, in this respect is uncertain, and is left for others to discuss. But in the other three types of case it is plain that eclampsia is associated with a presumptive rise of intra-abdominal pressure.

And labour, which produces great rises of this pressure, also plays a part in the causation of eclampsia. Post-partum eclampsia is to be attributed to the effects of labour. Labour increases the albuminuria of pregnancy, and is often the first cause of its appearance, and in this respect labour is comparable with the exercise of athletes. It is clear that labour often produces changes in the kidney; in the same way, at times, it may set up similar changes—perhaps even necrosis—in the liver.

In support of this, there is first to be remembered the evidence of *accouchement forcé* in eclampsia. Before this treatment was superseded by Cæsarean section it was believed by some to have a pernicious effect. The reason was that it caused too much straining. Prolonged labour, e.g., when obstructed, in presumably healthy women, acts similarly. The history of vagino-fixation, should the patient become pregnant, affords such evidence. Macan relates a series of disasters emanating from the German school after the performance of this operation. Eclampsia occurred certainly twice, and I believe three times. The patients were in labour many hours.

The question of the effect of straining on the blood flow through the viscera is not discussed. The author believes that the blood chooses the easiest path, and that not all the organs, or, indeed, all parts of the same organ, are supplied in quite the same way. The great mass of blood reaching the liver has first to pass through the gastric and intestinal capillary network, whilst that reaching the convoluted tubules of the kidneys must first traverse the glomeruli. If, in consequence of an increase of intra-abdominal pressure, an obstruction in these secondary capillary areas should occur, it could surprise no one that the tissues concerned should suffer from an inanition or that an ischæmic necrosis should result. The question is whether a study of the intra-abdominal pressure and of the incidence of eclampsia warrants the acceptance of the idea that such actually happens. The author believes that the evidence more than suffices that such a conclusion is more than justified.

DISCUSSION.

Dr. ARTHUR GILES asked whether Mr. Paramore had considered the question of the production of lesions of the liver and kidney as the result of increase of intra-abdominal pressure from tumours, apart from pregnancy. He had in mind such a case as that of a girl, aged 17, from whom he removed an ovarian cyst weighing 32 lb.; and he recalled also a case in which he removed an ovarian cyst weighing 30 lb. from a woman in labour. In such conditions there was presumably a great increase of intra-abdominal pressure; might one not expect, on Mr. Paramore's theory, that the presence of large ovarian cysts would lead to such changes in the liver and kidney as might produce, not eclampsia, of course, since this was only found in pregnancy, but at least such a condition as uræmia, with its attendant convulsions? In former days, when ovarian tumours were not operated upon so early, these tumours might reach a size of 40 or 50 lb., or even more; yet one did not hear of these patients being affected by toxæmic conditions analogous to those associated with eclampsia.

Mr. GORDON LEY could not agree with Mr. Paramore's hypothesis of the cause of eclampsia. Dr. Giles had already pointed out that if Mr. Paramore were correct in his theory, eclampsia should occur with large abdominal tumours. He (Mr. Gordon Ley) had seen many cases in which abdominal tumours had grown much more rapidly than the pregnant uterus grew. Mr. Paramore said that the lesions in the liver were produced by a pressure anæmia. If this were so, the lesions found in eclampsia would be found not around the portal radicles which were reached first by the hepatic circulation, but around the radicles of the subglobular veins, which were reached last. Further, how could Mr. Paramore explain the lesions in the kidney being confined largely to the convoluted tubules of the ascending limb of the loop of Henle? Why did the glomeruli of the descending limb of the loop of Henle, which were also in the cortex, escape? The lesions met with in eclampsia were similar to the lesions produced by known poisons and totally dissimilar to the lesions produced by an anæmia. Mr. Paramore had said that the onset of toxæmia in concealed accidental hæmorrhage was due to the increased intra-abdominal pressure produced by the intra-uterine hæmorrhage. Many cases were recorded, and he (Mr. Ley) himself had seen many in which the toxæmia preceded the onset of the hæmorrhage. In fact, he was strongly of opinion that the toxæmia always preceded the hæmorrhage, and was the cause of it. Mr. Paramore had further said that eclampsia was more common in unmarried women, and attributed this to their tight lacing. Surely he had failed to realize that the majority of unmarried women who were pregnant were primigravide. Taking this into consideration, he (Mr. Ley) would very much like to see figures showing that the unmarried primigravida was more liable to toxæmia of pregnancy than the married primigravida.

Mr. MALCOLM said that Mr. Paramore had brought forward a definite, easily understood explanation of an admittedly difficult question, and had argued his point with much ingenuity, but when he argued that the woman's body, "with its ancestral memories," should, at this stage of our history, be fully equal to bearing a child without any dangerous chemical disturbances, he overlooked the fact that the same argument was applicable to the power of the body to avoid the effects of a too great pressure upon the liver and kidneys. Again, it was stated in the paper that the blood in its course chose the easiest path. This ignored altogether the control of the vessels by the sympathetic nervous system, which should bring about a contraction of all the vessels of the body, rather than allow a pressure necrosis of the important organs affected in eclampsia. Definite signs of such a general vascular contraction should develop before a necrosis took place. He (Mr. Malcolm) agreed with those who asserted that an ovarian tumour might grow even more quickly than a pregnant uterus, and in cases of acute intestinal obstruction an enormous increase of abdominal pressure might be created in forty-eight hours, but there was no eclampsia.

Section of Obstetrics and Gynæcology.

President—Professor HENRY BRIGGS, F.R.C.S.

Tubal Lithopædion.

By BECKWITH WHITEHOUSE, M.S.

THE specimen was removed from a 2-para, aged 45, with the following history: Fifteen years previously she had had what she described as a very "bad miscarriage." No operation was performed, but she was acutely ill at the time, and was in bed for about three months afterwards. There had been no subsequent pregnancy, and the catamenia, although profuse, had been quite regular. On several occasions severe attacks of cystitis occurred, and ten years after the so-called miscarriage the patient was referred to a general surgeon and a diagnosis of renal calculus made. The woman was extremely obese and no operation was performed. Five years later inflammation of the bladder again occurred, and the patient passed a considerable quantity of small phosphatic concretions. On this occasion the patient was referred to the writer, and cystoscopy was performed. The fundus of the bladder was seen to be inverted, and the vesical mucosa acutely inflamed and coated with phosphates. Very little could be ascertained on bimanual examination even under an anæsthetic, owing to the patient's obesity. The lower pole of a hard body however, was just palpable in the right anterior quadrant of the pelvis. Laparotomy was performed and a lithopædion removed from the region of the right uterine appendages, some difficulty being experienced owing to the density of adhesions and general matting of the surrounding intestines and omentum. The uterine body was the seat of several interstitial fibroids, and subtotal hysterectomy was performed. At the time of operation the specimen was taken to be a detached calcified fibroid, and it was not until afterwards that its true nature was recognized. It appeared to be a typical lithopædion corresponding to a gestation of about the third month of pregnancy. The cranial bones, vertebrae, scapula, pelvic girdle, and certain digits were all recognizable on the surface. The patient recovered satisfactorily from the operation, but five months later was readmitted to the nursing home with double pyonephrosis, to which condition she succumbed.

Salpingotomy versus Salpingectomy in the Treatment of Tubal Gestation.

By BECKWITH WHITEHOUSE, M.S.

IN 1914, at a combined meeting of the North of England, Edinburgh, and Midland Obstetrical and Gynæcological Societies, a paper was read by my colleague, Professor Thomas Wilson, and myself, in which we reported the

results of an investigation into the pathology of tubal gestation, based upon the dissection of thirty fresh and unhardened specimens of tubal mole, abortion, and rupture. The paper was never published for various reasons, the chief being the incidence of the European war. As the present communication is somewhat in the nature of a corollary to that paper, it is necessary to refer briefly to the chief points resulting from the work.

We found, on dissecting under fluid a freshly removed Fallopian tube containing a tubal mole, that it was possible in all cases to trace the lumen of the tube around and beyond the mole; in fact, as far as it was required to carry the dissection. Further, the mole itself was always attached by a narrow base or pedicle to some part of the tubal mucosa. This area never exceeded $\frac{1}{2}$ in. in diameter, and we regarded it as the site of original implantation of the ovum (fig. 1). In dissecting such a pregnant tube it was, moreover, always



FIG. 1.—Showing (a) the site of implantation of an ovum on the floor of the tube; (b) the narrow "pedicle" of the tubal mole at the point of implantation, the same being the proximal pole; (c) distal pole of the gestation.

easy to elevate the mucosa from the surface of the gestation until this spot was reached. Here it was firmly adherent, and, if force was exerted, either fracture of the clot, or laceration of the tubal wall occurred. The former was by far the more common.

We also found it possible in the majority of cases to trace the tubal mucosa as a collar or sheath over the surface of the attached base of the mole for a distance of a few millimetres. This was verified by sagittal and coronal sections taken through the attached base, and it proved that rupture of the ovum had occurred from its embedded position in the tubal wall, through the mucosa. The shape of a dissected mole, if of any size, was ovoid or finger-like, the contour being undoubtedly the result of pressure of the walls upon

the coagulated maternal blood, whilst the latter was still of plastic consistence. That the wall of the tube does exert considerable pressure upon a mole was shown by the longitudinal furrows upon the surface of the clot, corresponding to the rugæ of the tubal mucosa (fig. 2). In the case of small moles, in which the extravasated blood was small in amount, the shape tended to be more globoid, as indicated in fig. 3. The surface of all the dissected moles was quite devoid of epithelium, with the exception of the narrow attached base shown in fig. 1. Tubal abortion appeared to be produced by peristaltic action of the Fallopian tube, sufficient to sever the attachment of a mole to the mucosa.



FIG. 2.—Tubal mole dissected to show finger-like shape, free distal pole (*a*), attached proximal pole (*c*), and furrows (*b*) on surface produced by pressure of longitudinal mucosal folds of tube.

We concluded from our investigations that sudden and complete rupture and detachment of the ovum into the lumen of the tube, and its *immediate* abortion rarely, if ever, take place. The original separation is only partial, the mucosal laceration being followed by extravasation of blood between chorion and tube wall, a mole resulting; this is expelled at a later date by rupture of the narrow attachment, initiated by peristaltic action of the tube, and probably stimulated by the presence of the foreign body. When abortion was complete,

we found that if the recently pregnant tube was dissected at once, it was still possible to ascertain the site of original implantation of the ovum. It appeared as a small area about $\frac{1}{4}$ in. in diameter, marked by a clot.

A factor of considerable importance, both in connexion with the shape of a tubal mole, and also in dominating the mode of termination of the gestation, is that of tissue resistance. The anatomical conformation of the Fallopian tube in its distal two-thirds is such that the direction of least resistance is away from the uterus and towards the ostium abdominale. Furthermore, the floor of the tube is supported to a considerable extent by the cellular tissue of the mesosalpinx. Rupture of a gestation situated upon the floor of the tube appears more likely, therefore, to take place into the lumen than into the mesosalpinx. This coincides with our clinical experience, for in all the investigated cases of tubal mole and tubal abortion the site of implantation had been the floor of the tube. Further, in all cases of implantation of the ovum upon

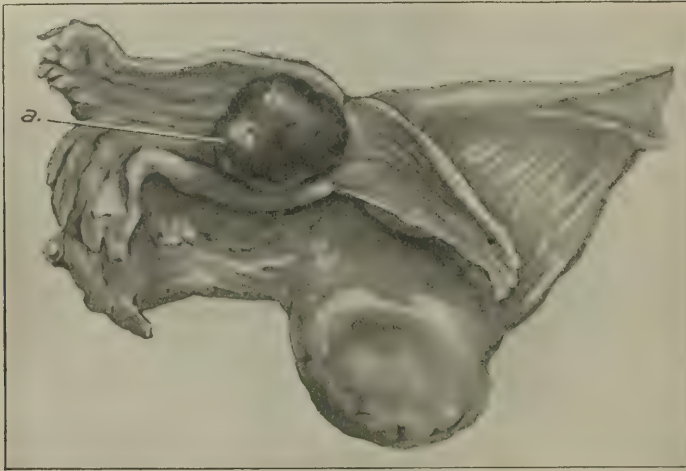


FIG. 3.—Tubal mole of small size, dissected to show globoid appearance and commencing ovoid (a) in the direction of least resistance.

the floor of the tube, with but one exception, tubal mole or abortion occurred. When intratubal rupture occurs, the blood, as may be expected, takes the direction of least resistance and flows towards the ostium. This was proved in our specimens by the fact that the attachment of the mole was *in all cases* at its proximal pole.

Should the ovum reach the narrow isthmic part of the tube and embed itself in this situation, the wall may become so thinned by syncytial erosion that the direction of least resistance is centrifugal through the peritoneal coat, rather than centripetal, and towards the ostium abdominale. In such a case rupture, of course, occurs.

The conclusions, therefore, which we reached in 1914, and which I have confirmed on many subsequent occasions, may be summarized as follows:—

(1) Tubal mole is the direct result of intratubal rupture, and the mole invariably retains a *very narrow basis of attachment* to the tubal wall.

(2) The attached base is usually situated on the floor of the tube, and is always at the proximal end of the mole.

(3) When tubal abortion takes place, the pedicle of a mole is torn through by the peristaltic action of the tube endeavouring to expel the foreign body.

(4) Clots may be expelled from the tube through the ostium abdominale without separation of the mole occurring. Such clots are frequently ovoid in shape.

(5) The conformation of a tubal mole or tubal abortion is the direct outcome of pressure of the tubal wall upon the clot, the impression of the normal rugæ being frequently observed.

(6) The surface of the pedicle of a mole usually presents traces of tubal mucosa. Such epithelium is not present at the distant pole.

(7) The deciding factor as to whether intra- or extra-tubal rupture shall occur, is the direct outcome of the combination of tissue erosion and tissue tension. This is influenced by the site of implantation of the ovum in the ampullary, isthmic, or interstitial portions of the tube.

(8) Evidence either macroscopic or microscopic of pre-existing inflammation of the tubal wall appears to be the exception rather than the rule in cases of tubal mole and abortion.

The two points upon which I would lay special emphasis are, first, the existence of a pedicle or narrow basis of attachment in all cases of mole, and secondly, the ease with which the rest of the mole can, in the case of *fresh* specimens, be detached from the tubal mucosa. These two points suggested to me the possibility of removing a mole from the tube *in situ*, and raised the question as to *whether we are justified in sacrificing the tube on all occasions*.

In a small series of ten cases, therefore, before removing the tube, I performed salpingotomy and extracted the gestation. The mole was excised without difficulty, and hæmorrhage either from the pedicle or the tubal wall was easily controlled by suture. The tube in these ten cases was then removed and examined in detail for evidence of inflammatory change. In all the specimens the tissues appeared to be normal, and the abnormal implantation of the gestation to be purely accidental in origin.

There seemed, therefore, to be quite a good case for salpingotomy, and on five occasions during 1921, I adopted this method in the treatment of tubal gestation. Three of the patients were cases of tubal mole, one a case of partial tubal abortion, and one a case of fulminating rupture. On each occasion the operation was performed without any great technical difficulty, and all the patients made a normal and uninterrupted recovery. Catgut was employed as the suture material, a round-bodied intestinal needle being used. No difficulty was experienced in maintaining the lumen of the tube, owing to its dilated condition. Care was required, however, to avoid laceration and unnecessary damage to the tubal wall owing to the softened condition of the tissues.

Hæmorrhage did not create a difficulty, although in the case of tubal rupture of a two and a half months' gestation, the precaution was taken of ligaturing the ovarian vessels before the tube was sutured.

With regard to the place of salpingotomy in dealing with cases of tubal rupture, probably salpingectomy is the better procedure. In the presence of severe hæmorrhage life is more important than conservatism, and the interests of the patient are probably better served by excising rather than suturing a badly lacerated tube.

In the case, however, of tubal mole and tubal abortion, salpingotomy

appears to be worthy of a trial as an alternative method of treatment. Time, of course, must prove whether the adoption of such a procedure is followed by other complications such as recurrence of the accident (for pure accident I believe it to be in the majority of cases), the subsequent development of hydrosalpinx, or, perchance the incidence of a tubal chorion-epithelioma.

DISCUSSION.

Dr. AMAND ROUTH asked if the pedicle of the mole was not often attached to the muscular wall rather than to the mucosa? Would not also the hæmorrhage be sometimes between the muscle and the mucosa instead of being intramucosal as in the cases described?

Mr. BECKWITH WHITEHOUSE (in reply) said that the pedicle of a tubal mole was not directly attached to the mucosa of the tube, but to the submucosal tissues. Rupture occurred through the mucosa, and a collar of the latter could almost invariably be demonstrated surrounding the pedicle at the point where rupture had taken place. In the specimens investigated it was most unusual for hæmorrhage to occur to any great extent between the muscle and the mucosa. The mucosa was too delicate a structure to withstand the tension. In reply to a remark made by another speaker, Mr. Whitehouse said that the risk of subsequent hæmorrhage from the point of attachment to the pedicle appeared to be negligible. In one or two instances only had it been necessary to apply a suture to the mucosa at the seat of rupture. A single catgut suture had sufficed. The whole operation was remarkably simple as regards technique and no difficulty had been experienced in maintaining the lumen of the tube, owing to its previous distension. Time of course must prove the ultimate value of the procedure.

Case of General Vascular Carcinoma.

By JOHN B. HUNTER, M.Ch., F.R.C.S.

(*Harker Smith Registrar for Cancer Research, University College Hospital.*)

(Introduced by Dr. G. F. BLACKER.)

THE following case is one of great interest owing to the extensive metastasis found throughout the vascular tissues of the body.

Mrs. A. R., aged 35, was admitted under Dr. Blacker at University College Hospital. For six weeks previously she had had rectal and vaginal pain, and discharge from the vagina. There was no history of bleeding, but there had been rapid wasting. The periods had been regular, and not excessive, and the bowels were confined. Owing to great tenderness, examination could only be made under an anæsthetic. The vulva was normal. There was marked thickening of the vaginal walls, especially anteriorly and posteriorly. The vagina was constricted, and admitted only the index finger. The walls were hard, indurated and fixed, the induration extending down to, and partly including, the urinary meatus. With a speculum the cervix appeared nodular, bleeding slightly on examination, but there was no ulceration. The only treatment thought likely to influence the condition was radium: 200 mg. radium bromide was inserted (September 12, 1921) into the vagina and kept in position against the lateral walls by dental wax, and left for twenty-four hours. Ten days later (September 22, 1921) the patient felt better, the pain

and tenderness had almost disappeared, and it was now possible to examine the cervix digitally without giving her pain. The pelvis was externally radiated for four hours with 200 mg. radium bromide. Seen again a week later (September 29, 1921) she stated she had very little pain, and that she was now able to sit down in comfort and to urinate without pain or dribbling, and that the foul discharge had almost ceased. There had been bleeding for three days, which she put down to a period. On examination, which was easier than before, the vagina appeared smooth, and the cervix showed no ulceration. She complained of pain in the left chest and between the shoulders. Her respirations were 35 and she had a slight eruption on the left side of upper lip; there were no physical signs in the chest. She was admitted for further treatment of the upper vagina and cervix seven days later (October 6, 1921): 200 mg. radium bromide was inserted against the cervix and packed off from the rectum.

Three weeks later (October 27, 1921) she was admitted with respiration of 40-44, pulse 136-140. The eruption seen previously on the lip now looked like a deep nævus. She had a similar condition on the back, abdomen, and both palms, but these eruptions were smaller, and not swollen; she was very cyanotic. The only physical sign in the chest was, that the left side did not move so well as the right. The X-ray showed a stippled appearance of the lungs, but no shadow that could be definitely said to be a secondary mass. The condition of the cervix and vagina appeared to be greatly improved. The blood count showed 4,600,000 red cells, and 16,500 white cells, polymorphs being 85 per cent. She remained in a very exhausted condition and died fourteen days later (November 10, 1921), her pulse during this period varying between 140-150, and respirations 36-50. Her temperature rose on the fourth day to 100·6° F., then fell to subnormal.

Post-mortem Findings.—The pericardium contained about 2 oz. of fluid, the right ventricle was distinctly dilated, and there was a small deposit of growth on one of the chordæ tendinæ of the mitral valve. The pleuræ were adherent, especially at the bases, and showed small nodules of growth with larger plaques. The lung tissue was very congested and contained miliary and larger masses of growth. The spleen likewise contained nodules of growth up to $\frac{1}{2}$ mm. in width. The liver contained numerous groups of small growth lying in congested and atrophied areas of liver substance of dark red colour: the mucous membrane of the gall-bladder was studded with numbers of minute cholesterin pearls. In the pelvis, Douglas's pouch was closed with adhesions in which nodules of growth were present. The vagina was smooth and the walls thickened. The cervix was smooth except for a small area of ulceration round the os. On cutting the vaginal walls, cervix, and lower uterine segment a tough white fibrosis was apparent. No gross glands were found in the pelvis, the broad ligaments were thickened. There was nothing in the rectum.

Microscopically all the organs examined showed the presence of growth. The vagina and cervix, besides the presence of unhealthy cancer cells, show areas in which degeneration of the cells has taken place, also newly-formed fibrous tissue. In the broad ligament the cancer is widespread, and can be well seen in the vessels, some of which are entirely blocked by the growth, while others have a central core of growth with circulating red cells surrounding it. In the liver the condition is interesting—the cancerous emboli have apparently arrived by the portal stream from the hæmorrhoidal veins, and have lodged in

the portal radicles of the organ. The portal veins can be seen blocked, with the hepatic arteries and bile ducts beside them free from growth as are also the hepatic veins. The lungs show the diffuse nature of the spread; throughout the section, the small vessels can be seen to be blocked; presumably the channel of conveyance here has been directly through the systemic vessels from the veins of the broad ligament. Here, likewise, larger masses of growth can be seen in the bigger vessels entirely surrounded by red corpuscles. The cancerous vegetation on the heart valve shows the circulatory nature of the growth; presumably an embolism had lodged on one of the chordæ tendineæ in its passage through the heart. The section of the lip explains the navoid condition seen before death. The vessels are choked with growth, the veins greatly dilated and partially blocked, allowing a slow circulation to be carried on through them.

The peculiar distribution of the growth throughout the vessels of the body justifies the term of a "general vascular carcinoma," which I have ventured to give it.

The microscopic appearances described in the various organs are shown in the specimens I have placed under the microscope, and now demonstrate by lantern slides.

Mr. FORSDIKE said dissemination was often thought to be the result of radium treatment, but he felt that there was no real evidence that this happened, and quoted figures in support of his view.

Hyperthyroidism in Functional Menorrhagia.

By ALECK W. BOURNE, M.B., B.Ch., F.R.C.S.

THERE is a large class of women suffering from excessive menstrual loss, who exhibit no signs of pelvic disease, and who are unaffected by the usual treatment of the pelvic organs, either by drugs or curetting. In the absence of any exact knowledge of the cause no scientific diagnosis can be given, but our general acquaintance of the actions of the ductless glands, especially in relation to the reproductive organs, has suggested a diagnosis of some form of endocrine disturbance as lying at the root of the more obscure menstrual disorders. It has been impossible, however, to define with any exactness which gland is at fault, or in what direction its failing lies.

Speculation as to the activities, antagonism and compensatory possibilities of these glands in relation to pelvic disease is almost unlimited, and clinical diagnoses and treatments are often based upon little more than hypothesis, unsupported by real evidence.

This vague uncertainty has resulted in the empirical administration of glandular extracts, either singly or in combination, in the hope that one or other may supply what is lacking. This mode of treatment or investigation, however, is disappointing, and carries us no further.

Amidst a mass of theory there stand out certain facts which have been derived from experimental work on animals, the study of clinical material, and the effects of the administration of certain principles such as adrenalin, thyroid and pituitary extracts, in limited and well defined conditions.

We know also the cause of some diseases, such as cretinism, acromegaly,

Addison's disease and others, in terms of the endocrine gland responsible, but with regard to menstrual disorders we know little beyond the fact that menstruation is dependent upon the presence of ovarian tissue. Amenorrhœa in cretinism and myxœdema is evidence of the necessity of a functioning thyroid for menstruation to appear.

It has been the object of this work to define a variety of menorrhagia due to thyroid excess, and to define it by precise quantitative measurements of the degree of hyperthyroidism.

Not all varieties of hyperthyroidism are associated with menorrhagia, or any other menstrual abnormality, for in Graves' disease the menstrual condition is variable, sometimes excessive, sometimes diminished, and occasionally unaltered. But I hope to show that one form of hyperthyroidism is not only associated with, but is the cause of some forms of menorrhagia, first, by the raised value of the basal metabolism, and secondly by the response of the basal metabolism and the clinical condition to X-ray treatment of the thyroid gland. I am not putting in the forefront any special therapeutic claim for the curative value of the X-ray treatment of the thyroid gland. Clinical results will be given rather as supporting evidence of the hyperthyroid theory than as a reason for X-ray treatment of the gland.

The method employed for measuring the degree of thyroid activity has been the now well-known estimation of the basal metabolism of the patient. Much evidence has been accumulated to show that it is a true thyroid index, and I have ventured to accept this measurement as a basis for diagnosis and treatment. All the estimations have been carried out by Dr. C. M. Wilson at St. Mary's Hospital.

The patients selected for investigation have been chiefly young unmarried women, complaining of excessive menstruation, examination of the pelvis in whom has revealed no physical signs of local disease. Selection was further based upon certain other clinical symptoms and signs usually associated with hyperthyroidism (Table I, p. 26).

The more important clinical points were as follows: Menstruation lasted for a variable number of days, but tended to preserve a definite rhythm, though irregularities were not uncommon. A two- or three-weekly periodicity was often met with. Pre-menstrual pain was frequently described. The patient's weight was usually decreased, or she was conscious of a loss of flesh having recently taken place, and there were symptoms of nervous instability in the shape of irritability, insomnia, and lack of concentration power; while lassitude, easy fatigue on exertion, backache, and a tendency to free sweating were other general symptoms.

The physical signs were not as a rule striking. The pulse was only slightly raised to the neighbourhood of 90, except in the case of two or three patients who showed an unusually high basal metabolism. In others the pulse-rate was within normal limits.

The knee-jerks were generally distinctly increased.

The thyroid gland was occasionally enlarged, but the clinical state of the gland did not always bear relationship to the figure of the basal metabolic rate, or to the degree of the hæmorrhage. For example, a child aged 14, suffering from dangerous floodings at puberty had a definite goitre, chiefly affecting the right lobe of the gland, but her basal metabolism was exactly normal. On the other hand, others with a high metabolic index showed no thyroid enlargement at all. The amount of enlargement of the thyroid was seldom more than

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enough to impart a slight degree of fullness to the neck, whilst in many cases the appearance of the gland was normal. This independence between thyroid enlargement and severity of the symptoms or the value of the basal metabolism is a common experience in Graves' disease.

TABLE I.

Case	Age	State	Pulse	Basal metabolic rate	Symptoms
4	27	S.	76	+19.6	Irregular, profuse menstruation; curetted without improvement
5	26	S.	82	+18	Intractable menstrual excess; thyroid enlargement, neck 13 in.
7	17	S.	96	+23.1	Puberty at 16; irregular and profuse loss; thyroid enlargement
8	28	S.	102	+46	Severe menorrhagia, curetted without improvement
9	17	S.	68	normal	Puberty at 14; severe and irregular loss
10	14	S.	96	normal	Dangerous floodings at onset of puberty; moderate asymmetrical goitre
11	28	S.	96	+23.5	Monthly excess and pain; thin, and tendency to exophthalmos
13	20	S.	84	+19.4	Menorrhagia since puberty, with pain
14	41	S.	66	+20.5	Lifelong menorrhagia and treatment with no improvement
15	14	S.	90	+11.2	Serious hæmorrhage of puberty
16	26	S.	78	+20.4	Menorrhagia for seven days, thin and pale
17	21	S.	74	+23	Menorrhagia and dysmenorrhœa, normal thyroid
18	?25	S.	72	+21	Menorrhagia
19	18	S.	94	+19	Menorrhagia since puberty; continual loss for six weeks; puberty at 13
20	40	S.	—	+14	Long history of hæmorrhage and dysmenorrhœa
21	25	M.	72	+16	Menorrhagia since confinement two years ago
22	22	M.	74	+18	Menorrhagia since confinement seven years ago
23	33	M.	—	+25	Menorrhagia since confinement seven years ago
24	13	S.	112	-20	Profuse floodings since puberty, August, 1921
25	38	S.	—	+24	Severe menorrhagia for seven years; pelvic organs normal
26	13	S.	—	-1.5	Free loss for ten days each month; puberty August, 1921; thyroid not visible
27	35	S.	—	+15	History of exophthalmic goitre; now has fortnightly menstruation; pelvic organs—nil abnormal

Note.—Cases 1, 2, 3 and 6 were X-rayed before the basal metabolism was determined.

True exophthalmos was absent except in one case. In a few cases there was a slight suggestion of a widened palpebral fissure when the patient was excited in conversation, but the majority did not show any tendency to exophthalmos. Slight tremors of the fingers were occasionally present. The clinical picture cannot be described as one of Graves' disease, even during the

early stages of the illness, and I am told by Dr. Wilson that the commencement of Graves' disease is not often associated with menstrual disturbance, whereas the outstanding symptom in this series of cases is menorrhagia. Further, the history of hæmorrhage often extended over many years in those patients who still did not exhibit the symptoms of exophthalmic goitre.

Turning now to the evidence of the basal metabolic rate, the results of Dr. Wilson's estimations will be more clearly seen if rendered in the following tabular form (Table II).

TABLE II.

Case	Basal metabolic rate before treatment	Basal metabolic rate after treatment	Clinical notes after treatment	Remarks
4	+19.6	+6.1	Menstruation normal	Later relapse to +22.7 and increased hæmorrhage
5	+18	-1.3	Normal periods, delayed ten days	—
7	+23.1	+24	No improvement	Refused treatment after six weeks
8	+46	+12	Normal menstruation of four days	—
9	normal	—	Menstrual improvement	Hæmorrhage of puberty
10	normal	-3	Menstruation still excessive, no further flooding	Hæmorrhage of puberty
11	+23.5	+2	Normal menstruation	—
13	+19.4	+16	No improvement	—
14	+20.5	+8	Normal menstruation; weight increased	—
15	+11.2	+6	No better	Hæmorrhage of puberty
16	+20.4	—	Normal menstruation	—
17	+23	—	Scanty menstruation; much pain	—
18	+21	+2	Normal menstruation	—
19	+19	+7	Loss normal and quite regular	—
20	+14	—	No better	—

Note.—Cases 1, 2, 3, and 6 were treated by X-rays before the basal metabolism had been measured.

It will be seen that excluding five cases of girls at the age of or just after puberty, the figures range around + 20 per cent., with two or three exceptions. It is noteworthy that five of the six patients at or just after the age of puberty did not show any tendency to abnormal increase in the basal metabolic rate, and in one of these it was as low as - 20. The distinction of this group from the others by the reading of the basal metabolism is further emphasized by the contrast in their behaviour to X-ray application to the thyroid. These findings are the more striking in view of the marked enlargement of the gland displayed by three of these patients. In Case 10 there was a moderate goitre, chiefly affecting the right lobe.

From the ætiological point of view three cases require special note. They

are Cases 21, 22, 23—those of married women having had children, in whom menorrhagia has dated from the last confinement.

From my own and Dr. Wilson's investigations of the basal metabolism in pregnancy, shortly to be published, we have learnt that the metabolic rate is almost invariably raised to the extent of from + 15 to + 30 during the later months of pregnancy. Whilst it is impossible to draw any conclusions from three cases, the state of these patients nevertheless suggests that the question of the possibility of the hyperthyroidism of pregnancy persisting afterwards, and producing menorrhagia, is worth following up. The interesting clinical point about these patients is the fact that in all three the uterus was distinctly smaller than normal, and therefore quite unlike the ordinary condition of post-sapraemic endometritis associated with menorrhagia. It would appear that these patients were suffering from a subinvolution of the thyroid.

The second piece of evidence supporting the hyperthyroid origin of the disease is the effect produced by X-ray application to the thyroid gland. In all the cases in our series the radiographic work was carried out by Dr. G. Harrison Orton of St. Mary's Hospital, and it consisted in superficial exposures of either lobe of the thyroid gland of about three minutes' duration, at intervals of from two to three weeks.

The number of applications given to each patient varied, but between four and a dozen were usually found to be sufficient. The result of direct treatment of the thyroid and of the entire neglect of any pelvic treatment can be seen in the alterations in the basal metabolic rate simultaneously with the improvement in the clinical condition. On the other hand, the patients who resisted X-ray treatment showed persistence of a high metabolic rate in correspondence with the failure to improve; and, further, relapse in the clinical condition was associated with a fresh elevation of the basal metabolic rate.

Whereas the majority of the patients show a favourable reaction, the exceptional cases require separate mention.

Case 4 was that of a woman, aged 27, who complained of menorrhagia extending over many years, of loss for eight days irregularly, and of general dysmenorrhœa. She was curetted in 1918 with only slight improvement resulting. In addition this patient was of feeble neurasthenic condition, always ailing and obsessed with the sense of her own ills. In two months, X-rays had reduced her basal metabolic rate from + 19·6 to + 6·1—a figure within normal limits. At this time, her period consisted of two days' free flow, followed by four days' slight loss, a considerable improvement on the eight days' flooding which had troubled her previously. The improvement lasted three months, after which she began to lose freely again, and relapsed to her former condition. On taking her basal metabolic rate again it was found to have increased from + 6·1 to + 22·7.

Case 14 is that of a patient who suffered from profuse and slightly irregular bleeding for eight years (i.e., since puberty). Treatment by drugs had been ineffectual, and, later, when she was curetted, the scrapings were normal in appearance. Her basal metabolic rate at the commencement was + 19·4, and in spite of many and strong doses of X-rays it did not fall below + 16 on three separate occasions, over a period of eight months. In correspondence with the persistently high basal metabolic rate it has been impossible to relieve her menorrhagia, and the last report stated that her condition was as bad as before X-rays were applied.

Case 23 was that of a patient admitted to hospital for a recurrent

Bartholin's abscess, almost certainly of gonococcal origin. In addition she complained of fourteen days' loss every month, and she had a moderately-sized symmetrical goitre. In view of the association of a goitre with severe hæmorrhage Dr. Wilson measured her basal metabolic rate and found it to be exactly normal. The inference is that the hæmorrhage originated from an organic infective endometritis, and that, in spite of the non-toxic goitre, she was not suffering from hyperthyroidism.

The patients at or about puberty appear to fall into a different category from those of a later age. I am unable to draw any conclusions from six cases, and can only state that of the six two were exactly normal as regards their basal metabolic rate, one was raised just above the normal limits to + 11'2, the fourth showed definite hyperthyroidism, and two exhibited a diminished value.

In four other cases the treatment by X-rays was begun empirically, for what was clinically diagnosed as hyperthyroidism; this was before I began to send patients to Dr. Wilson for estimation of their basal metabolic rate. For these cases we have no record of the basal metabolic rate before treatment, but only at the end.

The first had severe menorrhagia for which she had been curetted, and had tried many drugs, including pituitary tablets and hypodermic injections. During the two ensuing months, November and December, 1920, she had X-ray treatment, and from then until when last seen in September, 1921, she has had perfectly normal periods of four days. Her basal metabolic rate is now - 3'6. The second patient, similarly, had been treated by drugs and curetting, and was no better. X-ray treatment was applied during the autumn of 1920, and when last seen in May, 1921, she had a basal metabolic rate of - 13, and amenorrhœa.

Two other patients showed a similar clinical reaction yielding basal metabolic rate figures of + 2'5 and + 1 respectively, with clinical cure.

The figures and clinical results above given I believe justify us in concluding that there is a group of cases of functional menorrhagia associated with hyperactivity of the thyroid. It is possible that other varieties of functional menorrhagia are due to disturbance of some other glands; further work is required in order to segregate these clinical groups.

I do not assert that X-ray application to the thyroid gland is an adequate treatment for this variety of hæmorrhage, because sufficient time has not yet elapsed to enable us to arrive at any judgment as to the permanence of its effects. My belief is that a continuance of the investigation will see the fulfilment of the promise which these results hold out.

Dr. HARRISON ORTON said he had been asked by Mr. Bourne to make a few remarks concerning the X-ray treatment: but he did not think there was a great deal to be said at present. It was certainly not necessary, nor did he think it advisable, to give very large doses. All the cases described by Mr. Bourne, with one or two exceptions, had been treated in exactly the same manner, the procedure being as follows: to start with, a dose of $\frac{1}{2}$ B or 5X was given once a week for three doses, the filter being 1 mm. of aluminium and four layers of wash-leather. After this, doses of $\frac{1}{4}$ B were given once in three weeks through a filter of 3 $\frac{1}{2}$ mm. of aluminium and wash-leather as before; rays should be hard, not less than 9-10 on the Bauer qualimeter. The average number of doses was about eight. It was important that no erythema of the skin be produced as this was likely to lead to the formation of telangiectasis in the skin, which might become very unsightly. In one of Mr. Bourne's cases, and in another

case of Graves' disease, he (Dr. Orton) had seen a tendency to myxœdema produced after eight such exposures, so he thought it advisable to have the basal metabolism tested again after six exposures; on the other hand, some cases had had considerably more than eight exposures without improvement, so that the amount required seemed to be very variable. It should be remembered, too, that cases of Graves' disease in which there had been no X-ray exposures at all, and in which myxœdema had developed subsequently, had been reported. He had no doubt that the estimation of the basal metabolism would eventually lead to the determination of much more accurate dosage.

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Section of Obstetrics and Gynæcology.

President—Professor HENRY BRIGGS, F.R.C.S.

Glycosuria in Pregnancy.

By R. L. MACKENZIE WALLIS, M.D.

(ABSTRACT.)

[This paper will be published in full in the *Journal of Obstetrics and Gynæcology of the British Empire.*]

THE investigations made upon the blood and urine in cases of glycosuria in pregnancy revealed the existence of several features of interest. The cases were divided into two groups—namely, the intermittent or transitory glycosuria of pregnancy and the severe glycosurias and diabetes mellitus. The first group presented characters of particular interest, and, consequently, attention had been paid to this type. The investigation of the urine was confined largely to the question of the nature and amount of the sugar present. The presence of lactose was always excluded, and it was often a matter of some difficulty to decide whether the reducing substance present was sugar or not. In collaboration with Dr. P. J. Bose a method had been devised of detecting and estimating sugar in the urine in both normal and abnormal amounts. This method depended upon the removal of all interfering substances by means of an acid solution of phosphotungstic acid, and the identification and estimation of the sugar present in the clear filtrate. By means of this test it was possible to determine the nature and amount of sugar present in normal urine. The figure usually obtained was from 0.07 to 0.09 gm. per cent., and the total output of sugar on an ordinary mixed diet rarely exceeded 1 gm. *per diem*. In pregnant women it was possible by this method to determine variations in the amount of sugar in the urine. The blood sugar examination was done simultaneously, and used to determine the nature of the case, as well as to watch the effects of treatment in a severe case of glycosuria in pregnancy. The glucose tolerance test had, however, proved of the greatest value in diagnosis and prognosis. The charts shown illustrated the types of curve found in the transitory glycosuria of pregnancy. These were obtained by giving the patient 50 gm. of glucose by the mouth and estimating the blood sugar content at intervals of a quarter of an hour, half an hour, one hour, one hour and a half, and two hours after the sugar had been consumed.

Plotting the curves together, it was found that the glycosuria in pregnancy gave curves identical with those found in true hyperpituitarism. In this group cases of marked obesity, acromegaly, and intermittent glycosuria could be included on the results of the sugar tolerance test. This was a point worthy of further investigation in view of the part played by the pituitary body during pregnancy. Further, it would appear that by means of the sugar tolerance test, cases of hyperpituitarism could be detected where no evidence

of any skeletal changes, or other signs of acromegaly, existed. The observations showed that 500 gm. of glucose could be tolerated without the occurrence of sugar in the urine, and, in consequence, this type of case was best treated by leaving it alone. Attempts to restrict the carbohydrates in the diet generally made the patient worse, and the appearance of acetone bodies in the urine was likely to be misleading under such circumstances. The differentiation of severe glycosuria from the intermittent type was rendered possible by a simple estimation of the blood sugar. In a normal person the blood sugar ranges from 0.08 to 0.11 gm. per cent., whereas in severe glycosuria it may be over 0.2 gm. per cent., and reach such figures as 0.4 gm. per cent. The intermittent type of glycosuria, on the other hand, shows a blood-sugar content which is either normal or only slightly raised. The treatment of a severe case of glycosuria can be followed by means of the blood sugar, and the progress of the disease determined. The results of these investigations showed that the two types of glycosuria met with in pregnancy require to be clearly differentiated, since in one type no treatment is required; yet in the other type the greatest care in the dietary becomes necessary. The association of glycosuria and a hyperglycæmia in connexion with menstruation has also been studied, and the results will be communicated later.

DISCUSSION.

Sir WILLIAM WILLCOX thought that in addition to excessive activity of the pituitary gland during pregnancy, attention should be directed to the increased activity of the thyroid gland, since this gland undoubtedly played an important part in the causation of the glycosuria of pregnancy. In the case of patients suffering from diabetes mellitus who became pregnant he thought that the determining factor as to whether pregnancy should be allowed to continue was the carbohydrate tolerance. If this was below 20 gm. the pregnancy should not be allowed to proceed. In cases of diabetes mellitus with a carbohydrate tolerance of 50 gm. or upwards if special care was taken in the dietetic treatment of the patients then the pregnancy might be allowed to proceed with the hope of a successful result. Each individual case, however, must be considered on its merits. In his opinion the *continued* signs of acidosis, as shown by a marked excretion of acetone and diacetic acid in the urine, were indications that pregnancy should not be allowed to proceed to full term when the patient suffered from diabetes mellitus. He was aware that acidosis with acetone and diacetic acid in the urine occurred temporarily in pregnancy apart from diabetes, but the continuance of this in a patient suffering from diabetes was of grave import where pregnancy was present.

Dr. P. J. CAMMIDGE pointed out that Dr. Mackenzie Wallis appeared to assume that glucose and lactose were the only sugars met with in the urine in pregnancy, but this was not his experience. Analysis of the cases he had seen during the past fifteen years showed that glucose was present in abnormal amounts in only 52 per cent., while lactose alone was found in 22 per cent. In the remaining 26 per cent. the reducing substances were either levulose or pseudo-levulose. Of the cases showing glucose, this sugar was present alone in 15 per cent., in another 15 per cent. it occurred along with lactose, in 20 per cent. pseudo-levulose was also present, while in 2 per cent. glucose, lactose, pseudo-levulose and true levulose were found. In his experience a differential diagnosis of the reducing substances present in the urine was of great practical importance for it had a distinct bearing upon treatment. Cases of pseudo-levulosuria and levulosuria did not do well under ordinary diabetic treatment, and might indeed develop serious symptoms of acidosis and toxic poisoning. On the other hand, when they were given an abundance of carbohydrate, especially in the form of dextrinized starches, and their protein intake was limited, their "glycosuria" quickly cleared up and the general condition of the patient rapidly improved; so that cases in which the propriety of inducing labour had been discussed were enabled to go to full term and give birth to healthy children without further untoward symptoms. Cases of pure dextrosuria were

in quite another category and required careful regulation of the total diet according to the tolerance of the individual patient. The most difficult patients to treat satisfactorily were those passing a mixture of glucose and pseudo-lævulose, for a diet which was helpful for the one was unsuitable for the other, and vice versa. In such cases the best results were obtained by a system of alternating diets guided by the results of complete and regular analyses of the urine and blood. Dr. Cammidge described cases illustrating the effects of treatment in these various types.

Dr. GEORGE GRAHAM said that the only point on which he differed from Dr. Mackenzie Wallis was in the interpretation of the blood sugar curves. He did not believe that the cases could be divided into groups according to whether the highest point of the curve occurred a quarter of an hour, half an hour, one hour or two hours after the dose of sugar. He regarded this as evidence of a greater or lesser diminution of the sugar tolerance. The blood sugar curves of the cases of glycosuria of pregnancy which Dr. Mackenzie Wallis had shown that night resembled the curves of the cases of mild diabetes in men or non-pregnant women, and showed in his opinion that there was a diminution of the sugar tolerance. Such a temporary diminution of the sugar tolerance was produced by many diseases, such as a carbuncle, or gangrene of the leg, or pyorrhœa. He regarded the glycosuria of pregnancy in the same light and thought that the strain of pregnancy caused a temporary diminution of the sugar tolerance. He was much interested in what Dr. Cammidge had said about the lævulosuria and its treatment.

Section of Obstetrics and Gynæcology.

President—Professor HENRY BRIGGS, F.R.C.S.

A Wandering Silk Suture removed from the Urethra as a Sequel to Cæsarean Section.

By S. GORDON LUKER, M.D., F.R.C.S.

S. S., AGED 34, married, was admitted to the London Hospital on May 22, 1921, at full term with a generally contracted pelvis. She had had two previous children by Cæsarean section, the last in June, 1916. Labour pains began on May 23 and Cæsarean section was performed at once. The uterus was opened by a longitudinal incision through the lower uterine segment and a living male child weighing $9\frac{1}{2}$ lb. was extracted.

The uterine incision was closed by (1) a continuous silk suture to the deeper muscular layers; this was buried by (2) a continuous chromic catgut suture to the superficial part of the wound. The course of the puerperium was afebrile.

The patient was sterilized at her own request by excision of a portion of each Fallopian tube. After an uninterrupted recovery she was discharged from hospital on June 12.

On January 7, 1922, the patient returned to hospital complaining that "some sort of thread was hanging down" from the vulva. On examination about 3 in. of silk suture was seen to be protruding from the urethra—obviously the buried suture from the lower uterine segment which had

[March 2, 1922.]

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wandered into the bladder. The suture still retained some attachment within the bladder. The free portion was therefore cut off.

The patient did not complain of any symptoms and was therefore discharged and told to report if the remaining portion of silk suture appeared.

The specimen is shown in order to raise the point whether silk should be employed to suture the uterus when the lower segment incision is made.

Dr. ROUTH, Dr. EDEN, Mr. CLIFFORD WHITE, Dr. C. D. LOCHRANE, Dr. GILES and Mr. T. G. STEVENS all stated that they had seen similar cases, and Mr. STEVENS and Mr. CLIFFORD WHITE both said that they had removed silk sutures found after Cæsarean section by curetting.

Adenomyoma of the Recto-vaginal Septum showing Decidual Reaction.

By C. D. LOCHRANE, M.D., F.R.C.S.Ed.

PATIENT, a nullipara, aged 33, married four and a half years. When first seen on September 26, 1921, uterus appeared to be about eighteen to twenty weeks pregnant. Was sent to me on account of a small tumour in the vagina.

Examination further showed an irregular multilobulated non-tender tumour apparently imbedded in the vaginal wall in the posterior fornix, in or about the middle line. It had no palpable connexion with the uterus, from which its nearest point was distant, approximately $\frac{1}{2}$ in. The limits of the growth appeared well defined. Some of the nodules of which it was composed had a cystic feel, especially the largest, which bulged into the rectum; it appeared to be submucous and was best felt by rectal examination. The whole mass apparently measured about $\frac{3}{4}$ in. vertically by 1 in. antero-posteriorly, and had a fairly definite outline. A small polypus of mucous or fibro-adenomatous type hung from the external os. There was no evidence of uterine fibroids.

Patient stated that she had been aware of something in the rectum during defæcation for more than a year before pregnancy occurred, and that her private doctor had noted there a tumour the size of a bean twenty months before, but had called it a polypus. On one occasion, about a year ago, she felt that the bowels were obstructed, and on that occasion she twice had bleeding from the bowel. There was no family history of tubercle, and no personal history at all suggestive of gonococcal or other infection.

Menstrual periods before pregnancy were of the three to four twenty-eight-day type, with moderate loss (D. 6) and no clots, till two years ago. Since then she had had a slight loss lasting one day and occurring about a week after each period. Periods always painless.

A small portion of the tumour was excised *per vaginam* a fortnight later and was reported on by Mr. T. G. Stevens as an adenomyoma showing decidual reaction in the connective tissue stroma of the gland spaces. The appearances were so unusual that another and larger portion was excised eight weeks later. Sections of this specimen supported the view previously expressed. The tumour itself appeared to be slightly larger than on the previous examination. The polypus unfortunately was not examined microscopically.

The reports of the microscopical examinations kindly made for me by Mr. T. G. Stevens are appended (*see p. 37*).

Decidual reaction is very rare in adenomyomata whether of uterine or extra-uterine type. Lockyer has collected eight uterine cases. The only other recorded case of an extra-uterine adenomyoma showing decidual reaction was one similarly situated reported by Dr. Griffith to this Section in July, 1914.¹

Griffith's case was also associated with pregnancy. A great many cases of septal and other peri-uterine adenomyomata have been reported at various times, but these are the only two which have been associated with pregnancy, and are also the only two showing decidual reaction.

Though these cases are too few to justify a generalization in favour of a Müllerian origin of these growths, they show that, given the opportunity of pregnancy, 100 per cent. of peri-uterine adenomyomata have shown pronounced decidual reaction, a fact which is strongly suggestive of a Müllerian origin. In the case of similar tumours occurring in the uterus itself in association with pregnancy the Müllerian view receives strong additional support from evidence of the same sort. Lockyer in his monograph on "Fibroid and Allied Tumours," records eight uterine cases associated with pregnancy, collected from the literature. In only two of these was a decidual reaction in the tumours reported as absent, but in this connexion it must be noted that there is no evidence in these two reports of serial sections of the tumour having been examined. This is a point of critical importance, as an examination of different areas of my own sections will indicate, and it reduces the value of these negative observations as evidence against the Müllerian theory of origin. It has of course been contended that decidual reaction is not peculiar to Müllerian tissue, but the evidence in support of this view is neither abundant nor entirely convincing. The absence of any record in the literature of decidual reaction unassociated with pregnancy, occurring in an adenomyoma, wherever situated, lends further support to the above arguments. There is nothing unlikely in the view that these peri-uterine adenomyomata arise from Müllerian tissues. Their infiltrative character is well known, and several examples of such tumours in the broad ligaments have been shown to have a direct connexion with uterine or tubal mucous membrane. Cullen maintained that he was able to trace a direct connexion between the uterine endometrium and adenomyomata situated in the wall of the uterus in the great majority of his cases, and Doederlein and Hertzog reported a pregnancy in a cavity of a broad ligament adenomyoma, which had a fairly wide channel connecting it with the uterine cavity.

The occurrence of adenomyomatous areas in sites further removed from regions in which Müllerian tissue is normally found, is more difficult of explanation, and leaves the origin of these latter open to speculation. There appears to be no conclusive evidence justifying a contention that they are derived from Müllerian tissue. There is no intention of including them in the scope of this communication.

Finally it should be noted that the presence of a polypus in this case suggests a previous inflammatory condition of the cervical or uterine mucous membrane, and this is very usual, at least in the case of tubal and uterine adenomyomata. It is on the frequent finding of evidences of a coincident, or antecedent inflammatory condition, in or about the site of these growths, and also upon their regional variation, that the essential inflammatory nature of these growths is maintained by many. Upholders of the inflammatory view

¹ *Proceedings*, 1913-14, vii, p. 389.

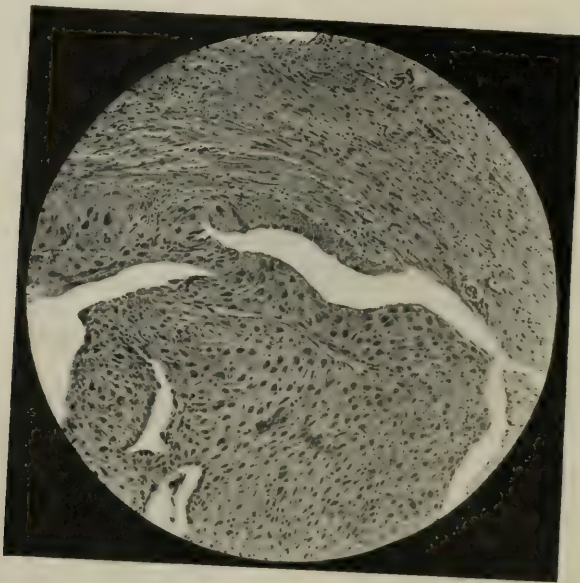


FIG. 1.—Adenomyoma of recto-vaginal septum with pregnancy. ($\times 75$.) Note extensive decidual reaction in the cellular mantle surrounding irregularly dilated gland spaces. Non-striated muscle tissue seen in upper part of section.

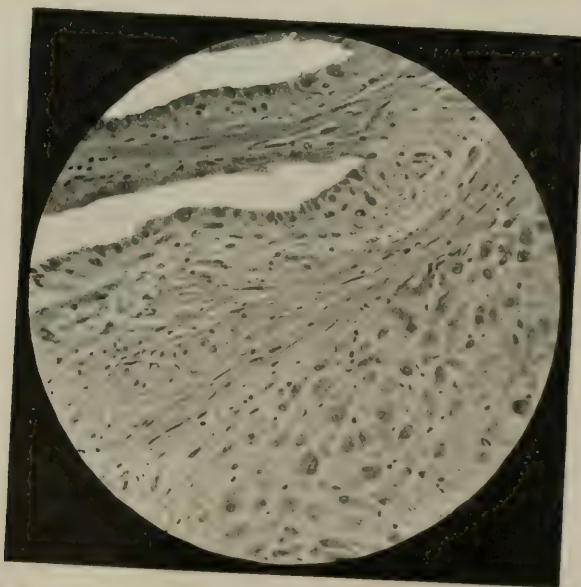


FIG. 2.—Decidual reaction in extra-uterine adenomyoma. ($\times 150$.) Note (1) Extensive and typical decidual reaction; (2) Layers of non-striated muscle; (3) Type of epithelium lining gland spaces.

object to the term "adenomyoma," as applied to them, wherever situated. They prefer to name the condition "adenomyositis," and believe that it may arise in association with a chronic inflammatory condition in almost any epithelial, or serous surface.

LABORATORY REPORTS BY THOMAS G. STEVENS, M.D., M.R.C.P., F.R.C.S.

October 14, 1921 (patient eighteen to twenty weeks pregnant): "This material is difficult to interpret as it is rather broken up and has lost its continuity. It clearly shows vaginal mucous membrane and submucous connective tissue with dilated vessels. Deep to this, as can be seen in one or two pieces, there is a much looser tissue showing a well-marked decidual reaction. This, we conclude, is the actual lesion. It contains wide spaces, only a few of which have any epithelial lining. Around this tissue is an incomplete layer

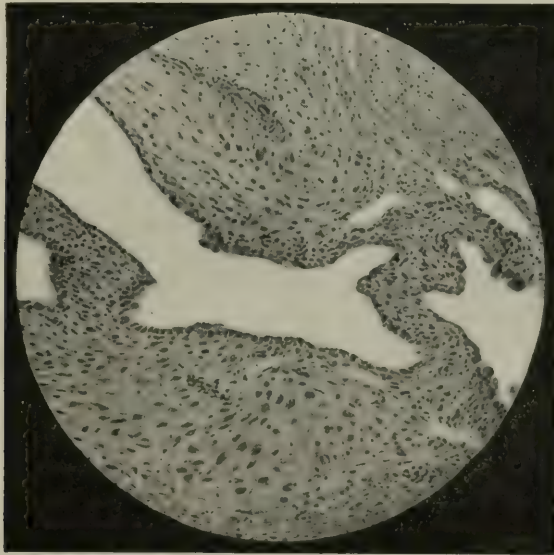


FIG. 3.—Decidual reaction in extra-uterine adenomyoma. ($\times 90$.) Another portion of same specimen, showing distended gland spaces lined by columnar epithelium (flattened in places), and surrounded by tissue, having appearance of typical decidua compacta.

which might be smooth muscle. Although the structure is entirely different from that usually seen, we believe the little growth is an adenomyoma, the peculiar appearances being the result of the decidual changes occurring in the stroma which surrounds the glands. Incidentally, unless this tissue was primarily derived from the endometrium we do not see why it should show a decidual reaction."

December 7, 1921: "This specimen bears out the original diagnosis and shows exactly the same appearances, only rather better. The muscle portion is well shown, the glands in places have obvious epithelium and the stroma shows the same decidual changes. We have no doubt that the growth is an adenomyoma, showing decidual reaction in the stroma around the glands."

Mr. T. G. STEVENS, anticipating that the exact nature of Dr. Lochrane's specimen might be questioned, gave his reasons for considering that the growth was an adenomyoma showing a decidual reaction in the cellular stroma directly surrounding the gland tubules. Mr. Stevens considered the specimen a most important one, as it served as a link in the chain of evidence in support of the theory that all adenomyomata were in reality derived from the endometrium. Although a decidual reaction was occasionally seen in tissues not derived from the Müllerian duct, such as the stroma of the broad ligament, such specimens were exceedingly rare, and then only occurred in immediate relationship with a tubal pregnancy which had ruptured between the layers of the broad ligament. On the other hand, any tissue directly derived from the Müllerian duct would be expected to share in the decidual changes which normally occurred in the endometrium.

Sterility with Reference to the State.

By R. A. GIBBONS, M.D.

ALTHOUGH sterility is naturally of keen interest to the individual who is anxious to have a child, it is of more vital moment to the millions constituting the State. The question becomes one of national concern, for upon a good average birth-rate depends the maintenance of the race. The population of a country is estimated by the balance of births over deaths, and of immigration over emigration to other countries. It is quite evident that if there is a declining birth-rate in any nation, and it is persistent, nothing really matters as to the nation's wealth, ability, or accumulation of art treasures, for it is decadent, and must be considered doomed. Therefore everything which science can do to produce healthy bodies, sanitary surroundings, abundance of good food at a cheap rate, and labour with proportionate rest, should be supported by the State. We know that healthy men and women living in wedlock should "be fruitful and multiply" if in hygienic surroundings, because congenital sterility or unavoidable sterility is rare, both in men and women. Therefore it has been truly said that the percentage of sterility is the index to the morals of a nation [1]. As we know that of late years the number of children born in each family has steadily declined from its former average it is evident that this matter becomes one of State importance.

I have taken the following from the annual report of the Registrar-General, and, without producing all the figures, may state that in England and Wales, from 1897, when there were 921,693 births, with a rate of 29.7 per 1,000, the numbers have gradually fallen to 668,340 births, with a rate of 17.8 per 1,000, in the year 1917. In 1918 the birth-rate was 17.7 and the civilian death-rate 17.6 [2], and in 1919 the birth-rate was 18.5.

Between 1840 and 1880 the birth-rate of England and Wales may be regarded as having been stationary at about 35 per 1,000, so that, compared with the above, the descent to 17.8 per 1,000 is serious. In London alone, from 133,616 births, with a rate per 1,000 of 30 in 1897, there was a fall to 80,554, with a rate per 1,000 of 17.5. Even between 1877 and 1909 there was a reduction of the birth-rate in England of over 28 per cent.

The tables of the present official census show that as regards natural increase—that is, the balance of births over deaths—the numbers recorded from 1911 to 1914 may only be compared with those of earlier years, but not later. The fall in the birth-rate during 1915 to 1918 was from 20 to 25 per cent. below what might have been expected in normal circumstances, and can therefore be attributed to the war, but the actual amount of the figures during the inter-

censal period is not nearly sufficient to compensate for the deficiencies of the preceding years [3].

The quarterly official return of births registered during the June quarter of 1920 for England and Wales shows 22,417 fewer births than for the March quarter, and of these births 12,132 were illegitimate. In the September quarter of last year there were 15,017 fewer births than in the same quarter of 1920, and 10,466 less in number than in the previous quarter of 1921. Of the births 9,876 were illegitimate.

I do not consider that we are one whit better morally than other nations, although there can be no question about our wishing to appear so in the eyes of those nations. In the case of our illegitimate children we are eager to hide them away, and, in most cases, to allow them to be brought up quite regardless of the future, so that they cannot be in later years claimed by the State, to which they might be of great use.

The following figures will appear rather startling with reference to illegitimate children. From 1897 to 1916 there were in England and Wales 72,443 illegitimate births, the proportion varying from 39·9 to 48 per 1,000 births. It is also interesting to note that from 1910 there has been a steady rise in the proportion of these illegitimate births to the 1,000 of all births, and during the same period there were 174,919 illegitimate births in Scotland, with a percentage varying from 7·04 to 7·1, and 54,801 illegitimate births in Ireland, with a percentage varying from 2·6 to 3·1 [4].

The proof of how little care is bestowed on illegitimate children is afforded by the mortality. Dr. Stevenson found from his record that 45 per cent. of all illegitimate children were the children of domestic servants, and that, whereas the death-rate for legitimate children was 0·99 per cent., it was for illegitimate children in general 9·1 per cent., and for the illegitimate children of domestic servants 8·5 per cent. during the first year of life. Dr. Amand Routh [5] states that the death-rate of unmarried mothers and their offspring during pregnancy and the lying-in period is about twice as high as that of married women and their children, which shows how much could be done by better nursing and skilful obstetric help.

In Russia, before the war, I was much struck by the very large foundling hospital which they have in Moscow, admitting yearly about 15,000 children. There any newborn illegitimate child is taken and admitted without any question being asked, except, Has the child been baptized, and if so, by what name? If the child does not possess a name one is given, and although the mother may be nameless and never known, she has the power of following up the child there by his registration number and claiming him at any time should she be in a position to do so. Thus the children are educated and cared for until the boys go into the army or navy, or are taught various trades, and the girls are trained as nurses or midwives. Hence these lives are saved to the State, whereas in this country, where we have no such institution, for our foundling hospital is not similar, the poor mother, as I have known more than once, has thrown her child on the dust heap to die, or disposed of the offspring in other ways than would have been the case had there been a similar institution in which the infant would be cared for.

On discussing the possibility of starting such a foundling hospital, with its excellent clinical work, in this country on my return, I was assured that I could not get any support, that it was a premium upon vice, &c., and so my scheme fell to the ground.

Although the birth-rate is falling here, it may be of interest to remark that

England is not the only country in which it is falling, as the following table will show [6, 7] :—

Country	Decade of highest rate	Highest rate	1891-1900	1912
Denmark ...	1851-1860 ...	32.5 ...	30.2 ...	25.6 (1914)
Norway ...	" " ...	33.0 ...	30.4 ...	25.2 (1914)
Finland ...	" " ...	35.9 ...	32.1 ...	27.1 (1913)
Germany*	1871-1880 ...	39.1 ...	36.1 ...	14.3 (1918) 27.5 (1913)
Belgium ...	" " ...	32.7 ...	28.9 ...	22.6 (1912)
Netherlands ...	" " ...	36.4 ...	32.5 ...	28.2 (1914)
Austria ...	" " ...	39.0 ...	37.1 ...	31.3
Italy ...	1881-1890 ...	37.8 ...	35.3 ...	17.9 (1918)
Hungary ...	" " ...	44.0 ...	44.4 ...	36.3
Serbia ...	" " ...	45.4 ...	41.9 ...	38.0
France ...	1801-1810 ...	32.3 ...	22.1 ...	12.1 (1918)†

* Less Mecklenberg-Schwerin and Mecklenberg-Strelitz.

† Seventy-seven non-invaded departments.

France may be considered as the best example of a pathological birth-rate [8] but France does not stand alone in showing an abnormally low birth-rate.

This general decline in the birth-rate cannot be attributed to the postponement of marriage, decrease in the number of those who marry, decrease in the proportion of the total female population which is of child-bearing age, or to diminishing fertility. When all the facts bearing on the matter are carefully considered, we must come to the conclusion that the voluntary limitation in the number of children born is the chief cause of this decline.

As a result of a voluntary confidential census among "intellectuals" it was found that of 120 marriages, 107 were "limited," and 13 "unlimited," and that the average number of children of each marriage was considerably under 2 [9].

A book called "The Fruits of Philosophy" [10]—an essay on the population question, published about 1874, which was withdrawn from circulation, and republished by Bradlaugh and Mrs. Besant, explaining so-called Malthusian methods of preventing conception—was the beginning of these practices being taken up and adopted by a certain number of the educated classes. At first these methods, it may be safely said, were exclusively used by these classes, but as they became more known, the directions for their use gradually percolated through the various social grades, until at the present moment very few newly married women exist who do not know all about them. The proof of this is that in a friendly society giving "lying-in benefits" [11], from 1866 to 1880 the proportion of lying-in claims rose slowly from 217 to 247 per 1,000, and then steadily declined from 1881 to 1904, when it reached only 117 per 1,000 members.

These methods are the result, in my opinion, of the increased indulgence and ever-growing luxury which existed before the war. Those who wanted to keep in the rush of continued social engagements could only do so by curtailing expenses; one way was to avoid having children, with the necessary expenditure attached to them. In the case of others, to whom money was not perhaps of such immediate value, the duties and responsibilities of motherhood were too exacting to allow of thorough social enjoyment, and therefore the chance of conception must be avoided by every means in their power, with the result that if even only one child was born into the world it was a mistake, and if by chance these matters were not immediately discussed after marriage and a child was born, every care was taken that the birth of a second must be prevented.

When Malthus advanced his thesis that the constant tendency of all living

beings was to increase faster than the food supply, he pointed out the immediate checks to population, such as epidemics, war, pestilence, famine, &c., but that the preventive checks were moral restraint and vice—the former peculiar to man through the use of reason, the latter full of injury to general and domestic happiness. He defines moral restraint as restraint from or postponement of marriage from prudential reasons, with conduct strictly moral while unmarried; and he considered that the period of celibacy should be extended until there is a prospect of being able to feed and maintain children.

The whole question, apart from morals, is a very difficult one, for we know that if certain marriages did not take place with the distinct understanding that the birth of children was to be prevented, only on account of financial reasons, these marriages would not occur at all, and those concerned would suffer much unhappiness. But we have only to consider what is best from a physiological point of view, and what in the end leads to healthy men and women for the State.

Undoubtedly the origin of the whole trouble is the cost of living at the present day—a cost which does not seem likely to decrease for some time; and this is a matter which could only be dealt with by the State.

Many years ago, when life was much more simple than at present, there was not the same desire on the part of those marrying to prevent the advent of a family, and the more children a woman had the prouder she became of the number. Now, however, it is all changed, and the main point apparently for a couple about to be married is how they can be most comfortable on their income. In fact, the source of the decline of the birth-rate is not increased poverty but the propagation of the “gospel of comfort,” which has become the ethical standard for all civilized nations. Therefore the presumption is that the fall in the birth-rate is due to conditions within the control of the people.

The following is an interesting table (compiled by Dr. Jacques Bertillon) as to the annual birth-rate per 1,000 between the ages of 15 and 50 in four cities:—

Classification	Paris	Berlin	Vienna	London
Very poor quarters ...	104	157	200	197
Poor quarters ...	95	129	164	140
Comfortable quarters ...	72	114	155	107
Very comfortable quarters ...	65	96	153	107
Rich quarters ...	53	63	107	87
Very rich quarters ...	34	47	71	63
Average ..	80	102	153	109

The table on the correlation of the birth-rate with the social and physical characteristics of the population for the year 1901 shows that the wives in the districts of less prosperity and culture have the largest families, and the morally and socially lowest classes in the community are those which are reproducing themselves with the greatest rapidity [12].

The relation of income to birth-rate has been studied in several European countries. The result of this investigation shows that the birth-rate falls as income increases, and our national statistics point in the same direction.

The conclusion is obvious, that as the decline seems almost universal, and people do not change their morality in a large number of different countries at a given time without some definite cause, the determining one—a strong economic factor—is the “gospel of comfort.”

Now, the test of fertility in a woman is the rapidity with which she conceives; ideal fertility would imply immediate conception after marriage, perfectly normal pregnancy and labour, and, again, rapid conception during the

whole of the sexual period of life. This, however, rarely occurs. Making allowances for sources of error tending to diminish the average amount of fertility, ten is about the average fertility of fertile marriage during the whole of the child-bearing period of life [13].

This shows how important the study of sterility becomes with reference to the State, for we know that on an average one marriage in ten is sterile, which means that during the whole of the child-bearing period there is a loss to the State of 1,000 children for every 100 marriages.

From the point of view of advantage to the State there is another matter which deserves attention, and that is the vitality of the children born. This is of the highest importance, because a woman who habitually brings into the world children who survive only a few hours or days, or are stillborn, is of no service to the State, and therefore infant mortality under one year calls for more attention than has hitherto been paid to it. Important reforms, however, take a long time to mature, and we must hope that with the Ministry of Health many beneficial changes will be brought about with reference to child life.

It is difficult to state accurately how many infant deaths, or what proportion of them, are due to congenital causes. Herbert M. Rich found that in 6,866 deaths under 1 year of age, 23·2 per cent. were due to malformations, congenital debility and premature birth. According to Henoch [14], for every 1,000 children born, 200 die in the first year of life. Dr. Amand Routh [15] estimates that there are four times as many abortions as stillbirths—that is, 2·2 stillbirths and 8·8 abortions to 100 live births—which means that in England and Wales 76,000 fertilized ova die annually before they are born.

Priestley said there is one abortion for every three or four full-time deliveries, and that from three to six out of every ten women abort at least once during their married life [16].

There is another matter which has a serious bearing on the diminishing population, and that is the attempts to procure abortion, either criminally or by the use of drugs. With regard to the latter, the information given by Sir Thomas Oliver as to the use of diachylon by pregnant women in the North of England is important. It is evident from the statistics already prepared that contraceptives or limitations of the occasions of sexual intercourse, which are extensively employed, must account for the fall in the birth-rate, and that this cannot be attributed to any possible decline of natural fertility. With reference, however, to both these matters, it is quite obvious that it would be impossible to collect sufficiently accurate data to be of any real value; but, it may here be remarked, it has been stated that in a large Continental town, where there was strong neo-Malthusian open propaganda, one-third of the pregnancies were aborted [17].

There is a factor which is of the highest importance with reference to the State—that is, the amount of nutriment available. Abundance of nutriment increases the number of births, and this applies to the whole of the animal world, although we know that this does not imply excessive feeding, which has an injurious effect on breeding. It is known how great is the influence of nutrition upon fertility by the results of famine, which leads to decrease of the population. It may be said, generally speaking, it is believed that the effect of comparative freedom from anxiety leads to the increase of fertility, and that the reverse conditions are followed by diminution in childbirth.

It will be interesting for those who compare the statistics of to-day with those compiled, say, twenty years hence, after the establishment of the

Ministry of Health. It will be essential for that department to see that the poorer classes are provided with sanitary and well-ventilated houses, and that those in existence not entitled to come under this description shall be swept away. This means that factory girls and those doing regular daily work in mills and warehouses will be able to be in a healthy atmosphere during the hours that they are at home. Everything that is done to improve the general health of girls, and to keep it at the highest level possible, will ensure that, when they marry, they will have the best chance of conceiving, and, if the husband be sound, of bringing into the world healthy offspring.

It cannot be too strongly emphasized that, from the point of view of the State, it is not conception alone which counts, but the bringing forth of healthy children who are capable of surviving and becoming useful members of the community; and as we are on this subject, I may say that while there is nothing to prevent men and women marrying whilst in indifferent health, according to our present laws, I am strongly of opinion that every man and woman should, before marriage, be compelled to be medically examined. This at least would be the means, in many cases, of discovering direct evidence of syphilis, gonorrhœa, tubercle, or of insanity, or of obtaining such information as would lead to the suspicion of the existence of one or other of these diseases. Gonorrhœa may be said to be the commonest cause of absolute and relative sterility in women, probably 50 per cent. of all cases. With such a percentage this disease must have an important bearing with reference to the State.

The influence of gonorrhœa and syphilis on the birth-rate is exerted in different ways: gonorrhœa lessens the birth-rate by preventing conception; syphilis influences the birth-rate and infantile mortality in a different and much more serious way. We know from the figures of Hochsinger [18] and of Veeder and Jeans [19] that about 40 per cent. of pregnancies in syphilitic women end in abortions or stillbirths, and of the infants born alive about one-quarter die. Dr. Amand Routh considers that about 25 per cent. of abortions and stillbirths are due to syphilis in city populations, and in rural districts the proportion is probably from 15 to 20 per cent. [20].

Elsewhere I have called attention to the influence of the gonococcus on the female pelvic organs [21], and when I wrote that paper there were over 71,000 cases of gonorrhœa in the British Army alone. Therefore the prophylaxis should be studied closely, and ought to be dealt with from a practical point of view. It ought to be our aim to stamp out this disease, and although in the light of our present knowledge total eradication is at present beyond us, we shall do most good by encouraging in every possible way any means to the end in view, and notification comes first of all.

As we are now certain that every case is a potential means of not only spreading the disease, which may be mild or severe, but of practically crippling a healthy woman for life, as well as possibly rendering her sterile, it is our duty to leave no stone unturned to accomplish our object. Doubtless there are many arguments against compulsory notification of syphilis and gonorrhœa, but we know that, owing to these diseases, the loss to the State is enormous. If we are to have healthy men and women, individual susceptibilities ought to be swept aside, and the State should have control of the treatment. The public will soon be educated to the fact that State registration can be done with the greatest privacy, and that no one will be concerned with any name but the doctor entering it, whilst officials have only to deal with figures. Therefore, all these communications will be treated as confidential State papers, open for statistics to the Registrar-General alone.

The extreme importance of registration is in order to know when a patient is pronounced to be cured. My strong opinion is that when once the name has been entered by a doctor the individual should be obliged to consider himself or herself under treatment until medically certified as well, and that he or she should be penalized if it can be proved that intercourse has taken place before receiving a certificate of health or of having been entered in the registry as cured. By no other means can this scourge be eradicated.

Moreover, I am strongly of opinion that no marriage should be considered legal until both the man and the woman about to enter upon it have been passed as fit for the responsibility of the possible procreation of children. This may seem a harsh condition at the present time, but the people would soon be educated to the knowledge that the State would not recognize a marriage between individuals unfit for matrimony. Parents, therefore, would know that, before allowing any girl to become engaged, they must be sure that the man she wished to marry was healthy in all respects. In this way unsuitable marriages from a State point of view could be avoided. This means that a medical certificate must be obtained by each, but considering that every man and woman desirous of obtaining life assurance must be examined medically, no hardship is entailed.¹ Not only would this preclude the possibility, so far as our knowledge would permit, of having any sort of venereal disease, but it would safeguard the race from other affections.

Doubtless, at first, such an idea may be resented, but when, on reflection, it is considered that it is only to ensure as far as possible healthy children for our country, there is no weighty evidence to be advanced against it. As it is of the utmost importance to the State to have children born into the world vigorous and healthy, it is to be hoped that the time will come when no marriage will be allowed to take place unless sanctioned from a medical point of view.

At the present moment we have no law to prevent any marriage, whatever be the circumstances or religion, in any country. If, in certain cases, there is strong evidence on either side to lead to the opinion that only infants likely to be mentally deficient, tuberculous, or otherwise diseased, can result from the union, then there ought to be no hesitation in asking for the consent of the man or woman, or both, to be sterilized, which would in no way prevent them living an ordinary married life. This would avoid the unhappiness likely to follow an order forbidding a marriage undesirable from every point of view to the State, and would prevent the legalizing of such unions as can never benefit the State.

No laws can ever prevent the advent of illegitimate children, but if legislation were to be passed with reference to marriage, men and women of education would not face the social consequence of running counter to the law, especially as marriage would only be denied in those cases in which sterilization was not agreed to.

It would be difficult to estimate the loss in numbers of idiots and other undesirable individuals which such legislation would produce, but such a lessened number of births would surely be received with satisfaction by those who are anxious to control the birth-rate.

War will go on as long as human passions remain; pestilence may arise at any time, as witness the terrible scourge of influenza which affected practically

¹ It is not suggested that medical examination should exceed that for ordinary life assurance, for which the family physician can give a certificate.

the whole world, carrying away thousands—the deaths in England and Wales, to say nothing of shattered health, amounting to 112,329, only comparable to the mortality of the terrible epidemic of cholera in 1849; and famine must occasionally rear its head, as in the awful visitations of Russia and India. Therefore there need be no fear that there will be any dangerous increase in the population.

It is for the political economist to point out in what manner the population is to be fed, and for statesmen to follow any advice which is sound; but, from our point of view, we have to do our best to secure the birth of vigorous children and everything in our power to make arrangements for their upbringing in such healthy surroundings as will give them the best chance of survival.

Whilst we have no power to interfere with the private lives of individuals, I consider that our profession ought to let it be known that contraceptives and anything which interferes with physiological laws cannot have our approval, and I think that, as regards the Obstetric Section of the Royal Society of Medicine, the expression of opinion of those who took part in the discussion following the paper of Dr. Arthur Giles in May last on sterility makes this clear.

I may say that I am fully aware that such a proposal as mine of a State marriage certificate means an Act of Parliament and probably much opposition. But is there any commendable reason why we should bring into the world so many idiots to be supported by the State or otherwise, especially when the country is taxed almost beyond its capacity for paying, and when we know that there are means at our disposal to prevent their advent?

Dr. Stansfield says that heredity is one of the great factors in the production of our C3 population, and the trend of modern civilization, by its Poor Law system, and by its treatment of the unfit during childhood, tends to foster the growth of this class. The care given to physically and mentally unfit children may reduce the degree of unfitness, but if their improvement is such as to enable them to escape incarceration under the Mental Deficiency Act, they are turned out mental, moral, and physical weaklings, to return to an environment which was associated with the development of their unfitness [22].

According to the seventh annual report of the Board of Control there were over 12,000 registered mental defectives, including criminal and non-criminal.

Certainly my suggestion is more humane than that of Plato, who, whilst not being opposed to marriages, wished that the offspring of the least worthy should not be reared; or of Aristotle, who was in favour of allowing children in excess of those required to die from exposure, and that all deformed children should not be permitted to live.

In conclusion, I may say first, that, as the mortality amongst illegitimate children is so great, it would be an advantage if an effort were made to save some of their lives, and if philanthropic arrangements could be inaugurated for bringing them up in healthy surroundings, so that they might eventually become of service to the State.

Secondly, that looking to the amount of sterility, and the enormous loss of life caused respectively by gonorrhœa and syphilis, it is urgently desirable that notification of these diseases should be rendered compulsory.

Thirdly, that, as I have pointed out, we have a distinct fall in the birth-rate; that by the widespread adoption of contraceptives, which from a physiological point of view cannot be approved, but which we are certain will be ever increasingly used, we must not expect an average of more than two births from each marriage—possibly only one; that, as we know from a most conservative

estimate there must be at least four children per marriage amongst families who can produce children, allowing for infantile mortality, those who never marry, and those who are unfit to produce children, we cannot anticipate a real increase in the population [23]. It follows, therefore, that our country is faced in the future with the problem of race suicide.

And lastly, should my suggestion of the granting of a State certificate of marriage be adopted, we should have the satisfaction of knowing that in future our profession would be the means of helping to compensate for loss of numbers by ensuring for the State the advent of healthy children—the best and surest evidence of a virile race.

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DISCUSSION.

Dr. EDEN said that he was not prepared to admit that a higher birth-rate was necessarily an advantage to the State. He could see no economic advantage in producing more babies than could be properly provided for by their parents, or employed in adult age in the business of the country. England and Belgium were the two most densely-populated countries in the world, and we could not do with such a birth-rate as Hungary had before the war, viz., 44 per 1,000. He thought it probable that a nation would evolve for itself the birth-rate which best suited the conditions of its existence for the time being, and as these conditions varied, the birth-rate would vary also. The present time of economic difficulty seemed an inauspicious moment for efforts to raise the birth-rate. The Great War had put the matter to the test, for the nations in Europe with the highest pre-war birth-rate were now in the sorriest plight of all, while the two nations with the lowest birth-rate, France and England, won the war. Dr. Gibbons had expressed the fear that France was on the way to commit "race suicide." It was, however, incontestable that before the war France was economically one of the strongest nations in the world. There were no poor in France, the visible savings per head of the population were higher than in any other country in the world, the land belonged to the people and was cultivated by small proprietors, and they never had the acute crises of unemployment which occurred in England. While there were many factors in national prosperity, Frenchmen themselves were convinced that the small family was an important factor in the prosperity of their country. The proposal that marriage should not be legal unless the husband and wife had both passed a medical examination filled him with amazement. Was it seriously proposed that all young women as well as young men should be subjected to the detailed internal and external examination which would be required to exclude venereal disease? The duty would, of course, fall on the general practitioner, and of what value would the certificate be? How many cases had they all known where men had married after being pronounced free of gonorrhœa by their family doctor, or even sometimes by an expert, and yet the most disastrous results had followed? And the same difficulty applied to tuberculous lesions. A large proportion of the population carried about old tuberculous deposits, and it was always very difficult to get an expert to say when a tubercular focus was no longer a source of danger.

Yet if the examination did not afford security it became a simple outrage. And even supposing that the certificates could be relied upon, for how long would they remain valid? For the day on which they were written and no longer. Did not married people contract venereal disease and tubercle? And was such an occurrence to be regarded as annulling the marriage? If such a law were ever enacted the immediate result would be to discourage marriage, to encourage irregular unions, and so raise the illegitimacy birth-rate. In his opinion these were ill-considered schemes which could not accomplish their purpose, and might be even productive of much evil.

Dr. AMAND ROUTH agreed with Dr. Gibbons that the main cause of the lowered birth-rate was voluntary limitation of conception which he believed led to much harm in both potential parents. It was unnecessary, for this country had to defend its dependencies all over the world, and the need for immigration into those lands was almost illimitable. The diminishing birth-rate due to voluntary limitation was especially prevalent amongst classes best able to afford children. Bertillon's table proved that to be the case. Malthus' original plan was the postponement of marriage and abstention for varying periods afterwards if necessary. Neo-Malthusianism connoted early marriage, or no marriage, with use of contraceptives to prevent conception. If limitation was necessary the doctor should decide upon its methods. Infantile death-rate had decreased from 150, reaching 80 in 1920, but the improvement had been only in the later months (the last four years' improvement was from 60 to 44 per 1,000 births). No figures were known as regarded deaths during the birth itself, and the neo-natal death-rates had remained stationary. There had been 76,552 infantile deaths in 1920; of these 9,894 infants had died in the first twenty-four hours, and 20,979 in the first week of life. Criminal abortion was increasing—Czecho-Slovakia had 100,000 cases annually according to Wassermann, France 400,000, and Germany 500,000. In the first country a bill had been introduced to authorize doctors to induce abortion up to three months in married women, if desired by the woman, so as to avoid maternal mortality. Syphilis did not impair fertility but led to many ante-natal deaths, especially in cases of "mixed transmission" or where the infection was not rendered latent. Treatment during pregnancy was now known to save the lives of offspring, even when begun at the sixth or seventh month, if infection had been rendered latent. Dr. Gibbons' proposals to check national sterility could only be adopted if pressure of public opinion demanded it. This was true of compulsory notification of syphilis, which was useless unless continuous treatment till cured was simultaneously secured. The question of health certificates before marriage, again, bristled with difficulties, especially as regarded women, and might perhaps be met, as an alternative, by the life insurance of both partners to a marriage, the medical fee for examination to be paid by the State if need be. Voluntary sterilization of the unfit by vasectomy or salpingectomy could not yet be encouraged by the State, and compulsory sterilization, even of imbeciles under State control, was not yet possible. Confidential death certificates, which every other European nation used, would enable more reliable statistics of infantile deaths to be available. This was advised by the Royal Commission on Venereal Diseases and should now be reconsidered. Stillbirths should be registered, not merely notified to Medical Officers of Health, as now, and the cause of the deaths should be investigated by experts.

Dr. ARTHUR GILES said that Dr. Gibbons' paper appeared to him to be very opportune, and most suitable for discussion in this Section, because those concerned with State medicine did not interest themselves overmuch in questions of sterility; it was primarily the general practitioners, and perhaps specially the gynecologists, who were concerned in this matter, because they were the people who were consulted by the women concerned. The advantages and disadvantages of a falling birth-rate were apt to be confused; if only people who could not afford to bring up children controlled their families, little could be urged against such control, but the important point was that this control was chiefly exercised by people who could quite well afford the care of children, and this was a real loss to the State. It was necessary to distinguish between what the State could do and what was dependent upon public opinion. The State could certainly take steps for the care of illegitimate children; these were not responsible for

their illegitimacy, and had as much right as any others to be properly cared for. The State could also legitimize children whose parents married after their birth. Public opinion, on the other hand, could do a great deal in other directions. A better teaching could be spread abroad showing the drawbacks of restricting childbirth, both from the State point of view and also from the point of view of individual private families. His own experience had led him definitely to the conclusion that people who in their early married life took steps to prevent conception lost the chance in many cases of having children later. The question of a certificate before marriage was of great interest, but he thought that State regulations requiring such a certificate would not be practicable. In his monograph on sterility he had touched upon the matter and expressed the view that in a state of more enlightened public opinion parents might well require from men asking their daughters in marriage some evidence from a responsible medical man that they were in a fit state of health to marry, and especially that there was no active venereal disease. Gonorrhœa was undoubtedly responsible for an enormous loss of life to the State. In the monograph referred to he had calculated from the data available that gonorrhœa alone was responsible for the loss of something like 200,000 children.

Dr. LAPHORN SMITH said that while he was in sympathy with Dr. Gibbons' plea for saving the unwanted and unborn children it was doubtful whether we could afford the enormous sums which would be required to do so. With over a million people living in underground cellars and no prospect of houses being built, where would we lodge them if saved? At the most there were only a few thousand of them, among whom there would be many mentally deficient owing to the hardships of the unmarried mothers. He was much more concerned about the two million possible mothers who, by reason of a shortage of two million men, had no hope of ever being married and having a child as long as they remained in Britain. If they were all married they would have on an average three children each, so if it was more population we wanted for national defence it was more important to encourage them to meet an equal number of unmarried Britishers overseas. Their progeny would practically all be fit and would provide in forty years many millions for Imperial needs.

Dr. R. A. GIBBONS (in reply) said that Dr. Eden had misunderstood what he had intended to convey with reference to the examination of women for a State certificate of marriage. He would greatly regret if it were thought that he suggested every girl about to be married should be examined as if at a clinic in a Lock hospital. He was particular to mention that, in his opinion, no hardship was entailed, because every woman desiring life insurance must be medically examined. He agreed that occasionally this might lead to error, but, if so, the family physician would be responsible for it, because his certificate would be accepted. With regard to the remainder of Dr. Eden's remarks, it was evident that he (Dr. Eden) was content to leave matters as they were. Dr. Gibbons agreed with Dr. Amand Routh that compulsory notification of venereal disease was useless, unless the patient could be kept under observation and continuously treated until pronounced well. Compulsory notification had been successful under the new Swedish law; although only in existence since 1918, the incidence of venereal disease per 10,000 of the population throughout Sweden, and especially in Stockholm, had been considerably diminished in 1920. It would be of great advantage to the State and to science if Dr. Amand Routh's suggestion of confidential death certificates of infantile deaths could be adopted. Dr. Gibbons agreed with Dr. Routh that compulsory sterilization was not possible at present, and he considered that Dr. Giles was right in saying that we must distinguish between what the State could do and what was dependent upon public opinion. The hope of Dr. Gibbons was that in time to come, however distant that might be, public opinion would be sufficiently powerful to compel the State to act, and the logical conclusion was that the race would be so healthy that sterilization would practically never have to be done.

Section of Obstetrics and Gynaecology.

President—Professor HENRY BRIGGS, F.R.C.S.

Extension of Carcinoma of the Cervix to the Vagina.

By ARCHIBALD LEITCH, M.D.

(ABSTRACT.)

THE author said that the records of several hundreds of post-mortem examinations of cancer of the cervix showed that the vagina was practically always involved in the spread of the disease, only 2.5 per cent. of cases escaping. If that could be taken as a sure indication of the tendency of cervical cancers during life, as it seemed reasonable to argue, then more attention would have to be directed to the treatment of the vagina in radical operative technique. In Wertheim's operation the removal of a vaginal cuff to enclose the septic cervix partly served this purpose, but it was doubtful whether enough vaginal tissue was generally removed. Apart from post-mortem findings, the presence of cancer cells in the vaginal wall could often be demonstrated in the "vaginal cuff." In discussing the various ways in which the vagina might become infected from the cervical tumour, he insisted that by far the most common mode was by direct and continuous growth of cancer cells from the cervix down the deeper lymphatic vessels of the vagina, so that the superficial malignant ulceration, if it existed, was always less extensive than the deeper lymphatic spread. In this vaginal involvement the possibility of lymphatic emboli could be neglected, because these emboli did not often seem to be formed against the direction of the lymph flow. The older theory of contact transference of cancer from the cervical growth to the posterior wall of the vagina could be ruled out of account because both surfaces were septic and this would constitute an absolute bar to successful grafting. He suggested another possible mode of vaginal infection with cancer that was generally difficult to prove, but could be shown in cases where lymphatic dissemination was demonstrably absent. The successful results following his experiments in the production of cancer by means of tar and other tumour-producing substances threw into relief certain features attaching to such experimental tumours. One of these features was the frequent multiplicity of tumours over part of the area exposed to the action of the agent and the absence of reaction at other parts. He suggested that the same agent, whatever it was, that induced malignant disease of the cervical epithelium might also operate on the less sensitive epithelium of the vagina and there produce a "second primary" neoplastic reaction. He stated that he had occasionally found patches of keratin formation in the vaginal mucosa, localized hyperplasias

of epithelium, and he showed sections from two cases in which the proliferation of the vaginal epithelium had extended so far as to constitute invasive tumours which, if not definitely and beyond all doubt malignant, had certainly progressed beyond a benign stage. In one of these cases there was lymphatic involvement higher up in the deeper layers of a different histological type; in the other, lymphatic spread from the cervix was definitely absent. While not suggesting that this was a common occurrence, he would be glad to learn from gynaecologists any facts pointing in that direction. The microscopic examination of very minute surface growths in the vagina in cases of cancer of the cervix might throw more light on the question.

DISCUSSION.

Dr. FLETCHER SHAW said he thought that Dr. Leitch's hypothesis would probably explain a case of recurrence after Wertheim's hysterectomy, for which he had never been able to find a quite satisfactory explanation. This was the first Wertheim's hysterectomy which he had done and though the operation took a very long time, the patient ultimately made a good recovery; she was lost sight of for some time, but reappeared at the end of five and a quarter years with an epithelioma in the vagina, the upper edge of which was fully $\frac{1}{4}$ in. away from the vaginal scar, so that it could not be looked upon as implantation recurrence in the scar.

Dr. LAPHORN SMITH remarked that he had always regarded these growths of the vagina, where they were in contact with the cervical growth, as evidence of the contagiousness of cancer. He had seen comparatively few, and had ascribed this to his practice of removing badly-damaged cervixes before carcinoma developed in them.

Mr. CLIFFORD WHITE stated that he had seen similar separate vaginal deposits in a case of sarcoma. The patient had been seen six months ago with a large malignant ulcer of the cervix and with nodules in the vagina to within 1 in. of the urethra. The uterus and vagina were removed with difficulty per abdomen. The cervical growth was reported by a competent pathologist to be a sarcoma. The patient was still alive and well.

Dr. FAIRBAIRN asked whether the explanation given by Dr. Leitch as to the origin of these vaginal growths would also cover those strange cases of late recurrence after removal of the original growth. These recurrences commonly occurred in the scar or its immediate neighbourhood, and it was difficult to understand how the cancer cells could lie dormant for five years or more and then suddenly wake up into activity. Would he consider these recurrences as fresh growths arising from persistence of the original irritative cause?

Dr. S. CAMERON stated that on several occasions he had observed ulcers on the vaginal wall at a considerable distance from a cancerous cervix. He thought that ulcers of this description usually arose from permeation followed by breaking down of the overlying vaginal tissue. On account of this tendency to permeation he always practised free excision of the vagina in the radical operation for cancer of the uterus. Dr. Leitch's discovery was interesting and seemed to indicate that the causative factor of cancer might produce the disease in more than one locality almost simultaneously, and if in cancer of the cervix there was even a slight tendency for an independent growth to appear in the vaginal wall, then the entire vagina should be removed.

Dr. LEITCH (in reply) said that vaginal nodules found long after removal of the uterus for cancer of the cervix might possibly be "second primaries," but unless it could be shown microscopically that they were growing from the surface epithelium and were without connexion with involved lymphatics, it would be better to regard them as metastases. Recurrences arising in other directions from long dormant cancer cells were not unknown: they might start a progressive career after ten, fifteen, or more years. What kept their growth in abeyance, and what allowed them to proliferate again, was still unexplained.

Treatment of Uterine Fibroids.

By WILLIAM FLETCHER SHAW, M.D.

At the annual meeting of the British Medical Association in 1914, Dr. A. Donald of Manchester opened a discussion upon the treatment of uterine fibroids. He based his remarks upon a consecutive series of 309 cases upon which he had operated with a mortality of 2.9 per cent., and advocated operation for these cases in preference to X-ray treatment. Professor Karl Gauss, of Fribourg, followed with a plea for X-rays which, he contended, gave quite as good results without any mortality. In the subsequent discussion all the British speakers agreed with Dr. Donald and said that they practised operation rather than radiation in the treatment of these tumours. Such, then, was the opinion of British gynæcologists in 1914.

Since that date many articles have been published advocating the use of X-rays in the treatment of uterine fibroids. One important contribution to this subject was made to this Section by Dr. Eden and Mr. Provis in February, 1921,¹ followed by a full discussion; but, although these communications were made by enthusiasts for this particular treatment, many of them strike the impartial reader as rather half-hearted and all give a long list of conditions which render this method of treatment unsuitable.

The one great risk of operation is thrombosis followed by fatal embolism, and it is the recent occurrence of two cases of this complication in my own practice which has led me to reconsider the subject seriously.

If, after mature consideration, we find the patient runs less risk with X-rays than with operation then our duty to our patients is to advise X-ray treatment rather than an operation. But if, on the other hand, we find that an operation, in spite of this operation mortality, means less risk to the patient than X-ray treatment, it is quite time we said so; otherwise the general public hear only the radiologists' side of the subject, and, soon, patients will refuse to submit to operation: and who can blame them?

The only way by which to form a true estimate of the relative risks of these two methods, is to take a consecutive series of patients with uterine fibroids, note the results of treatment and compare these with a fair estimate of the results which would have been secured by the alternative method.

It is quite useless to pick out a few cases here and there, submit these to one method of treatment and found an opinion for or against this method of treatment on the results obtained in these few picked cases.

For the purposes of this investigation, I have examined the notes of all patients with fibroid uteri upon whom I have operated during the two years 1920-21—138 altogether. During these two years I saw a few additional cases of uterine fibroids, but in these cases the tumours were very small and produced only slight symptoms or none at all; therefore, for these, I did not advise any treatment. So that the 138 cases form a consecutive series of uterine fibroids in which treatment of some kind was required.

The ages of the patients ranged from 30 to 65 years, and eighteen of them had passed the menopause. In the great majority of cases supravaginal hysterectomy was performed, as it is much the shorter and less severe operation, but in twenty-one cases panhysterectomy was carried out, either because

¹ Eden, T. W., and Provis, F. L., "A Record of Seventy-six Cases of Uterine Fibroids and Chronic Metritis treated by X-rays," *Proceedings*, 1921, xiv, pp. 283-295. [Discussion, pp. 295-303.]

the fibroid grew low down and involved the cervix, or because the cervix was badly lacerated and so was removed for fear of subsequent carcinoma, while in nine cases myomectomy only was performed.

(1) MORTALITY.

The number of deaths after operation was four (2·9 per cent.): two from embolism, one from shock within twenty-four hours of the operation, and one from acute nephritis.

My experience of X-rays in the treatment of fibroid uteri is very slight and for the purpose of this investigation I am accepting the statements of its most enthusiastic supporters, and taking for granted that in all uncomplicated cases the hæmorrhage will cease and the tumour diminish in size and there will be no complications from the action of the X-rays. A study of the whole literature on this subject makes one sceptical as to the validity of some of these claims; another point to remember is that further extension of the treatment means treatment carried out by operators with less experience and skill than those who now advocate the method, together with increased numbers of burns and other disasters which are already mentioned by these operators. This argument against X-rays I am not considering in this investigation as the same objection might be urged against operation; we must only consider the best which can be produced on either side. So far, then, as mortality is concerned operation on these 138 cases resulted in four deaths, whereas if X-rays had been used these four deaths would, in all probability, not have occurred.

(2) MALIGNANT DISEASE.

Unfortunately, a microscopical examination was not made of all the specimens removed and it is quite possible some cases of unsuspected malignant disease were overlooked. Amongst those which were examined five specimens of malignant disease of the body were discovered—two of carcinoma, three of sarcoma. All these patients were over 50 years of age and two of them had passed the menopause, so that malignant disease was suspected in these two cases before operation was undertaken. But the other three had not ceased to menstruate, so there was no suspicion of malignant disease and these undoubtedly would have been treated with X-rays if this had been the treatment selected for all uncomplicated cases of fibroid. If these five cases had been treated by X-rays there is little doubt that the growth would have continued, so that systematic treatment of this series with X-rays would have resulted in five deaths from malignant disease alone as against the four deaths by operation.

In addition to these five cases of malignant disease of the body there were three cases of carcinoma of the cervix occurring along with fibroids of the body. No doubt it will be argued that these cases would have been diagnosed beforehand; but two of them were referred to me by doctors because of the fibroids, the carcinoma of the cervix having been overlooked. We can hardly expect better diagnosis from X-ray operators, so the cancer in these three cases would probably have been overlooked had X-rays been recognized as the accepted treatment; and these cases had been referred for this treatment rather than to a gynæcologist for operation.

There was also a case of adeno-myoma which was removed with good results, but with X-rays the condition would have progressed, and probably operation would have been impossible or very difficult by the time the condition had been recognized.

(3) DEGENERATIONS.

These occur very frequently in fibroids, and one of my chief reasons for advising operation, even when the patient has passed the menopause and hæmorrhage has ceased, is the fear of this event occurring at a later date, and so necessitating an operation in more advanced life when the patient is less able to stand the strain.

Amongst the specimens examined there were twenty-one cases of degeneration :—

Fatty degeneration	1
Red degeneration	4
Cystic degeneration	8
Calcareous degeneration	4
Hyaline degeneration	2
Septic infection...	2

If all the specimens had been systematically examined, no doubt a much larger number would have been found to be degenerating, and undoubtedly a number of those fibroids removed would have degenerated in later life.

How often degeneration would occur in later life in uterine fibroids treated with X-rays I cannot say, my personal experience of this treatment being small, but I have seen one such case which necessitated operation six years after the hæmorrhage had been stopped by X-rays, and it may reasonably be supposed that degeneration would occur in these tumours at least as frequently as in those which have atrophied naturally after the menopause.

Degenerative changes are most liable to be set up after the menopause, when the blood supply to these tumours is diminished, and I can see no reason why they should not occur as frequently after the artificial menopause produced by X-rays. In the reports of the large number of cases now being published after X-ray treatment, the recorders do not take this into consideration, as the cases they report are recent. To get the true results these cases should be reviewed ten years later.

Gross degenerative changes produce acute symptoms, but we must also remember that many patients suffer in general health from absorption of products from slighter degrees of degeneration without manifesting acute symptoms. The profound anæmia, quite out of proportion to the amount of hæmorrhage, and the bad general health of many patients with uterine fibroids are probably due to this.

It is impossible to say what would have happened if all these cases of degeneration had been treated with X-rays; I do not contend that all would have ended fatally, though probably some would have done so. In all the articles written on this subject by X-ray workers, degenerating tumours are placed in the category of those which must not be treated with X-rays. The great difficulty is to be sure without operation that a tumour is not degenerating, and the instances must be very few in which a competent gynæcologist will take the full responsibility of saying there is no degeneration present.

(4) INFLAMED APPENDAGES.

This is a complication which precludes treatment by X-rays according to the authorities on this method of treatment. Of course acute inflammation will usually give rise to acute symptoms which are easily recognized, but there are other cases which only come under treatment after the acute symptoms have subsided.

In this series there was only one case which could not be included in one of the other categories, but there were several others with inflamed appendages complicated with degenerating fibroids or with adhesions to rectum or intestines.

(5) ADHESIONS.

Adhesions firmly binding rectum, bladder or intestines to the tumour contra-indicate the use of X-rays; and yet how often do we unexpectedly find these adhesions during an operation. The following case—one of the last in this series—is an instance.

An unmarried woman, aged 49, one year past the menopause, with no previous history of inflammation, was brought to me because of recurring hæmorrhage. Examination revealed the presence of a fibroid uterus about the size of an orange, apparently quite mobile; there were no symptoms or signs suggesting degeneration, and it was just the case which would be referred to the radiologist if this form of treatment were used. I opened the abdomen and found both tubes and ovaries firmly adherent to the back of the uterus, but before I could find the tubes and the uterus, I had to separate the rectum and bladder from these organs and from each other, as they were adherent over the top of the uterus. These adhesions were so firm that the rectum had to be cut off from the uterus and appendages, not stripped, as is usually possible. In all probability these cases with unsuspected adherent rectum and intestines explain the damage to these organs which is occasionally reported after the use of X-rays.

In this series five cases are noted as having adhesions between the uterus and abdominal viscera. Though the number is not very great, the frequent impossibility of diagnosing the presence of adhesions before operation greatly increases their importance.

(6) RETENTION OF URINE.

Seven patients, all about or past the menopause, were operated upon for this condition alone. In each case the uterine fibroid filled the pelvis, and had been allowed to drop into this position through atrophy of the tumour due to the menopause. I cannot say how far X-rays would have still further diminished the size of the tumours and relieved the symptoms, but their application would have taken some little time, and have necessitated catheter treatment in the meanwhile.

We must also bear in mind the action of X-rays upon larger tumours, as the atrophy set up by this treatment may cause a fibroid situate above the brim to descend into the pelvis and set up these acute symptoms.

(7) WRONG DIAGNOSIS.

Every gynæcologist knows the difficulty of making a correct diagnosis in all cases of pelvic tumour. This is recognized by the radiologists who have published their work up to this time, and they all insist upon the necessity of an examination by a gynæcologist before X-ray treatment. I doubt whether this will be so in future. Even if a gynæcologist is consulted, the risk is not entirely eliminated, and I have to confess to two bad mistakes made during the last two months. In both cases I diagnosed uterine fibroids, and fortunately advised operation, and in both cases the solid tumour proved to be carcinoma of the ovary. In both cases the tumour was not adherent, and was removed, so that the patient has a very fair chance of being cured, which she would not have had with X-ray treatment.

(8) CONCLUSION.

From the study of this series of cases and from a perusal of the literature, I can only arrive at the conclusion that the routine treatment of uterine fibroids by X-rays is quite unjustifiable. Apparently a small, uncomplicated, undegenerated fibroid may be made to diminish in size and to cease to bleed; but how seldom can a conscientious gynæcologist say definitely of any particular fibroid: "This is free from adhesions, degeneration, and malignancy, and is not complicated by old inflammatory tubes." Unless such a guarantee can be given, operation holds fewer risks for the patient than radiation.

There are two classes of uterine fibroids only in which I should now consider X-ray treatment—patients with some general disease which makes operation risky (exophthalmic goitre, diabetes, &c.), and patients with profound anæmia from hæmorrhage, though unfortunately many writers consider this class unsuitable for X-ray treatment. This is a subject upon which I am not qualified to give an opinion, as I have had experience of one case only, though the patient did very well.

In this series I had five patients so profoundly anæmic that I feared to operate upon them. Two of these died from embolism and one from shock, although one of them had been in bed for the four weeks preceding the operation. The only other death I had in this series was due to acute nephritis, and probably the operation had nothing to do with it. If the radiologists can treat these few cases of profound anæmia for us, we shall almost eliminate operation mortality.

In the other cases—133 in this series—I feel sure that much the safest treatment is operation.

DISCUSSION.

Dr. S. CAMERON stated that at one time he had studied the pathological changes to which fibromyomatous tumours were liable and, in consequence, he failed to realize how X-rays could prevent degenerative changes or septic infection. He noticed that certain continental electro-therapeutists claimed that malignant disease of the uterus could be cured by the use of X-rays and radium. This had not been his (Dr. Cameron's) experience, and he was of opinion that in the treatment of fibromyomatous tumours hysterectomy should be performed whenever any discomfort was caused by the presence of the neoplasm. X-ray therapy would not safeguard a patient against impaction or acute torsion of a fibromyomatous uterus, and even when the tumour became calcareous danger existed, as the calcified mass might lead to injury of the bladder or gut. A low death-rate was now associated with the operation of hysterectomy and it would become still lower when all operators abandoned silk for catgut.

Dr. ARTHUR GILES said that those who advocated X-ray treatment of fibroids always made the proviso that there were a number of important contra-indications for this method; and probably many operators would increase this list. But even in cases in which there was no obvious contra-indication—for instance, Dr. Fletcher Shaw's theoretical group, in which the cases were guaranteed free from malignancy, degeneration or inflammation—there was still an important objection to X-ray treatment which had not been referred to. It was generally admitted that the chief factor in the arrest of hæmorrhage by X-rays was the atrophic action which they exerted on the ovaries. Years ago, when the mortality of hysterectomy was very high, an alternative operation was introduced, namely, the removal of the ovaries. But as the results of hysterectomy improved, it was soon felt that it was incongruous to remove healthy organs for the sake of saving a diseased one; and the same objection could be urged against X-ray treatment. The risk of hysterectomy at the present time might be considered as being even lower than had been cited. Referring to his own cases, in view of this paper, he

found that his total hysterectomies for fibroids to date were 965, with 17 deaths, a mortality of 1·3 per cent. But excluding the first 300 cases, in which there were 12 deaths, the later results showed 665 cases with 5 deaths, a mortality of 0·75 per cent. It was therefore possible in these days to advise operation with great confidence. Another reason for operation had not been mentioned but was of considerable importance, namely, that in a large number of cases it was possible to do a myomectomy, removing the tumours and leaving the uterus and functional ovaries. He found in his own cases that myomectomies were in the proportion of one to five hysterectomies. There were, of course, definite limitations to the scope of myomectomy, which he need not go into, but, without doubt, myomectomy was the ideal method in the treatment of uterine fibroids.

Dr. LAPHORN SMITH said he had had considerable experience with fibroids of the uterus. He began as assistant to a pioneer abdominal surgeon who, though seldom losing any other case, had a high mortality with his hysterectomies for fibroid, namely, 60 per cent. Very few operators in those days had the courage to publish their death-rate. When he succeeded Professor Trenholme he could not face such a risk, and about that time Apostoli published his remarkable results in curing the symptoms with the carefully-measured galvanic current; he at once started for Paris and spent several months with him. On returning with a very complete outfit he began treating all cases of menorrhagia due to endometritis, as well as to fibroids, with almost uniform success. Of 105 cases of fibroids sent to him during the next ten years, in all but eight the patients were satisfied, and many subsequently had children. At the end of ten years Baer, of Philadelphia, had invented his bloodless operation, tying the six arteries before cutting them. He spent some time with Baer and saw the great drop in the mortality, and decided to abandon the slow but safe electrical treatment for the quick operative procedure. Most of the patients in the eight cases of failure with electricity, were operated on because they could not bear it, and in some cases there was no fibroid at all, the large round mass being made up of cystic ovaries and large pus tubes cemented into one large round mass which had deceived many experts. From what he had seen and heard of the X-ray treatment he would still advise everyone with a fibroid to have it removed by early operation; but if for any reason this was not done, he would strongly recommend Apostoli's method, which had no death-rate and did not sterilize the patients.

Professor HENRY BRIGGS (President) commenting upon the available and reliable evidence, whether from long personal experience or from abundant published literature, maintained that the surgical treatment of uterine fibroids had been established within limits repeatedly and accurately laid down by experienced operators. He recalled a paper read at Ipswich¹ about twenty years ago, by Mr. Harrison Cripps, as an illustration of surgery after very stringent selection.

Dr. FLETCHER SHAW (in reply) said that his plea for operation rather than radiation had been specially strengthened by Dr. Giles, who quoted a long series of hysterectomy for fibroids with a remarkably low operation mortality. His (Dr. Fletcher Shaw's) original reason for strongly advising operation, even when there was no excessive hæmorrhage, was the same as stated by Dr. Cameron, namely, fear of degeneration occurring in these tumours at a later date. He had examined so many of these specimens of degenerating fibroids when Pathologist at St. Mary's Hospital, Manchester, and later had seen so many of them occurring with acute symptoms in old women many years after the menopause, that he felt very strongly that the risk of the occurrence of degeneration with acute symptoms in large fibroids was much greater than the immediate risk of operation, quite apart from the other risks which he had mentioned in his paper, malignancy, adhesions, &c., &c. He had originally undertaken the investigation for his own enlightenment and had conscientiously weighed the advantages and disadvantages of each method of treatment, and it was very satisfactory to find all the speakers were in agreement with his conclusions.

¹ Annual Meeting, British Medical Association, 1900; *Brit. Med. Journ.*, 1900, ii, p. 716.

Chemical Observations on the Toxæmias of Pregnancy.

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(A REPORT TO THE MEDICAL RESEARCH COUNCIL.)¹

IN the following communication we wish to put before the Section the results of certain chemical observations on a series of cases of albuminuria and eclampsia, admitted to the gynæcological wards of St. Thomas's Hospital during the past year. We wish also to thank the staff of the General Lying-in Hospital, York Road, for allowing us access to certain of their patients, an opportunity for which we are especially grateful, owing to the comparative paucity of this type of disease in the London area. The necessity for work on this group of diseases scarcely needs emphasizing. It is not too much to say that we have at present no adequate criteria for estimating the severity of the lesion, and the necessity for such criteria is obvious, when we consider that we are called upon to decide in these patients, whether the pregnancy ought at once to be terminated, in order to avoid risk of permanent damage to the mother, or whether, in the interests of the child, we can safely allow it to continue.

We know from post-mortem findings that the brunt of the lesion, in this class of cases, falls upon the liver and kidneys, and it is the function of these organs that we have investigated from the chemical standpoint, as far as the methods now available allow. Before we proceed further, it is necessary, therefore, to discuss very briefly modern methods of estimating renal and hepatic function. Our methods of examining renal efficiency are comparatively adequate. Put briefly, we have to deal with two main classes of renal defect. In the first of these, the hydræmic or parenchymatous type of nephritis, there is defective elimination of sodium chloride, with resulting œdema; of the existence of this defect, the presence of œdema is sufficient evidence. In the second class of lesion, seen in the azotæmic or interstitial type of nephritis, the excretion of nitrogenous bodies is at fault. To detect the presence of this form of functional failure, we have utilized the estimation of the urea and non-protein nitrogen of the blood, and the urea concentration test of MacLean and de Wesselow. Both functions are, of course, frequently involved simultaneously, producing the mixed type of nephritis so commonly seen in acute forms of the disease. In addition to these methods of examination, we have employed estimations of the blood-pressure, and of the diastase content of the urine. In dealing with the liver we are in a very different position. Though numerous methods of estimating the functional capacity of this organ have been suggested, it cannot be said that any completely satisfactory test is at present available. In our own work we have studied the nitrogen partition of the urine, the ratio of urea to total non-protein nitrogen of the blood, and the lipase and fibrinogen content of the plasma.

The lipase and fibrinogen content of the plasma undergo marked changes in experimental lesions of the liver, a great rise in the lipase, with a fall in the fibrinogen, resulting from such lesions as experimental chloroform necrosis. Whipple has reported a rise in the lipase content in some cases of eclampsia, an observation which, as far as we are aware, has not been confirmed, and a

¹ The Report to the Medical Research Council upon which this paper is based is published in the *Journal of Obstetrics and Gynæcology of the British Empire*, 1922, xxix, pp. 21-47.

marked diminution in the plasma fibrinogen has been noted occasionally in liver lesions in man. On the other hand, the plasma fibrinogen is of interest in view of the observations of Dienst, who states that a rise in the fibrinogen occurs in the toxæmias of pregnancy and believes that this rise plays a definite part in the production of the renal and hepatic lesions.

The nitrogen partition of the urine has been a frequent object of study in liver lesions, in view of the importance of the des-aminating and urea-forming functions of the organ. Observations on this point in the toxæmias of pregnancy date from the work of Stone, and the more elaborate investigations of Wolf and Ewing. Impairment of hepatic function would be expected to show itself in a diminution of the percentage of urinary nitrogen appearing as urea, and an increase in the ammonia and amino-nitrogen. The question is complicated, however, by the fact that considerable variations occur in the proportion of these bodies in the urine, as the result of increase or diminution in the total nitrogenous metabolism, a fall in the percentage of nitrogen appearing as urea accompanying a fall in the total nitrogen excreted, and conversely. In addition, the percentage of urea nitrogen may be diminished as the result of deviation of nitrogen as ammonia, for neutralization purposes, in cases in which acid excretion is in excess, the ammonia nitrogen utilized in this manner being diverted from the metabolic path which ends in urea formation. In drawing conclusions as to liver function from the partition of the urinary nitrogen, the possibility of abnormal acid formation, such as is seen in a ketosis, has therefore to be taken into account before we can attribute abnormalities in the nitrogen distribution to defective liver function. We have limited ourselves to estimation of the total nitrogen, the urea nitrogen and the ammonia nitrogen in the urine of our cases. Since no excess of amino-nitrogen has been demonstrated in the blood or urine of the toxæmias of pregnancy, by those workers who have investigated the point, and since, according to Van Slyke and Stadie, increase of amino-nitrogen has not been demonstrated in the urine of patients suffering from hepatic disease, with the exception of cases of acute yellow atrophy, we have not estimated the amino-nitrogen content of the urine in our patients.

Before embarking on the chemical examination of toxæmic patients it seemed necessary to investigate a certain number of cases of normal pregnancy along the lines indicated above. The data available for normal pregnancy are scanty and to some extent conflicting, and it is difficult to obtain from published work any clear cut idea of a normal standard for this condition.

A complete examination was therefore made of the blood and urine of fourteen normal in-patients, in the last eight weeks of their pregnancies, care being taken to insure a complete twenty-four hours' collection of the urine, and to avoid ammoniacal decomposition. These patients were for three days previous to examination kept on a stock diet containing about 70 gm. of protein. In addition certain observations were made on thirty-four normal out-patients from the ante-natal clinic. The findings in normal pregnancy differ from those in non-pregnant individuals in three respects, the urea content of the blood, the nitrogen partition in the urine, and the fibrinogen content of the plasma.

The blood urea content in the cases examined by us was at a singularly low level, the in-patients showing an average content of 17.5 mgr. urea, and the out-patients only 14.2 mgr. per 100 c.c. of blood. The extreme range of values found in our series was from 10 to 26 mgr., and in only three instances did the blood urea reach or exceed 20 mgr. On the other hand in ten healthy

female out-patients examined as controls, the blood urea varied from 20 to 27 mgr., and in no case fell below the former figure.

It is not necessary to discuss in detail the figure for the blood urea in healthy non-pregnant individuals. It may be noted, however, that in 179 normal individuals between the ages of 20 and 40 years, examined by Feigl, seventeen only showed a blood urea content below 18 mgr., and that in twenty-nine healthy persons, examined by Addis and Watanabe, the blood urea exceeded 20 mgr. with two exceptions. In the latter series, the patients had been kept for three days previous to the bleeding, on a diet containing 75 gm. of protein per diem, and therefore corresponded fairly closely with our own in-patients as regards dietetic conditions. It appears, then, that it is exceptional to find the blood urea at a lower level than 20 mgr. per 100 c.c. in the non-pregnant individual, while in the cases of pregnancy examined by us the blood urea was almost invariably below this figure.

Only two large series of figures for the blood urea in pregnancy have, as far as we are aware, been published. Folin, in 100 cases, gives figures closely corresponding to our own, and comments on the low level of the blood urea in this condition. Caldwell and Lyle, on the other hand, investigating a series of 150 cases at all stages of pregnancy, found an average content of 24 mgr. The figures published by the latter authors show a wide variation, and include many instances of a urea content below 20 mgr. While, therefore, it cannot be said that any definite standard has been established for the blood urea value in pregnancy, it is at least certain that in many instances an abnormally low figure is found.

A possible explanation of this fact can only be arrived at by considering the conditions upon which the level of the blood urea depends. Urea being a readily diffusible substance, its concentration in the blood will be determined by three factors—the amount produced, the rate of excretion, and the total amount of the body fluids. The importance of the last factor lies in the fact that urea, being, as already stated, highly diffusible, tends to distribute itself at an approximately equal concentration throughout the fluid content of the body.

Folin, in explanation of the low blood ureas which he found in pregnant individuals, suggested that in pregnancy the kidney is abnormally sensitive to waste products, and that the low blood urea is due to increased excretion. The results of the urea concentration test in our pregnant patients did not suggest that any such abnormal renal sensitiveness is present, since in the majority of cases the result fell between 3 and 4 per cent., the figure usually seen in young healthy individuals. In addition, the fact that the blood urea still remained at a low level in some of our cases of albuminuria, in which the concentrating power of the kidney, as judged by the concentration test, was definitely lowered, appears to negative Folin's hypothesis.

We are left, then, with two possible explanations, a diminished production of urea, or an increased dilution of the urea when formed. Apart from the fact that the specific gravity of the blood is lowered in pregnancy, and that the content of the amniotic sac is added to the fluid reserve of the body, we have no evidence that dilution is a cause. In cases of pure parenchymatous nephritis with massive œdema and effusions into the serous sacs, the blood urea is sometimes found to be at a level below 20 mgr. per 100 c.c., but even in extreme examples of the condition, the blood urea remains at a level above that frequently seen in cases of normal pregnancy. It seems probable therefore that diminished production of urea is a more important factor. The infor-

mation available as to the effect of altered levels of nitrogenous metabolism upon the blood urea is scanty. Folin, Denis and Seymour working on cases of chronic interstitial nephritis were able to raise or lower the urea content of the blood by corresponding alterations in the nitrogenous ingestion. The point has also been recently investigated in healthy individuals by Addis and Watanabe. These authors have shown that it is possible to increase or diminish the blood urea by varying the protein of the diet. With a daily urinary excretion of 5 to 7 gm. of urea, the blood urea was reduced in three days to the low level of 12 to 15 mgr. per 100 c.c., while on increasing the protein of the diet and the urinary urea to 30 to 50 gm., the blood urea rose to over 40 mgr. The blood urea content is therefore directly proportional to the total nitrogenous metabolism.

In pregnancy, we are of course dealing with a condition in which a considerable deviation of the ingested nitrogen is constantly taking place. Nitrogen which under normal conditions would be utilized in the exogenous protein metabolism of the mother and which would eventually be excreted as urea is diverted to the needs of new tissue construction. In metabolism experiments in which this point has been studied the average daily retention of nitrogen during the latter half of pregnancy amounts to about 2 gm. (corresponding to $12\frac{1}{2}$ gm. of protein), though this figure is frequently exceeded during the later months. Nitrogen utilized for this purpose has of course to be subtracted from the protein nitrogen available for the maternal metabolism. We are at present completely ignorant of the average protein consumption of the working-class woman of this country during her pregnancy. In Hofström's case, in which the patient was given a free choice of diet, the protein consumed daily amounted to about 80 gm., a moderate ration; von Winckel has recommended a daily protein allowance of 70 gm. as a suitable ration in pregnancy.

In our own in-patient cases the average nitrogen output in the urine amounted to 8.3 gm. per diem, corresponding to a maternal metabolism of about 50 gm. of protein. Since in normal individuals investigated by Addis and Watanabe, the blood urea in the great majority of cases varied from 20 to 28 mgr. on a diet containing 70 gm. of protein, a lower blood urea would be expected in these cases of pregnancy which were excreting only 8 gm. of nitrogen per diem. If in these in-patients the low figure found for the blood urea is to be attributed to a low nitrogenous metabolism, due to deviation of nitrogen to foetal needs, the out-patients examined must have been taking a considerably smaller amount of protein per diem, than were the cases under observation in the ward. In a few cases, after a preliminary estimation of the blood urea, an increased protein diet was given for three days and the blood urea again estimated. In these patients who showed a definite increase in the urinary nitrogen on the day of the second observation a slight rise was found in the blood urea. Owing to shortage of beds it was unfortunately impossible to conduct prolonged experiments along these lines, but the fact that the blood urea of the pregnant woman appears to react as does that of the normal individual to an increase in diet, suggests that the differences found in the level of the blood urea in pregnancy by different observers, may be due to the dietetic factor.

The second point in which the blood of pregnancy shows a striking variation from the normal is in the plasma fibrinogen content. In the normal female the plasma fibrinogen content expressed as nitrogen approximates to 40 mgr. per 100 c.c. In thirty-four cases of normal pregnancy the average

fibrinogen content amounted to 76 mgr., or nearly double the normal figure. No constant connexion was noted between the stage of pregnancy and the height of the fibrinogen content, though on the whole the fibrinogen was higher in the later than in the earlier months.

Lastly, the nitrogen of the urine showed a partition which is usually associated with a comparatively low protein diet, the urea in our in-patient cases invariably constituting less than 80 per cent. of the total nitrogen present, and the ammonia nitrogen being slightly on the high side. In a few urines, which were obtained by catheter from patients actually in labour, with a view to controlling any cases of intra-partum eclampsia that might be seen, marked deviations from the normal were frequently noted. The urea nitrogen tended to fall to a still lower level (in one case to 52 per cent. only of the total nitrogen), and at the same time an increase was usually found in the percentage of urinary nitrogen appearing as ammonia. In one case, which was followed at intervals throughout a short labour, the urea showed a steady fall in successive specimens, while the ammonia showed a slight but constant increase. The net result being that at end of the labour a marked fall had taken place in the urea, a slight rise in the ammonia, and a considerable rise in the undetermined nitrogen. This phenomenon appears to be in part, at least, due to a ketosis, since acetone is frequently present in the urine of labour. In part, it may be the result of a period of comparative starvation, supervening in an organism which has for some time been in a condition of low protein metabolism. In any case the fact that such changes may occur in the urine during apparently normal labours suggests the need of caution in drawing conclusions from the nitrogen partition when dealing with the toxæmias. It was noticed also that in catheter specimens obtained towards the end of labour protein was frequently present, either in traces or in definite amounts. Such an albuminuria appears to us to be more rationally explained as the consequence of the muscular exertion involved in labour than as the result of a purely theoretical toxæmia.

Such, briefly, are the main features which, as far as we have examined the matter, differentiate the chemical picture in the pregnant from the non-pregnant individual. They appear to us to suggest an organism in a condition of comparatively low nitrogenous metabolism, owing probably, in the class with which we have been dealing, to the increased nitrogen demands for foetal and maternal tissue construction not being met by a corresponding increase in the maternal diet. Such a lowered protein metabolism is evidenced by the nitrogen distribution in urine, and in all probability by the low content of urea in the blood. It does not, of course, imply a condition of starvation or malnutrition, but merely that the increased demands made upon the organism in pregnancy are not met in one respect, the protein, by an equivalent increase in the diet. The work of Hindhede has shown that nitrogenous equilibrium is possible for prolonged periods on far lower nitrogen rations than those noted above. It seems to us, however, that this lowered protein metabolism must be considered in the interpretation of the chemical findings in the toxæmias, to which we now turn.

In considering the chemical findings in the toxæmias of the later months of pregnancy, the chief question that we have asked ourselves is, what value have such methods of investigation in the clinical management of the case, and what indications do they give us as to the necessity of induction? Our material has unfortunately been small—twenty-one cases of albuminuria and six cases of eclampsia—and our conclusions must therefore be guarded: but

we hope to be able to suggest certain lines along which these cases may be investigated; also certain provisional criteria for induction.

We may preface our remarks by stating that, while from a practical point of view the investigation of liver function along the lines outlined above has proved valueless, we believe that useful information as to the condition of the case may be obtained by examination of the functional capacity of the kidney by the methods suggested. The question of the type of the renal lesion in these cases presents certain difficulties. Edema is a recognized feature of the toxæmias of pregnancy, and, with the exception of a few of the milder cases, was present, often to a marked extent, in all the patients that we have had under observation. It may, therefore, be assumed that the capacity of the kidney for excreting chlorides is impaired, and that the type of nephritis, known as parenchymatous or hydræmic, is present.

On the other hand, marked accumulation of nitrogenous waste products in the blood is exceptional, and this absence of an increase in the non-protein nitrogen of the blood is regarded by some authors as a distinctive feature between the true toxæmias and cases of nephritis complicated by pregnancy. Looked at from this point of view, the renal lesion of the toxæmias would be regarded as a pure parenchymatous nephritis—the capacity of the kidney for excreting the nitrogenous waste products being unimpaired. It must, however, be recognized that an increase in the urea and non-protein nitrogen of the blood is not a *necessary* result of a failure of renal function of the azotæmic type of nephritis—nor, when present, is it to be regarded as an exact measure of such failure. The actual figure reached by these waste products in the blood is dependent on three factors—the total daily nitrogenous metabolism and the gravity and duration of the renal lesion. Folin and his collaborators have shown that in cases of chronic interstitial nephritis the height of the non-protein nitrogen of the blood is largely determined by the amount of nitrogen ingested. That the gravity of the renal lesion is another factor is obvious, and it is also clear that time is needed for an accumulation of waste products to take place, since it is only the excess of nitrogen metabolized per diem over the amount that can be excreted daily that accumulates—and even in very severe cases considerable amounts of nitrogen continue to be eliminated. Moderate degrees of damage will not, therefore, make themselves apparent by increase in the urea and non-protein nitrogen of the blood, but can only be detected by the use of other methods of examination. Such methods cannot be discussed here, but we have employed the urea concentration test for this purpose in preference to the co-efficient of Ambard and its variants. By submitting the kidney to the strain of the ingested urea, degrees of functional impairment can be demonstrated which cannot be detected by an estimation of the blood urea content. The test appears to be especially valuable in the class of case under consideration, since the conditions present are not such as would be expected to lead to large accumulations of urea in the blood. In the first place, as we have suggested above, the pregnant organism appears to be in a condition of low nitrogenous metabolism, owing to the demands of the fœtus upon the maternal protein ration. In the second place the disease is not usually of long standing, and, lastly, the degree of impairment of functional capacity, though definite, is not usually of the severest type. Under these conditions a marked rise in the blood-content of urea and non-protein nitrogen is scarcely to be expected, and it was only found in three of our twenty-seven cases—the highest figure noted being 91 mgr. Taking the albuminuric cases as a whole, the blood-urea was undoubtedly in excess of that seen in our cases of normal

pregnancy, averaging in twelve mild cases 21 mgr., in nine severe 38 mgr., and in six eclamptics 44 mgr., per 100 c.c. Such figures could certainly not be regarded as abnormal in the healthy non-pregnant woman, but in pregnancy, as contrasted with our control cases, they appear to be definitely above the normal level. On the other hand, if we take the urea concentration test as our measure of the functional damage present, we find that in the normal patients the concentration reached attained 3 per cent. or upwards, except in one case in which the figure found was 2'85. In the albuminuric cases the figure of 3 per cent. was only reached in four instances. In three of these patients no other evidence of a grave lesion was forthcoming, and albumin was only present in the urine in traces. In the fourth, with a blood-pressure of 185 mm. Hg and a high-grade albuminuria, no foetal movements had been felt for eight days, and the foetal heart was inaudible. A steady fall of blood-pressure in the course of a week to 150 mm. Hg and the delivery of a macerated foetus led to the conclusion that the patient when first seen was undergoing a spontaneous cure as the result of foetal death. With these exceptions the urea concentration following a dose of 15 gm. invariably showed a lower figure than that obtained in normal cases, and frequently fell below 2 per cent. By this test, then, in contradistinction to the blood-urea, definite evidence of a failure to excrete the nitrogenous waste products appears to be forthcoming, and the conclusion appears to be justified that, in spite of the frequent absence of a rise in the nitrogenous waste products of the blood, the renal lesion is of a mixed type, both chloride and nitrogen excreting functions being involved.

The diastatic index of the urine was found to be definitely lowered in only two of our cases. In one of these the preceding pregnancy had terminated in eclampsia, and in the other symptoms had been present throughout pregnancy. In both cases, then, there was reason to suspect a comparatively chronic type of renal lesion, and in both instances recovery after delivery was incomplete. In acute renal lesions of the mixed type the diastatic content of the urine is not, in the early stages at least, abnormally low, and a low diastatic index would not therefore be expected in these cases of toxæmia. We have only seen one instance of the increase in urinary diastase first described by Corbett, and recently investigated by Mackenzie Wallis. In this case a diastatic content of 100 units was found in a catheter specimen obtained during eclamptic fits. The same specimen of urine also showed the fall in the urea-nitrogen percentage with the increase in the undetermined nitrogen that has been regarded as characteristic of an impairment of liver function. The same type of urinary nitrogen distribution was also found to be associated with a high diastatic index in a specimen of urine from a foetal case of acute yellow atrophy.

After delivery the usual sequence of events appeared to be a rise in the urea concentrating power of the kidney proceeding *pari passu* with the other evidences of clinical improvement. This rise may be exceedingly rapid, and a normal concentration figure was frequently reached before discharge. Thus three of our six eclamptics concentrated to 3 per cent. or upwards before leaving the hospital. In a certain proportion of cases the power of concentrating urea was not restored to a normal figure while they were under observation, and a fall in the diastatic index to a subnormal level is a not infrequent event after delivery. It is possible that in this latter class of cases we are dealing with the onset of a chronic lesion.

The liver findings, as we remarked above, have been unsatisfactory. The

lipase content of the plasma, with the exception of one case of eclampsia in which a slight rise was observed, was within normal limits.

The fibrinogen figures to some extent bear out the contention of Dienst that a rise in the plasma fibrinogen is a feature of the toxæmias of pregnancy. Expressed in milligrams of nitrogen per 100 c.c. of plasma the figure for thirty-four cases of normal pregnancy was 76 mgr., for twelve mild cases of toxæmia 87 mgr., and for fifteen severe toxæmias and eclamptics 101 mgr. Though the averages obtained are suggestive, the rise was not a constant phenomenon in individual cases, and it does not appear that it would have been a trustworthy guide in estimating the severity of the lesion in particular instances. In addition, it must be remembered that a much more marked rise in the fibrinogen content of the plasma, than any seen in the toxæmias, is frequently present in cases of pregnancy complicated by infection. Thus in three cases of *Bacillus coli* pyelitis complicating pregnancy we found 175, 144, and 126 mgr. of fibrinogen nitrogen per 100 c.c. of plasma. The difficulty of excluding the presence of such infections is obvious. It appears probable also from cases which we have followed that an increase in the plasma fibrinogen may be masked by the dilution of the blood which accompanies œdema. It does not appear to us, therefore, that estimations of fibrinogen are likely to prove of any great value in the clinical management and prognosis of these cases.

The determination of the nitrogen partition in the urine has also, from a clinical standpoint, given disappointing results. We have only observed two cases in which a distribution of the urinary nitrogen was found, which could not be paralleled in our normal series. In the first of these, an eclamptic, specimens obtained both before and after Cæsarean section showed an altogether abnormal nitrogen partition. The urea nitrogen amounted to 32 and 35 per cent. only of the urinary nitrogen, the ammonia to 14 and 20 per cent., leaving 50 per cent. of the total nitrogen unaccounted for. No acetone bodies were present. A normal ratio was re-established on the following day. In the other case, an albuminuric, first seen in labour, an almost exactly similar distribution was found in a specimen obtained at the conclusion of labour. Traces of acetone were present. A specimen obtained from the same patient six hours previously had shown a normal partition, and apart from the urinary analysis, nothing was noted which would suggest that the case was of an exceptionally severe type. Looking at the question of the urinary nitrogen distribution from the clinical standpoint, it does not appear that the analysis of the urine will in this class of case afford any useful prognostic indications, since a marked feature of the phenomenon is its sudden appearance and evanescent character. From the theoretical point of view the urinary partition in such cases resembles that seen in severe liver lesions, for instance, in acute yellow atrophy, but it seems to us difficult in view of the transitory nature of the phenomenon that such extensive damage to the liver can have in fact, been present.

The distribution of the non-protein nitrogen of the blood also did not afford us evidence of a failure of hepatic function. In a recent paper Killian and Sherwin have attempted to divide the toxæmias into hepatic and nephritic types on the grounds of difference in the ratio of urea N to total non-protein N of the blood. According to these authors a low urea ratio is indicative of hepatic, a high urea ratio of nephritic lesions. Caldwell and Lyle, in a paper appearing in the same number of the same journal, arrive at an entirely opposite conclusion. In particular they instance a case in which, post mortem, the typical hepatic lesions of eclampsia were present, but in which the blood

showed the highest urea ratio of any in their series. In our own cases the tendency has been for any rise in the non-protein nitrogen of the blood to be accompanied by an increase in the proportion of the total non-protein nitrogen appearing as urea. This is the usual finding in cases of nephritis in which retention occurs, and in marked retention it is not unusual to find that the urea nitrogen constitutes 80 per cent. and upwards of the total non-protein nitrogen instead of a normal 50 per cent.

Observations therefore directed to the detection of functional failure of the liver have not yielded us any information of value from a clinical standpoint. With our present methods, and with the knowledge at present available it is the study of renal function which in these cases of toxæmia in the later months of pregnancy appears to offer the most valuable information as to the severity and progress of the disease. Our material is at present small, and deductions drawn from our findings must therefore be provisional, but the routine examination which we would suggest hinges on the estimation of the blood-urea, the urea concentration test and the determination of the blood-pressure. We are inclined to regard a blood-urea content above 40 mgr. per 100 c.c., in conjunction with a blood-pressure above 180 mm. Hg, as evidence of a severe type of case and indications for induction in the interests of the mother. When the blood urea is below 40 mgr., we would suggest the employment of the urea concentration test. A concentration below 2 per cent. to this test also suggests a grave lesion. If on repetition of the examination after a few days the blood urea and blood-pressure have risen and the concentration fallen, the lesion is progressive and induction would appear to be indicated if permanent damage to the maternal kidney is to be avoided. A low diastatic index is suggestive of disease of some standing and of an eventual incomplete recovery. The fibrinogen and lipase content of the plasma and the nitrogen partition in the urine, though interesting in themselves, do not appear to be of much clinical significance. Convulsions may supervene in cases in which the blood-urea is below 40 mgr., and on the lines on which we have worked we have not found any distinctive features in the cases in which eclamptic fits have occurred. The gradation from the mild albuminuria to the eclampsia appeared to be complete, but speaking generally it was in the cases with the more severely damaged kidneys that fits were liable to occur.

DISCUSSION.

Dr. HERBERT WILLIAMSON said that reliable observations, like those of the authors, upon the bio-chemistry of normal pregnant women had long been needed, and this series was of particular value because it comprised two groups of cases, viz., patients who were leading an every-day life upon an ordinary diet, and patients who were kept at rest upon a fixed diet containing 70 gm. of protein. The results obtained showed that in normal pregnancy there were found: (1) a low blood-urea content; (2) certain differences in the nitrogen partition of the urine; (3) an increase in the fibrinogen of the blood. The observations upon the blood-urea were of special value because they showed that when dealing with pregnant women they must take a lower standard as the normal and regard anything above 0.02 per cent. with suspicion. He asked the authors: (1) Was there any difference in the urea concentration power of the kidney between gravid and non-gravid women? and (2) was there any considerable retention of chlorides during pregnancy? Turning to the cases of albuminuria he thought it important to remember that among them there were only six eclamptics, that these appeared to have been of a mild grade and none of them exhibited symptoms suggestive of a grave liver lesion. He could recall cases with a very different clinical picture, epigastric pain, vomiting, tenderness over the liver, alterations in the area of liver dullness, jaundice and

occasionally tyrosine in the urine, and in his experience the prognosis in these hepatic cases was grave. The authors happened to have struck upon a series in which the liver had suffered very little; had their series been larger he thought their results might have been different. He hoped the paper would not have the effect of discouraging investigation of the lipase content of the blood, and the non-protein nitrogen partition. The authors had rendered a service in demonstrating the danger of basing deductions upon a single observation of the urine, particularly if passed during labour, and had shown that during a normal labour the nitrogen partition of the urine might be completely upset. The value of the diastase test could not be regarded as settled. Among his patients the highest diastase values had been found in cases of toxic vomiting; he had recently seen a patient whose urine contained no albumin, but 2,000 units of diastase. Thirty-six hours later, however, the urine contained a heavy cloud of albumin. He believed that a rise in the diastase often preceded the albuminuria, and that as the renal function became seriously impaired the diastase output fell.

Mr. FRANK COOK said that the authors had presented fundamental data in place of the usual hypotheses and statistics. Had they made any observations, apart from nitrogen partition, bearing on the acidosis factor, as this might acquire a renewed interest in the light of recent scientific methods? They had accepted the salt retention theory of œdema, which was by no means beyond criticism.

Professor H. BRIGGS (President) said that he realized that the speedy publication of the observations of the bio-chemists, followed as it was by speedy corrections, was as creditable to them as it was welcome and valuable to economy and co-operation in research.

Dr. DE WESSELOW and Mr. WYATT (in reply) stated that they had found no difference in the urea concentration power of the kidney between gravid and non-gravid women. There was no evidence of an increase in the power of urea concentration during pregnancy. They had not made any examination of the chloride balance in pregnancy. They recognized that their series of cases was small and that any conclusions must therefore be provisional. The recent literature did not suggest that results of any value were likely to be obtained from a study of the acidosis factor in the toxæmias. The retention of bicarbonate which had been noted in the toxæmias could be attributed to the renal lesion with resulting defective excretion, and estimations of the alkali reserve of the plasma did not appear to have given uniform results.

Section of Obstetrics and Gynæcology.

President—Professor HENRY BRIGGS, F.R.C.S.

A Specimen of a Bleeding Uterus.

Shown by Professor HENRY BRIGGS, F.R.C.S. (President).

THE uterus weighed $4\frac{1}{4}$ oz. The patient suffered from extreme anæmia, due to hæmorrhage. She was aged 48, had only had one pregnancy, which went to term seventeen years and ten months ago. When seen on April 11, 1922, there had been considerable daily blood loss for over six weeks. The patient had had three similar bouts of uterine bleeding, the last of these ending on December 17, when the uterus was curetted and thickened endometrium removed.

The specimen was removed by the vaginal route on April 15.

A complete section of the uterine wall does not demonstrate inflammatory tissue change or new growth.

DISCUSSION.

The specimen was discussed by Dr. HERBERT WILLIAMSON, who said he now treated such cases by X-rays after curetting for diagnostic purposes; and by Mr. FORSDIKE and Dr. HEDLEY, who said they introduced radium into the uterine cavity after dilatation and curetting.

Dr. GILES questioned the wisdom of destroying healthy ovaries for the sake of saving an unhealthy uterus.

The PRESIDENT (in reply) said that the expressed views of the Section on his specimen had proved to be as he expected: they were not yet fixed against a radical operation in advanced cases near the climacteric period.

Description of an Operation for Radiation of Iliac Glands and Deep Tissues in Cases of Carcinoma of the Cervix.

By MALCOLM DONALDSON, M.B.

ALL of us hope, and some of us believe, that if radium is put near enough to carcinoma cells it has a selective action, and will permanently destroy them. The difficulty in the case of carcinoma of the cervix that has been encountered up to the present is that of applying the radium to the growing edge of the growth, and to the metastases in the iliac glands.

Professor Daels, of Ghent, has invented an operation for putting radium deep down in the pelvis near the iliac glands. This I am about to describe, but having only performed it five times I shall only discuss the technique.

[May 4, 1922.]

When I went over to Belgium to see Professor Daels do this operation he told me he had performed it on forty patients, with complications in only four cases. These complications consisted of sepsis in two cases and secondary hæmorrhage with fatal results in two cases. I will say a word or two about these after describing the technique. It must be remembered that this operation should be performed after the cervix has been thoroughly treated.

TECHNIQUE OF THE OPERATION.

The patient is prepared for operation in the usual manner, and a general or spinal anæsthetic administered. She is then put into the lithotomy position, and an incision 4 cm. long is made just to the inner side of the ischial tuberosity. This incision is carried down to the deep fascia and the finger is inserted into the wound. Then, by a blunt dissection, it is carried along the surface of the obturator internus to the inner side of the ischial spine. From the ischial spine the finger is pushed between the two parts of the levator ani, i.e., between the ilio-coccygeus and the ischio-coccygeus, over the surface of the pyriformis, and it reaches the sacro-iliac synchondrosis.

The curved tube (which I have handed round for demonstration) is then pushed along the finger to the same point, and the distance between the skin and the synchondrosis is now measured on the scale at the side of the tube, the latter being held in position by an assistant.

An incision is then made 2 cm. above and just to the inner side of the anterior superior spine. This is carried down through the aponeurosis of the external oblique. The finger is inserted and by means of blunt dissection through the internal oblique and transversalis muscle and *outside* the peritoneum the ileo-psoas muscle is reached. Passing over this muscle the finger feels the sacro-iliac synchondrosis, and the point of the tube with the large vessels lying to the *inner* side.

The finger is then withdrawn, the point of the tube following, and the complete distance from incision to incision is measured on the curved tube. The difference between the first and the second measurement will give the distance from the iliac incision to the sacro-iliac synchondrosis just below the vessels. This is an important measurement, as it denotes the first position in which the radium will be placed. Usually this point is at a depth of 10 cm. from the skin. This is measured off from the radium along the chain, and a safety-pin placed through the chain to act as a stop and prevent the radium from moving, as will be seen in fig. 1. A further safety-pin is put in 4 cm. more distal from the radium, and yet a third pin 4 cm. away from the second.

The cap of the tube having been removed, the chain is now drawn through the tube and the tube itself withdrawn from the pelvis, leaving the chain with the radium attached in the first position.

When the first safety-pin reaches the skin the radium should be opposite the sacro-iliac synchondrosis. Another safety-pin is then placed just outside the ischial incision, to prevent upward movement.

At the end of twelve hours the first of the upper safety-pins is removed, and the chain, which has been wrapped in sterile gauze, is pulled down a distance of 4 cm. (see fig. 2). At the end of another twelve hours the second pin is withdrawn, and the chain pulled down another 4 cm. The radium is left in this third position for twenty-four hours, and then withdrawn altogether. After the withdrawal of the radium a small drainage tube is inserted into the lower wound for three or four days. This is to prevent any accumulation of serum or blood, which was the cause of sepsis in the complicated cases mentioned by

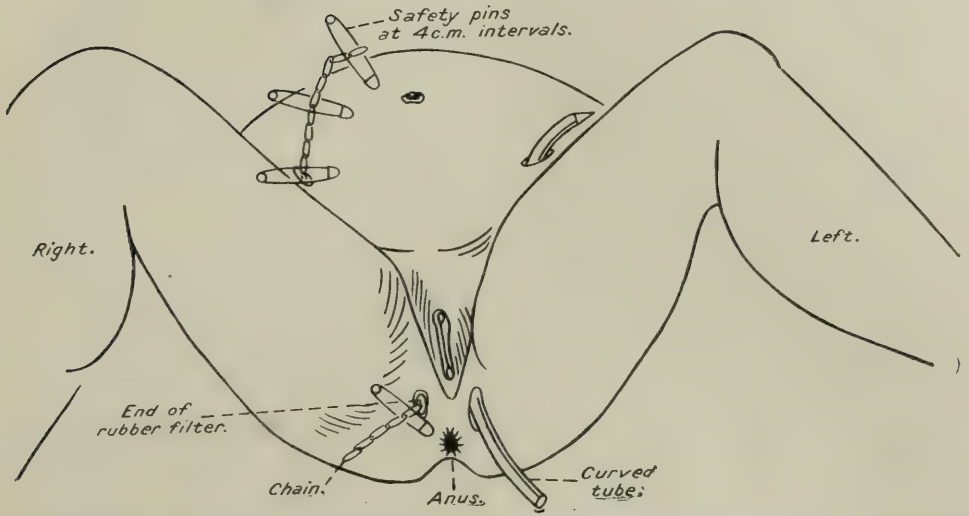


FIG. 1.—On the left of the patient the tube is in position ready to receive the chain. On the right of the patient the tube has been removed and the chain pulled into position.



FIG. 2.

Daels. The other complications—viz., secondary hæmorrhage—occurred, I understand, three weeks and two months respectively after the operation. In both cases it was at the time of a further dose of radium being applied to the cervix; and the explanation given by Professor Daels is that a certain amount of injury is done to the vessels, and that the injured spot is strengthened by young fibrous tissue. The final dose of radium, however, to the cervix, he considers, destroys this young fibrous tissue, and in this way causes a secondary hæmorrhage. When it is remembered that all these cases are inoperable carcinoma of the cervix the risk of these complications in the hope of allowing the radium to get at the new growth is a justifiable one.

DOSAGE OF RADIUM.

At the present time it is impossible to dogmatize on what should be the dose. The dose used by Professor Daels is 25 mgr. of radium element on each side of the pelvis, heavily filtered with aluminium, platinum and rubber, and he uses it for ten hours in the first position and twenty-four hours in each of the other positions. The dose used in the cases I have treated in this way was approximately 50 mgr. radium element on each side of the pelvis, filtered with platinum and rubber. Obviously it would be ideal to have sufficient radium to give the dose in the three different positions at one and the same time.

Of course, as mentioned above, this operation and method of treatment is only secondary to putting in radium in the form of needles, &c., into and around the cervix itself.

I have not discussed the question of treatment of the local cervical growth by radium to-night, as I merely wished to bring to your notice Professor Daels' operation. I think this operation opens up a new field of possibilities for treatment by radium of inoperable cases of carcinoma of the cervix.

BRIEF NOTE ON THE CASES.

Before concluding this short article I will add that four of the five cases upon which I operated had practically no reaction at all, and are very well so far as the immediate results of the operation are concerned. The fifth operation was an attempt in a patient on whom I had previously done a Wertheim hysterectomy, but in whom I fear there is some recurrence. I was anxious to discover whether Daels' operation was possible in such a case, but found that the vessels were too adherent to the side wall of the pelvis to allow the tube to pass to the outer side. In this case therefore radium (52.2 mgr. radium element) was placed in the lower wound as high up as the finger could reach. In this patient, owing to a misunderstanding, no drainage-tube was inserted after the removal of the radium, with the result that the patient had a high temperature for three or four days, but she is now quite well and has left hospital.

DISCUSSION.

Mr. MALCOLM said that the conduct of these cases presented the greatest difficulty to the gynecologist until radiation and X-ray treatment had been introduced. If the results of these methods were persistently good a great advance would have been made. If any doubt of this arose it should not be forgotten that the results of operation in the most favorable cases were amongst the best in surgery.

Professor HENRY BRIGGS (President) said that where pelvic fixation was advanced lumbar glands, in his experience, were usually infected: as to this Mr. Donaldson's results would prove of great value.

Surgical Shock.

By JOHN D. MALCOLM, F.R.C.S.Ed.

THE expression "surgical shock" is applied in this paper to a condition caused by a stimulation of sensory nerves. It is not intended to indicate so much an association with surgery, as that there is an absence of hæmorrhage, crushing of tissue, septic infection and other complications which are apt to accompany accidental injuries. A study of shock during a surgical operation has the further advantage that the changes develop so slowly that they can be analysed, whereas if an uncomplicated stimulation of sensory nerves arises from an accident, the symptoms often progress with such extreme rapidity that their sequence cannot be traced. There is, of course, no reasonable doubt that the conditions are essentially the same whether they arise gradually or immediately.

When I first had the opportunity of studying the surgery of the abdomen, frequent tappings of ovarian cysts and consequent peritoneal adhesions often necessitated prolonged operative treatment. Irritating lotions were freely used and a chilling carbolic spray played upon the abdomen throughout the operation. Symptoms of shock were then common. This condition in a well-marked form is comparatively seldom seen, as a result of modern surgery. The changes may, however, still be detected. They want more looking for, but a constant watching of the peripheral pulse will show them from time to time. They should be looked for before serious trouble develops. If the pulse at the wrist is examined because danger arises, the opportunity for studying the vascular changes in their beginnings has generally passed away.

When a state of shock occurs, the pulse may be quick and feeble from the first, in which case an important change cannot be detected. But in a considerable proportion of operations, chiefly in those performed upon patients with a strong heart muscle, the pulse at first does not hasten, and sometimes it becomes larger and more tense at the wrist. Later the radial pulse, still sometimes remaining slow, gradually diminishes in size and in the force of its beats, until it may cease to be felt. A complete absence of palpable impulse in the radial artery and a slow heart action are occasionally found together. In these circumstances the carotid arteries pulsate with great force, whilst the heart sounds are loud and distinct, and indicate clearly that the heart muscle is acting with vigour. The evidence of a gradual contraction of the arteries in spite of a powerful heart action is thus clear. In the following stage of the development of shock there is a progressive quickening of the heart's pace, during which the radial pulse, if not already gone, ceases to be felt. The carotid pulse also becomes smaller and weaker, and the blood-pressure obviously falls everywhere. Exposed tissues are now blanched, an incision does not bleed, and eventually the whole vascular system becomes as depleted of blood as in a case of severe hæmorrhage, even when the operation has been performed with little loss of blood. In my earlier work I often saw anæmia so extreme that after closure of the abdomen it was difficult to be sure that a ligature had not slipped, and that a free hæmorrhage into the peritoneal cavity was not taking place. As the changes develop the temperature falls gradually until, in the rectum, it is too low to be registered by any ordinary

clinical thermometer. Perspiration becomes profuse in proportion to the smallness and quickness of the pulse.

A condition not now often obvious was formerly of serious clinical importance. The removal of the patient from the operating table to her bed frequently aggravated the symptoms and threatened death from heart failure. Recovery of consciousness and a return of the pulse to the wrist, with a slowing of the heart's action, were frequently delayed for hours, and during that time the circulation was a cause of much anxiety.

These troubles almost completely ceased after a change of treatment. It was formerly customary to starve patients as much as possible after abdominal operations, both solid and liquid nourishment being withheld, under the belief that by reducing the activity of intestinal peristalsis a development of peritonitis was made less likely. Now liquids are given by the mouth as soon as the patient can swallow, or by rectal, subcutaneous or intravenous injection on the operating table, if that is considered necessary. By these changes the dangers of shock were at once enormously reduced and curtailed, proving to demonstration that a loss of fluid from the body is one of the most important conditions of that state.

It is known that irritation of sensory nerves causes contraction of the arterioles, followed by a rise of blood-pressure in the large arteries, and the evidence above recorded indicates that if the injuries to nerves are sufficiently prolonged, or often repeated, the whole vascular system, beginning with the arterioles, becomes tensely contracted. As a consequence the blood-pressure in the large arteries rises at first. Fluid then escapes freely from the vessels, and from the body in the form of perspiration. This causes a fall of blood-pressure throughout the vascular system in proportion to the escape of fluid. The fall of blood-pressure is exactly comparable to the loss of pressure-raising power when a leakage occurs in any piece of machinery which depends upon hydraulic force for its movements. For example, when an operating table is raisable by a hydraulic pump, the foot lever sometimes offers little resistance to pressure and the table cannot be moved because the oil leaks.

The significance of the escape of fluid is occasionally emphasized during an operation if a sudden loss of blood is accidentally permitted when a state of advanced and still developing shock exists. The effect in aggravating symptoms of shock is out of all proportion to the amount of blood lost.

Hæmorrhage during an advanced stage of surgical shock, by allowing a rapid removal of fluid, permits a more speedy contraction of the peripheral vessels. The rapid accelerating effect on the symptoms shows that the loss of fluid, when there is no hæmorrhage, is resisted and delayed by an opposition to its escape through the walls of the vascular system. That such a resistance exists is indicated also by the concentration of the blood which is characteristic, and which shows that the fluid parts of the blood escape more readily than the red cells. This concentration was noted by Sherrington and Cope-man¹ in 1893, and it has recently been shown that the red blood cells may be relatively increased to a degree which indicates a loss of 40 per cent. of the blood volume.² In a case of severe uncomplicated hæmorrhage an opposite change in the density of the blood takes place. The pressure in the tissues is relatively higher because of the escape of blood, and fluid passes rapidly into the vessels from the tissues. The blood therefore becomes diluted. Although

¹ *Journ. Physiol.*, 1893, xiv, p. 52.

² Dale and Laidlaw, *Journ. Physiol.*, 1918-19, lii, p. 373.

in shock the changes are primarily due to active contraction of the vessels and in hæmorrhage to a passive contraction as fluid escapes, the end results are the same. In both, the blood-pressure becomes very low, because of the diminished quantity of fluid in the vascular system. In both coma and death are produced by a shutting off of the blood supply to the tissues and brain as the vessels contract. In hæmorrhage blood-pressure falls from the first, and an active vascular contraction hastens the loss of fluid. In shock the vascular contraction first raises blood-pressure in the large arteries, but in the later stages it hastens the loss of fluid and shuts off what remains from the tissues. The clinical importance of making a distinction between the two, for example, in a case of possible unseen hæmorrhage, is of course very great.

Professor Leonard Hill¹ has urged, with convincing arguments, that filtration from pressure does not take place in the living body and that movements of fluids outside the vessels, or from the vessels into the tissues, depend upon vital forces in the body cells. That there is no filtration in living tissues comparable to that which takes place through dead membranes is certain and the evidence that excretory cells are active agents is complete. But they are under nerve-control and as a rule abundant material is supplied by the vessels when secretion is active. Moreover, when much fluid is swallowed much is got rid of by the kidneys or by the skin, and when it is desired to increase the secretion of urine an abundance of bland fluid is administered, in addition to the use of other therapeutic measures. In the state of shock slowly induced in human beings a transference of fluids *out of* the vessels is *preceded* by an increase of pressure upon the blood by the vessel walls, and in a case of hæmorrhage the transfer of fluid *into* the vessels is *preceded* by a reduction of resistance within the vessels. In these two instances the fluids of the body certainly pass from place to place so as to rectify the temporary relative disturbance of pressure which occurs. No doubt a nervous centre controls the body cells and compels them to facilitate the necessary changes, any increased imbibition of fluids which takes place being reflexly induced as part of the mechanism provided for the maintenance of a normal balance of pressure and of chemical constitution within the tissues. That fluid must pass out of the vessels when they contract is a mechanical necessity and that it must be received by the lymphatics or by the body cells is also necessary.

The importance of this becomes obvious when certain septic and toxic cases are considered, in which, although the need for a secretion of sweat is urgent, yet this cannot be brought about by any means. I hope to discuss this condition elsewhere and will only point out now that the patient who will not sweat has generally a hot and dry and often a red skin and is not in a state of surgical shock.

When a healthy man is suddenly thrown into a condition of profound shock by a severe injury the changes above described take place almost instantaneously; an enormous amount of fluid is quickly lost and the dazed or unconscious mental condition, the extreme muscular collapse and the tendency to failure of respiration which constitute part of the symptoms are attributable to the shutting off of the blood-supply from the tissues generally, and finally from the brain. The tissues are starved, exactly as they are starved by an uncomplicated slow hæmorrhage. If this condition continues death must follow.

A development of surgical shock may be arrested at any stage by a cessation

¹ *Lancet*, February 14, 1920.

of its cause, and, if this occurs in time, all the symptoms pass off, evidences of a relaxation of the vessels being often easily detected by a careful examination of the vascular changes.

In his latest book dealing with the nature of shock, George W. Crile writes:—

“In our experiments on normal animals the first injury, or at least one of the early injuries or stimulations of a nerve trunk, always produced a rise in blood-pressure; but after the animal had become well exhausted, trauma or severe stimulation of nerve trunks caused little or no rise in the blood-pressure; in fact, in many instances the blood-pressure fell. After an animal had been reduced to the condition in which stimulation or injury produced a primary fall of blood-pressure instead of the primary rise which resulted when the animal was fresh, it was found to be practically impossible to raise and sustain the blood-pressure by therapeutic measures.¹

Thus, there is complete agreement between my clinical and Crile's experimental observations concerning the changes of blood-pressure caused by stimulation of sensory nerves. The pressure is first raised, later it falls. But whereas I believe that the vasomotor system is intensely active, that the vessels are contracted and that the fall of pressure depends upon a loss of fluid from the vascular system, Crile teaches, in 1921, that a directly opposite change takes place, that—

“*Surgical shock is mainly due to impairment or breakdown of the vasomotor mechanism. . . .* The evidence from which this conclusion has been reached is the following: Nerve-fibres when subjected to stimulation, mechanic or otherwise, may cause either an increase or a decrease in the blood-pressure. The presence of the so-called ‘pressor’ nerve mechanism is well established, as is also the presence of the so-called ‘depressor.’ . . . Now, mechanical stimulation of sensory nerves when both the animal and the nerve are fresh produces a rise in blood-pressure, due largely to a stimulation of the ‘pressor’ mechanism. On repeating the stimulation of the same nerve each subsequent repetition is attended by a diminished pressor action, and finally no effect is produced upon the blood-pressure. On the contrary, there may be a fall in blood-pressure after still further stimulation, that is to say, the depressor action alone may appear.”²

Running through the whole of Crile's writings there is the idea that in connexion with shock a low blood-pressure indicates a depressor action or a relaxation of some part of the vascular system and that a very low blood-pressure is a proof that there is a complete breakdown or exhaustion of the vasomotor nervous mechanism. This makes his argument extraordinarily difficult to follow and altogether unconvincing, whilst his conclusion seems quite unproved to those who recognize that leakage of fluid from any piece of mechanism, actuated by hydraulic force, necessarily takes away much and perhaps all of its pressure-raising power.

Moreover, there is direct evidence regarding the state of the vessels. Crile himself says the arteries are “quite empty”³ in the state of shock, and if so they must be contracted.

Definite experimental evidence that the vessels are contracted was put forward⁴ in 1909 by Dr. M. G. Seelig and Professor E. P. Lyon, of St. Louis, Mo., U.S.A. Crile refers⁵ to their records in his latest work, but he does not state, and he makes no attempt to refute, their contentions.

¹ “A Physical Interpretation of Shock, Exhaustion and Restoration,” 1921, pp. 37-38.

² “Surgical Shock,” pp. 151-152.

³ “A Physical Interpretation of Shock, Exhaustion and Restoration,” p. 38.

⁴ *Journ. Amer. Med. Assoc.*, January 2, 1909, lii, pp. 45-48.

⁵ “A Physical Interpretation of Shock, Exhaustion and Restoration,” p. 107.

In 1907 Professor Porter recorded an "investigation into the effect of uniform stimulations of the depressor, brachial and sciatic nerves at various levels of blood-pressure from the normal down to about 9 mm."¹ of mercury. He showed that "the absolute change in blood-pressure upon stimulating these afferent nerves remains almost unchanged until the blood-pressure has fallen to about one-third its usual height,"² and that "the reflex fails only when the blood-pressure sinks to a level at which anæmia of the vasomotor centres is certain. Indeed, all we know regarding these and similar cells strengthens the belief that their endurance under stimulation is very great. On the other hand, they are extraordinarily sensitive to changes in the blood supply." It was added that "the data presented are wholly opposed to the hypothesis that would explain surgical shock by the exhaustion of the vasomotor centres."³

Porter showed that the percentile rise of pressure (the percentage of rise with reference to the existing pressure) was greater as the blood-pressure fell. Crile's comment is that—

"Applying Porter's percentile interpretation to the effect of adrenaline, the percentile rise would be over 300 per cent., that is, according to Porter's reasoning, the vascular state is three times better than normal, but nevertheless the dog is dying. The error in Porter's reasoning may be made more clear by a homely illustration. If a goad be applied to a fresh horse, the resulting increase in speed may be stated as a percentile increase. When the horse is in extreme fatigue and an equal goad is applied, the percentile increase will probably be the same, but nevertheless the horse is exhausted."⁴

This looks like a *reductio ad absurdum*, but Porter gives definite evidence that the vasomotor mechanism responds to stimulation when shock is fully developed, and Crile exclaims that "the dog is dying." This is not a contradiction of Porter's contention. Crile's illustration confuses the issue by failing to differentiate between the dog and its vasomotor system. His hypothetical horse, which becomes exhausted, represents the dog. His hypothetical horse, which responds to the goad when exhausted, represents also the vasomotor system of the dog. To deduce from the state of the one that the other must be in the same condition is not a sound argument, and to represent the dog and its vasomotor mechanism, the whole and a part, by the same horse makes any conclusion of doubtful value. The suggestion that "the vascular state is three times better than normal" in the condition described by Porter is as unwarranted as would be an assertion that a horse behaves better in proportion to the speed with which he bolts.

Further support for the view that the vasomotor mechanism maintains its activity is found in the reports of the Special Investigation Committee on Shock and Allied Conditions. That Committee concluded that "the vasomotor centre should be regarded as an agent whose functions are extremely stable and whose capabilities for continued services are its most outstanding feature."⁵

So far as I know, Crile has not in any of his writings discussed the effect of a loss of fluid from the vascular system as the state of shock develops. In man a large loss of fluid by sweating takes place. In the dog there is no corresponding loss from the skin, but the pallor of the tissues, the low blood-pressure following a rise, and the concentration of the blood are

¹ *Amer. Journ. Physiol.*, 1907, xx, p. 399.

² *Loc. cit.*, pp. 404-405.

³ *Loc. cit.*, p. 405.

⁴ "A Physical Interpretation of Shock, Exhaustion and Restoration," pp. 107-108.

⁵ Medical Research Committee: Special Report Series, No. 25, p. 113.

demonstrable. The mouth and lungs are the natural channels for a loss of fluid and heat in the dog. When that animal has taken hard exercise he lies down with his tongue out, panting rapidly, fluid dripping from his mouth, and his breath is saturated with moisture. Probably in the dog there is in the state of shock a free loss of fluid as saliva and in the expired air, but no such excess is recorded, although in the few experiments I have seen on this animal it appeared to me that there was always a free flow of saliva.

Shock is not easily induced in a dog and heroic measures have to be taken. Crushing and burning the feet and removal of as much as three-fourths of the integument of the body, followed by sponging the raw surfaces, are mentioned by Crile, who from time to time also helped the process by a letting of blood.¹ A loss of fluid is essential unless shock in man is entirely different from shock in dogs. It is obvious that the great loss of fluid from the vessels known to occur in the state of shock may explain the fall of the blood-pressure in shock as in hæmorrhage, but this does not seem to have been considered. By this omission it seems to me that Crile has overlooked the true explanation of the nature of shock.

Crile has published a series of photographs, a really beautiful demonstration that certain brain, liver, and adrenal cells undergo degeneration in a great many conditions of exhaustion, but exhaustion of the vasomotor system is inextricably mixed up with exhaustion of the body itself. Crile says, "our premises in studying fatigue, shock, and exhaustion, are identical. These states have interchangeable values."² In describing the condition of the cells, Crile records his observations on cases of hæmorrhage next to those arising from trauma, and surely hæmorrhage must act by starving the tissues, and cannot be a cause of over-stimulation and exhaustion of the nerve centres. The evidence is strong that these changes are in all cases due to starvation, sometimes because of exhaustion of the body, sometimes from hæmorrhage and in surgical shock from a cutting off of the blood-supply by an excessive contraction of the whole vasomotor system, associated with much loss of fluid. In a paper published in this month's issue of the *Proceedings* of our Society, Mott and Uno³ state that the brain in fatal cases of shock shows conditions similar to those observed by Mott after ligature of all four vessels to the brain. This corroborates the view that death by shock occurs from arrest of blood-supply to the brain.

If the view advocated in these pages is correct then recovery from shock must depend upon a relaxation of the blood-vessels when the causes of shock cease. Clinically, after the most severe operations are finished, if the patient is alive, in a case of uncomplicated surgical shock, the chances that recovery will take place are very strong. When the causes of undue vascular contraction cease the tendency to a return to the normal calibre is insistent.

Nevertheless, attempts to force recovery, whilst the stimulus to vascular contraction is active and increasing, are of little if any use. Concerning injection of saline fluids into a vein Crile wrote that this always caused a rise in the blood-pressure of an animal in shock. But—⁴

. "In the cases of deepest shock the rise in the blood-pressure was not sustained beyond a certain time, even during the infusion. . . . Blood-counts and hemoglobin

¹ "Blood-pressure in Surgery," 1903, pp. 64, 67, 69.

² "A Physical Interpretation of Shock, Exhaustion and Restoration," 1921, p. 2.

³ *Proceedings*, 1922, xv (Sect. Neurol.), p. 38.

⁴ "Blood-pressure in Surgery," 1903, pp. 269-270.

estimations showed that the blood was not much diluted by the saline. The solution escaped from the vessels at a rate fairly proportional to the rate of infusion, mainly through the channels of normal absorption of water. The fluid accumulated in the walls and the lumen of the stomach, the intestines, and the abdominal cavity, in the respiratory tract, the thoracic cavity and the subcutaneous tissue.

"When the infusion approximately equalled 300 c.c. per kilo, the amount of fluid accumulated in the splanchnic area caused embarrassment, even failure, of respiration, by mechanically fixing the diaphragm and the movable ribs."

The Special Committee on Surgical Shock devoted much attention to the treatment of "secondary shock" and of hæmorrhage during the war, by injection of various fluids into the veins, and declared that "the results were less favourable in proportion as the shock factor predominated over that of hæmorrhage."¹ Here, again, there is evidence that when the causes of shock are active an injection of fluid cannot force recovery. In cases of hæmorrhage a replacement of suitable blood is immediately effective, and as the vessels relax, when causes of shock cease to act, fluids are readily taken into the body however and whenever they are offered. The difficulty in introducing fluid when the causes of shock are active, and the avidity with which it is received as recovery takes place, are easily understood if the vessels contract as shock develops and relax as it passes off. These conditions cannot be so readily explained if the vessels relax during shock and therefore contract during recovery.

Attempts to raise blood-pressure, when shock is well developed, by administering vasoconstrictors is, according to the explanation advanced, worse than useless.

Not vasoconstrictors but vasodilators are indicated. Eugene Boise² has long advocated the intravenous injection of a preparation of *Veratrum viride* to release a cardiac spasm, and according to this view the time-honoured resort to brandy as a vasodilator after an accident is justified. But these measures are chiefly required to start vasodilatation when it is doubtful whether recovery will take place. When dilatation begins it will continue if other conditions are favourable and if fluid is supplied as the vessels dilate. Conservation of heat and restoration of heat lost are powerful agents in promoting recovery. Any fluid given in any way should therefore be warm.

In the treatment of surgical shock, then, the important needs are to arrest further causes of shock, including pain, to keep the respiration going, to supply fluids as they can be absorbed and to restore heat to the body. No operation that can be delayed should be undertaken when the state of shock is acute and severe.

The foregoing pages have been devoted to a consideration of the effects of an uncomplicated stimulation of sensory nerves. It seems to me of supreme importance for the understanding of many surgical developments that shock thus induced should be clearly differentiated from similar conditions which, however exactly they may simulate shock, are produced in other ways. A reaction to injury constitutes the condition to which the word "shock" was first applied, and in my opinion much confusion would have been avoided if the word had been preserved with only one medical meaning. I wish, therefore, to insist upon the view that a condition of shock, brought about by an uncomplicated stimulation of sensory nerves does occur. This seems to be proved by the excellent work of G. W. Crile on the prevention of shock. By blocking,

¹ Medical Research Committee: Special Report Series, No. 25, p. 9.

² *Trans. Amer. Gynæcol. Soc.*, 1908.

that is, anæsthetizing, nervous tracts, the passage of stimulations through them can be arrested and a development of the condition of shock can be absolutely prevented. When a patient is under efficient spinal anæsthesia, it is impossible to induce shock by an operation causing stimulation of nerves communicating with the brain through the anæsthetized part of the cord. Symptoms of hæmorrhage may arise during such an operation and simulate those of shock, but there can be no true primary shock as long as the spinal anæsthesia is effective.

Therefore, whatever the correct interpretation of the changes following stimulation of sensory nerves may be, it is certain that a state of shock with the fall of blood-pressure above described may be induced by a sufficient stimulation of these nerves.

The Special Investigation Committee on Surgical Shock, appointed in 1917 by the Medical Research Committee, drew "a distinction, afterwards generally recognized, between 'primary shock,' a condition appearing rapidly after injury and resembling a prolonged and severe fainting, and 'secondary shock,' a more slowly ingravescent condition seen in the cases as received at the clearing stations. . . . The special problem before the Committee was to investigate the nature of the 'secondary wound shock,'"¹ and they described this as largely a toxæmia, associated with a relaxation of the capillaries throughout the body.

The conception of shock as a toxæmia has in some writings, if only by implication, been extended to include all forms of shock. There are at least four causes of mischief—namely, trauma, toxic poisoning, septic poisoning and hæmorrhage—each of which when fatal may produce, without the others, similar if not the same end conditions in the vascular system. I pointed out in 1893² that in a fatal septicæmia "the temperature is usually high but may fall, almost as in shock," and that wound discharges and other substances, with or without life, when circulating in the blood may produce effects "so severe as rapidly to induce great contraction of the arteries, a high temperature and death. It is even conceivable that death may be produced almost with the suddenness of shock."³ Recent investigations concerning histamine and anaphylaxis show that death can certainly be produced by poisons, immediately, as if by a severe irritation of sensory nerves. I do not, however, know of any surgical condition in which evidence of dilatation of the capillaries is associated with symptoms of shock or with any shock-like state.

But whether the vascular changes characteristic of shock when brought about apart from an injury should still be described as shock, and whether there is a form of shock-like condition characterized by a dilatation of the capillaries, are questions which, as this paper is already so long, must be discussed on some other occasion.

DISCUSSION.

Dr. LAPHORN SMITH said that all the cases which had been described as those of shock were not really cases of shock at all. Most of them were cases of primary or secondary hæmorrhage. The real cases of shock due to reflex action through the nervous system did occur at one time quite frequently when the intestines were taken outside of the abdominal cavity and handled and chilled a great deal. He had always taught his students that it was unlucky even to see the intestines, much less to handle

¹ Medical Research Committee: Special Report Series, No. 25, p. 8.

² "The Physiology of Death from Traumatic Fever," p. 120.

³ Loc. cit., p. 127.

them. Since the adoption of the Trendelenburg position and the preparation of the bowels by gentle laxatives the empty bowels fell away out of sight as soon as the air entered the peritoneal cavity, and if a flat pad were placed over the omentum most operations in the abdominal cavity could be done without even seeing the bowels. Among the other causes of so-called shock in the order of their frequency were: (1) Prolonged anæsthesia, especially with chloroform; (2) cooling of the body by the air or wet towels, meant to be warm; (3) excessive purgation before the operation especially with sulphate of magnesia; (4) great loss of water from the blood from perspiration. During his first five years' operating he had sometimes seen a case of shock but hardly ever since. The reason, he thought, was that tumours were now removed while smaller; arteries were tied before they were cut, so that there was much less primary and secondary hæmorrhage; there was less exposure and handling of the intestines; ether had taken the place of chloroform with most operators and the preliminary hypodermic of morphine had lessened the quantity of anæsthetic required. He would suggest giving the patient as much liquids as possible the day before operation so that she would begin with good blood-pressure in the coronary arteries; and the taking of the blood-pressure every quarter hour during the operation so that the operator would know exactly the condition of the patient. Also the giving of normal saline enema for the duration of operation to make up for the loss by hæmorrhage and sweating. Also that the patient be kept warm artificially by large flat pans full of hot water under her and covered with blanket; and the elevation of the foot of the bed on returning to the ward. When he had had to deal with a very large ovarian cyst, say 30 to 40 lb., he had generally removed the bulk of fluid contents two days before so as not to reduce the intra-abdominal support too suddenly at the time of operation. One might thus avoid the so-called hæmorrhage into the large veins which, although the blood would be recovered later, might, for the time being, have the same effect as if it were lost. If all these things were done he thought that shock after abdominal operations at least would be a thing of the past.

Mr. GORDON LUKER said how difficult it was in some cases to decide whether a patient was suffering from surgical shock only, or from surgical shock and hæmorrhage. He had seen several cases recently in obstetrics and gynæcology in which the patient had suffered from profound shock for a very long time—in one case thirty hours—and yet not more than a pint of blood had been lost. His opinion was that some pregnant women stood a small loss of blood very badly. It was very difficult to decide in such cases whether to treat the patient for shock, avoiding any examination, manipulation or anæsthetic; or whether to treat her as if she were suffering from hæmorrhage; as, for instance, by giving gum or saline solution intravenously, or by performing blood-transfusion, to be followed by any necessary examination or exploration. In some cases too long a delay might result in the death of the patient. He would be very glad of any information on this point.

Professor HENRY BRIGGS (President) said that the Section highly appreciated the continued researches of Mr. Malcolm on surgical shock; and he hoped that Mr. Malcolm would carry out the special research he had outlined as to the escape of body-fluids.

Mr. MALCOLM (in reply) said he agreed that anæsthetics might produce symptoms indistinguishable from those of stimulation of sensory nerves, probably by poisoning. Removal of very large tumours did not cause shock, and symptoms from this could only be due to sudden removal of support of the vessels which he did not regard as a cause of shock. If the contention was right that contraction of the arteries was the cause of symptoms, this could hardly be also a preventive of mischief. Leonard Hill and J. McQueen said there was a critical level of blood-pressure.¹ They supported Mr. Malcolm Donaldson's contention that "attempt at compensation is made by increased vasoconstriction and while, by this means, succour is brought by increased pressure in the coronary arteries to the heart-muscle and to the brain, the capillary field in the body generally, outside these areas, falls from its already low level of a few millimetres of blood-pressure still lower. Vasoconstriction is no cure for an arterial blood-pressure below the critical level." His (Mr. Malcolm's) contention was that if vasoconstriction

¹ *Lancet*, 1922, ii, p. 66.

made blood-pressure fall *below* the critical level it must also make it fall *to* the critical level. At one time he had recommended amyl nitrite on theoretical grounds but had not had good opportunities of trying it. Adrenalin had been largely used. On theoretical grounds he objected to it, but he never saw definite evidence that it did good or harm. The only way to distinguish between shock and bleeding if there was no history, was by finding concentrated blood indicating shock, or diluted blood indicating hæmorrhage. Each case must be judged by this and on its merits. The question of symptoms of poisoning must also frequently arise. As to where the blood went out, when the arterioles contracted, they did not, of course, completely close but pressure rose in the large arteries. After a time, by vagus action, this pressure was passed back to the large veins and from them back to the small veins and capillaries, where the blood escaped. The capillaries, practically everywhere and especially in numerous lymphatic and other glands, were in close contact with the lymphatics. The lymphatic system seemed to act as a reservoir for fluid to be taken from, or returned to, according to the size of the vascular system, which varied with many conditions, and particularly with exercise. Leonard Hill had said that a man "may lose 7 lb. of water by sweating during hard exercise in hot weather." This was equivalent to half the blood in a man weighing 13 st. Sweating might be as profuse and more rapid from shock than from exercise. The double evidence of the concentration of the blood and the loss of fluid by sweating was proof that much fluid left the vessels and the body.

Section of Obstetrics and Gynæcology.

President—Professor HENRY BRIGGS, F.R.C.S.

Labour obstructed by a Solid Carcinomatous Tumour of the Left Ovary ; Cæsarean Hysterectomy, with Removal of the Tumour.

By S. GORDON LUKER, M.D., F.R.C.S.

THE specimen consists of the uterus and left appendages removed by operation during labour. Before removal, the ovarian tumour was in Douglas's pouch, pushed down into the pelvis by the foetal head, thus forming an insuperable obstacle to delivery. In the fresh state the tumour measured 19 cm. by 9 cm. by 4.5 cm.; the surface was smooth; on section it was dull white and whorled in appearance. Towards the centre it appeared degenerate and yellow with some extravasated blood: it was free from attachment to other organs and there was no evidence of any other disease in the pelvis. The right ovary appeared healthy.

The clinical history of the case was as follows: The patient, E. P., a primigravida aged 40, was admitted to the London Hospital on April 24, 1922, having been in labour fourteen hours. She had been attended by a doctor outside who had applied forceps unsuccessfully before sending her up to hospital.

Examination after admission showed the general condition of the patient to be quite good; the foetus was alive, the foetal head engaged; the external measurements of the pelvis were normal. From her dates the patient was fifteen days over full time.

On vaginal examination the os uteri was three-quarters dilated and torn. Posteriorly the examining finger felt a large, hard rounded lump behind and outside the uterus, pressed down into the pelvis by the foetal head. The pains were strong and frequent. Diagnosis of the lump rested between an ovarian tumour or a fibroid in the lower uterine segment: the latter seemed rather more probable.

Cæsarean section was decided upon for the following reasons: First, the foetus was post-mature and large, the foetal heart was becoming more rapid, the patient was over 40; therefore, Cæsarean section afforded the only real chance of a living child. In the event of the child being alive it was decided to do hysterectomy because the uterus was most probably infected: the membranes had been ruptured nine hours and forceps had been applied under unfavourable conditions.

Laparotomy was therefore performed and the uterus was packed off from the peritoneal cavity. The infant was extracted by the ordinary uterine incision, and the uterus, left tube, and left solid ovarian tumour were removed in the ordinary way. A small portion of the cervix of the uterus was preserved. The child was a well-developed female weighing 8 lb. 10 oz. The patient made an uninterrupted recovery, the temperature and pulse-rate settling after two days.

Histological examination of the tumour shows it to be a solid polygonal-celled scirrhus carcinoma of the ovary.

On thinking over this case I am aware that my treatment will not be accepted as orthodox in two points:—

(1) The performance of Cæsarean section for obstruction by an ovarian tumour.

(2) The necessity for hysterectomy after Cæsarean section owing to the danger of sepsis.

On the first point I have mentioned above that the reasons I decided upon Cæsarean section were that the patient was over 40, and a primigravida who had already been in labour with membranes ruptured for some hours. As the fœtus was large it seemed quite likely that, if the ovarian tumour could be removed by laparotomy, natural labour might go on for quite a long time before delivery of the child could be effected. In other words, the chance of a living child was bad except by Cæsarean section. On the second point I am, in general, opposed to hysterectomy after Cæsarean section in a suspect septic case. I have found it impossible to determine practically by bacteriological methods whether or not the liquor amnii is seriously infected. I am therefore guided by the condition of the patient, the length of the labour, the length of time the membranes have been ruptured, and the extent and nature of manipulations and the conditions under which they were carried out. In this case I decided that it would have been dangerous to leave the uterus.

The carcinomatous nature of the ovarian tumour might be considered an additional indication for hysterectomy. Yet I feel that in similar cases in younger women apart from sepsis I would prefer to leave the uterus and keep the patient under observation.

DISCUSSION.

Dr. HERBERT SPENCER thought the right treatment had been carried out in removing the uterus; he would however have preferred total hysterectomy, as it removed the cervix, which was torn and the most likely part to be infected. The specimen appeared to be an undoubted case of unilateral hard carcinoma. He had, however, seen two cases of unilateral soft carcinoma of the ovary, which, although ruptured at the time of operation, remained well for many years, and one of the patients had since had children. He agreed with the President that in spite of the typical appearance under the microscope of medullary cancer there was a doubt whether the growth were cancers at all.

Dr. ANDREWS said that if Mr. Luker hoped for adverse criticism of his treatment of this case he would probably be disappointed. The case did not come into the ordinary category of labour complicated by an ovarian tumour, in which Cæsarean section was to be deprecated. The reasons given by Mr. Luker for the course which he adopted, viz., the age of the patient, and the probability of infection during the attempt at delivery by the forceps, would satisfy most people that Mr. Luker was justified in performing the operation of Cæsarean hysterectomy.

Carcinoma of the Cervix following Gonorrhœa in a Young Woman.

By S. GORDON LUKER, M.D., F.R.C.S.

THE following notes refer to a young 2-para, aged 28, in whom carcinoma of the cervix followed an attack of gonorrhœa contracted five months previously. The occurrence of a carcinomatous growth during local treatment of the cervical canal for gonococcal cervicitis seems to support the "irritation" theory, since in this case there was the discharge and also the repeated application of antiseptics.

The following are notes of the case: 2-para (younger 2½), aged 28, was sent to the Venereal Department, London Hospital, on August 27, 1921. She was examined by Dr. Malcolm Simpson who noted a slight white discharge and a definite laceration of the cervix. Though there was very little clinical appearance of gonorrhœa, gonococci were found by the Bacteriological Department both in the urethra and cervix. The history of the discharge dated from six weeks previously. Local routine treatment was carried out until January, 1922, including many applications of silver nitrate solution. Polypoid warts were noted in November and as they did not disappear the patient was sent to me for examination at the end of January. She was admitted to the London Hospital under my care on February 26, 1922.

Examination under an anæsthetic showed an irregular ulceration at the external os: the edge was undermined and bluish. There was no evidence of infiltration of the cellular tissue round the cervix. I decided to do a partial amputation of the cervix; during the performance of this I found the tissues of the cervix distinctly friable, and the diagnosis of carcinoma was a moral certainty. This was confirmed by microscopical examination which showed a "squamous-celled carcinoma, with much inflammation."

A few days later Wertheim's hysterectomy was performed after preliminary radiation by the insertion of 50 mgr. of radium into the cervical canal for thirty-six hours. There appeared to be no glandular involvement and the cellular tissue was not infiltrated. The patient made a good recovery.

The pathological report on the specimen is the following: "(1) Chronic purulent ulceration and early squamous-celled carcinoma of the vaginal portion of the cervix. (2) Chronic, purulent ulcerative cervicitis of cervix uteri."

Since the inauguration of the Venereal Department in 1915 no other case of a similar nature has occurred.

DISCUSSION.

Dr. ARTHUR GILES considered that Mr. Gordon Luker's case was a very interesting one: he gathered that Mr. Luker regarded the carcinoma as having been brought about by the irritation of the gonorrhœa or of the treatment adopted for it. But he thought that such a view would be difficult to prove or to maintain. Carcinoma of the cervix was so rare in a nullipara that it was possible to adopt a safe working rule in diagnosis that if a woman were a nullipara, carcinoma of the cervix could be excluded. Yet gonorrhœa was a very common thing in nulliparæ, and probably many of these patients had severe treatment in the form of caustics. If a causal relation were established, we should expect that carcinoma of the cervix would not be an almost unknown occurrence in nulliparæ.

Mr. RIVETT questioned whether irritation was a causal factor in carcinoma of the cervix. Had the senior members seen many cases of carcinoma of the cervix

associated with proclitica? If irritation were a predisposing cause it should be a common cause.

Dr. HERBERT SPENCER had seen only two cases of cancer of cervix and vagina in cases of proclitica. One of these he had drawn for Sir John Williams' "Cancer of the Uterus," the other he had exhibited at the Gynæcological Congress at Birmingham. Dr. Spencer had published¹ his opinion that there was probably a close connexion between venereal diseases and cancer of the cervix. Mr. Luker's case seemed to be an undoubted case of squamous-cell carcinoma; but papillomatous growths occurred which closely resembled carcinoma unless the sections were made with great care.

Dr. ANDREWS did not feel certain that this case could be described as one in which carcinoma of the cervix had *followed* treatment by silver nitrate, &c. The fact that the deep tissue of the cervix was found to be friable within five months of the beginning of the treatment showed that the growth was not very early, and it might have been present when the treatment was begun. In answer to Mr. Rivett's question Dr. Andrews said that he had shown, about fifteen years previously, a case of carcinoma with prolapse. On that occasion the majority of the Fellows considered it to be an exceedingly rare condition.

Conservative Labour-induction.

By Professor HENRY BRIGGS, F.R.C.S. (President).

IN the treatment of labour in the minor degrees of pelvic contraction or of the equivalent fœtal oversize, the first place belongs to labour-induction. In practice, whether debated, accepted or declined on any of its attributes, there can be no denial of its outstanding merit as the nearest approach to the normal labour.

The obstetrical meridian is occupied by well-housed and well-managed normal labour. On its borderland, within the above group, arise exacting appeals to higher obstetrics to be gradually or promptly fulfilled prior to the adoption or exclusion of induction.

Exclusion by a patient and her relatives cannot be regarded as singular: one example will suffice. A slightly flattened pelvis in a short-statured vigorous mother of seven healthy children; the following is the order of her eight child-births—(1) female, (2) male, (3) female, (4) male, born naturally; (5) male, lost to craniotomy after failure of forceps-delivery by an experienced family doctor; (6) male, by induction in the thirty-seventh week; (7) female, again a natural labour; (8) female, by podalic version for a transversely lying head persistently above the brim after a confidently and patiently watched second stage. During the labours (7) and (8) the regular family doctor was abroad on War Service.

Patience and confidence on the part of the accoucheur may be subjected to the intervention of another accoucheur: take the instance of a flat pelvis in Mrs. C., whose four induction-babies in hospital at birth weighed 5 lb. 11 oz., 6 lb. 9½ oz., 6 lb. 9 oz., 7 lb. 11 oz.; under my charge in the course of the fifth induced labour; in my enforced absence at a distance, one of my senior colleagues was called to her at the Maternity Hospital and by Cæsarean section delivered the child weighing 7 lb. 2 oz.—9 oz. lighter in weight than the preceding induction-baby: the same obstetrician repeated the Cæsarean section with the addition of sterilization when the last baby of still lighter weight,

¹ "Tumours Complicating Pregnancy, Labour and the Puerperium," 1920.

6 lb. 4 oz., was born at the estimated full-term in 1919. All her children, four of induction and two of Cæsarean birth, are now alive and well: they were preceded by two stillbirths, forceps-deliveries, one at each of the first two full-term labours in her own home.

A complete change of the obstetrical team may explain the obstetrical history of Mrs. T. Obstruction by a flat pelvis in her first labour at full term in 1900 was treated by a craniotomy after forceps-failure at the hands of a most experienced family doctor, who two and three-quarter years afterwards, 1903, shared in the management of an induction in the thirty-seventh week. The girl then born is now a broad-shouldered stalwart, aged 19 years, and took a keen and pleasant interest during my inquiry call in the course of the preparation of this paper. Mrs. T. herself voluntarily stated that after the death at a ripe age of her former family doctor she underwent, at other hands, in 1918, a Cæsarean operation in a nursing home: the induction-bougies placed on the Saturday at 9 p.m. had not started labour on the Monday afternoon whereas her former induced labour was over in thirty-four and a half hours. Impatience is to be expected especially at times when, and in places where, a more speedy substitute offers temptations. The convalescence after the Cæsarean operation was reported tardy and febrile.

A change of residence interrupts continuity of obstetrical method: in a frail young married woman with a generally contracted pelvis, induction of labour in the thirty-seventh week of the first pregnancy was followed in twenty-four hours by the unaided delivery of a fine infant now a most promising and healthy girl of 4 $\frac{3}{4}$ years. After a second marriage and near the end of the next pregnancy Cæsarean section was chosen and carried out in 1921, with success, in a distant city. The value of the latest report from the grandmother that the induction baby is the stronger child is qualified by the male parentage.

These events of the last five years represent the trend of obstetrical practice. The conservative labour-induction to-day stands below par and Cæsarean section above par. A more accurate balance can be promised from higher obstetrics. In the meantime a half-hearted faith in clinical investigation shelters under Cæsarean section. A whole-hearted faith resolutely seeks and gathers diagnostic equipment for a more accurate differentiation and limits and lifts Cæsarean section. Delicate, intricate, formidable and far-reaching clinical problems are to be acknowledged. In this connexion how singularly apposite is the available picture of the abiding patience and peaceful contentment attached to labour-induction for habitual death of the fœtus without maternal hazards when a less promising foetal vitality softens criticism, if it arises, in these very rare and as a rule gratifying opportunities of practice.

Notoriously disturbing elements in labour-prognosis are attached to the uncertainties in the behaviour of a flat pelvis of minor degree and the surprises they have so far impressed upon clinical discernment and labour management.

The fixed prescription of a Cæsarean section at full term has been cancelled even in high obstetrical centres when the earlier unexpected efforts of Nature in glaringly exceptional instances rightly or wrongly have evaded spontaneously and successfully the most prudently arranged Cæsarean section. Between an over-estimate and an under-estimate many of the errors are insignificant.

The attitude of the patient in the first of the illustrative obstetrical histories I have already quoted is wholly pardonable; she herself took the risk of her flat pelvis in the eighth labour repeating its obstruction to the foetal head lying transversely above the brim. The definite obstetrical indication of version is not to be ignored as an inset in the total 8-parous history of a patient with one child lost to craniotomy.

Obstetricians admit the subtlety and surprise in a small fractional percentage of total labours and accept both without the belittling qualification "they may be taken for what they are worth": they are to be practically respected by the cultivation of clinical methods.

The generally contracted pelvis or its equivalent in minor degrees is a formidable antagonist during full-term labour; it intensifies the head moulding and the expelling forces; increases the suffering; demonstrates the invaluable benefits of opium against a too hasty or too early forceps-application with the risk of the object lessons of forceps-failure, an easy and rare craniotomy, or a conservative Cæsarean section.

Each of these operations in the course of an induced labour cast a lurid glare on the disappointing and embarrassing clinical error in the original selection of induction.

Ante-natally the note of a previous difficult forceps-labour has always to be widely and wisely weighed. Maternal and fœtal disproportion may never recur or its slightness be negligible in any of the subsequent labours.

The aim of this communication is to indicate the plan of selection and to report a twenty-nine years' (1893 to 1922) experience of collateral conservative labour-induction and Cæsarean section.

A preceding period, my first ten years, 1883 to 1893, saw for induction the exclusion of tents and bags, the choice of Krause's single bougie and the later multiplication of it to three or four bougies; the steps to the method of induction ultimately adopted for maternal safety and enhanced speed.

In 1893, before the North of England Obstetrical and Gynæcological Society, I supplemented a previous note I had read in 1887 before the Liverpool Medical Institution and referred to the details of ten inductions, adding a note of appreciation of the "palper mensurateur" advocated in 1891 by Pinard in confirmation of the bimanual or "measurement grip." In the interests of the child the practice of induction earlier than the thirty-sixth week was in my opinion in 1893 to be discouraged, if possible, as full-term Cæsarean section had promised the brighter future it has since shown and had on two occasions been adopted by me in the treatment of labour in the major degrees of pelvic contraction. The fœtus in the thirty-sixth week or later has reached a period of eligible viability; it became in my practice the workable criterion.

The selection of induction, the persistent crux of obstetricians, leads beyond an isolated pelvimetry or the latest ruling on the pelvic conjugate. It raises the accoucheur's bimanual estimate of the relative size of the fœtal head and the pelvis to a responsible ante-natal assessment to which average fingers and thumbs have generally proved equal, without or with a judicial allowance for adaptability and compensation within the concerned anatomical structures and physiological functions.

Against the prominent merits of labour-induction bogey after bogey has been lodged. The least bogey attacks the bougies-method: three or four gum-elastic bougies, Nos. 8 to 10, are more easily inserted after an index finger or two fingers have dilated the cervical canal and slightly detached the membranes: the lithotomy position of the patient on a table lends its own special aid in primigravidæ: prævia placenta is to be excluded especially in multiparæ: six hours to seven days are wide margins occasionally inconvenient, they are insignificant in the face of an assured or assurable onset and of the enormous compensatory attributes of maternal safety with a very minor and decreasing ratio of morbidity.

The imperious criticism and the imperious demands cast upon labour induction by the exuberance of Cæsarean section are exposed to vivid light in the recently issued and highly appreciated double number of the *Journal of Obstetrics and Gynæcology* which will forthwith be subjected to the general literary criticism that the best of prose is rightly called pedestrian: at every step it must find a foothold on the ground of experience firm enough to support its weight: it is more various than poetry and assumes in the reader an old acquaintance with the facts of life and keeps him in touch with them by a hundred quiet devices of irony, reminiscence and allusion.

Blacker's rivets bring steadiness within practical obstetrics. A prerogative place in special literature is to be claimed for Blacker's article on the limitations of Cæsarean section in the same issue of the *Journal*¹: it reaches and touches every point of thought and action and impresses the highest teaching and leading of advanced obstetrics.

At the moment, as if in a creek on the shore, I realize that I am placed in a Section of Obstetrics face to face with eight stillbirths and five neo-natal deaths within my series of 159 conservative inductions (8.2 per cent.). Let me make the frank confession that although they are below and not above the average of published records they are dark and partly deletable blemishes.

In one of two vertex cases, a second induction after a first induction (thirty-seventh week) live birth without forceps, I regretfully accepted the clinical report and applied forceps too early and finished with a craniotomy on a foetus, 5 lb. 2 oz.; in the other vertex case the obstetric assistant on my own guidance waited six hours before his mid-forceps operation.

In two breech deliveries after impaction in a generally contracted pelvis the regret is thrilling: in one the patient's abdominal wall and pelvic floor were rigid and thick, a general anæsthesia was needed: ante-natally and during the induction I overlooked the breech presentation; myself only partly convalescent after a serious operation I left the labour management in the hands of a most experienced and capable obstetrician clearly to substantiate the sound rule of the preclusion of labour-induction in a breech presentation in a primigravida with a generally contracted pelvis: the other stillbirth followed preliminary external version for breech with hydramnios: the membranes were punctured and the head left above the brim under an abdominal binder: the head two hours later was found in the left iliac fossa and the right elbow in the brim: podalic version: the foetal heart, 120, gradually slowed to 50 during a moderately speedy extraction of the child: its life was lost by asphyxia.

The two stillbirths and three neo-natal deaths (not one of them was in a suspect case) represent in the 104 collateral Cæsarean operations 4.8 per cent.

The special literature on labour-induction which is wholly British in origin was so ably, fully, deeply and widely digested and presented by Dr. Herbert Williamson in two articles, 1905 and 1906, in the *Journal of Obstetrics and Gynæcology*, that the last word has been uttered.

Among confirmatory reports the contribution by Sir John Phillips in the *Lancet*, ii, 1920 (p. 741) appeared with its able and precise pilotage from experience.

Dr. Norman B. Capon, after working on behalf of the Medical Research Council in the Liverpool University Laboratory, reports to me as follows:—

¹ *Journ. Obst. and Gyn. Brit. Emp.*, 1921, xxviii, pp. 147-162.

" A certain proportion of fetal and neo-natal deaths occurring after induction of premature labour are ascribable to causes having no relationship to this operation. For example, an infant which lived for fifty-one and a half hours was found to be the subject of chronic plastic peritonitis (contracted *in utero*), with intestinal obstruction and acute peritonitis.

My investigations into the causes of intracranial birth-traumata confirm the widely-accepted view that the premature infant is specially liable to these injuries. Further, since a rapid delivery subjects the fetus to added danger, the use of forceps after induction of premature labour is strongly contra-indicated."

Concerning the use of forceps in induced labour, complete restraint as an item of control is ideal but impracticable in team work: forceps were used in private practice in 10 of 53 and in hospital in 15 of 106 cases, occasionally by myself but more often by loyal and competent associates: in private practice the baby was of a higher average weight.

In explanation of the maternal mortality after Cæsarean section I may add this note of the two maternal deaths: (1) From primary shock in two and a half hours in a delicate young woman with a kyphotic pelvis; (2) from pulmonary embolism thirty-six hours after operation in a healthy young woman with a generally contracted pelvis. Both were clean cases: in each before the onset of labour the operation took place in the hospital theatre supplied with its full and regular staff.

In piling the two sets of figures supplied in the Table printed below for a balance there can be no hesitation in the maintenance of the claim for the valid position of labour-induction. The contrasts appear in stillbirths and maternal deaths. The neo-natal deaths bear almost the same ratio.

This last fact and the birth weights are consistent with the general acknowledgment of the familiar fact that induction babies are little if at all behind other babies. Some years ago in Liverpool at the Baby Show the first prize was given to an induction-baby born in the maternity hospital.

DISCUSSION.

Dr. DUNBAR HOOPER (Melbourne) said that Professor Briggs' paper disclosed his exceptionally ripe experience and his lucidly stated facts proved the wisdom of a carefully conducted induction of labour in suitable cases. Such "suitable" cases, when seen by younger and less experienced obstetricians, were often hastened to Cæsarean section. In Melbourne recently this very subject had been very keenly debated. There, as in London, experienced men had pleaded for the induction of labour at or about the thirty-sixth or thirty-seventh week of gestation, where the conditions would lead less experienced practitioners to attempt a Cæsarean section. Dr. Dunbar Hooper acknowledged that the temptation to perform a section was often inevitable to an obstetrician with aseptic surgical instincts. But the results to mothers and infants by an aseptically conducted "induction" were excellent, and, given similar indications, could not be beaten by the advocates for Cæsarean section, as proved indeed by Professor Briggs' personal experience.

Dr. AMAND ROUTH said that the President's address on the advantages of induction of labour in the slighter pelvic contractions over more surgical operations would prove most opportune and instructive, and would prevent further forgetfulness of its safety and value. Induction of labour after the thirty-sixth week of gestation for cases of moderate general contraction, by the method of Krause's multiple bougies, was free from danger to the mother, and, apart from unexpected circumstances, ensured living children, who with ordinary care could be reared, and would equal in physique and mentality other average children. He had made it a practice, if the child appeared to be relatively large or if the previous labour had been difficult, and the pelvis was only

slightly contracted, to induce labour ten or fifteen days before full term, and had never had cause to regret the results to mother or child.

Dr. HERBERT SPENCER said that the paper dealt with induction of labour: he pointed this out, as many were apt to confuse this with induction of *premature* labour, the foetal mortality of which was much higher than when induction was carried out at, near, or beyond term. He had in thirty-five years only known one patient die of infection after induction of premature labour: after a very prolonged labour and the occurrence of high fever and a rigor, the case was operated on by Cæsarean section, and died of peritonitis—the only fatal case he had had after Cæsarean section for contracted pelvis. Last year at University College Hospital they had had forty-four cases of induction of premature labour with two foetal and no maternal deaths. A child could be born alive through a 3-inch conjugate and grow up into a healthy woman. But usually he limited his cases to pelves with a 3½-inch conjugate and for these cases induction at the thirty-fifth, thirty-sixth or thirty-seventh week was usually required. The reason induction was not more often done was that continental practitioners had been misled by the formula of Dubois (at the seventh month (lunar) for a 7 cm. conjugate, at seven and a half months for a 7.5 conjugate, &c.). To induce labour at these dates was equivalent to craniotomy. He had induced labour in a very large number of cases. One woman had twelve living children in all of whose births labour had been induced by the speaker. He agreed with the President that Cæsarean section was done too often.

Dr. ANDREWS said that the President's paper was of special value at the present time when there was a danger of Cæsarean section being performed indiscriminately in cases of minor degrees of contraction of the pelvis. As one who believed that induction of premature labour was a most valuable procedure, not to be relegated to the scrap-heap as old-fashioned and out-of-date, Dr. Andrews welcomed the paper. There were two factors which led to the idea that induction of premature labour should be given up. One was that it was often performed too early, on account of the ancient heresy, which was still deeply rooted, not only among the patients but also among some medical men, that a seven months' child might live, while an eight months' child had less chance of living. The other factor was that still more patience was necessary in delivery of a premature child than in labour at term, and those who hastened the delivery of a premature infant by use of the forceps were usually disappointed with the result. The premature, incompletely ossified, head could not stand pressure which might be harmless in the case of a more completely ossified head at term. If induction of premature labour was done at the right time, and the delivery conducted with patience, the results were excellent, and were more likely to be repeated time after time than if Cæsarean section was performed, as the high rate of sterility after Cæsarean section, whatever the explanation of it, must not be forgotten.

The Essential Buttress of Practical Training in Obstetrics and Gynæcology.

By Professor HENRY BRIGGS, F.R.C.S. (President).

THE good sayings Johnson crowded into the "Lives of the Poets" remind us that "great thoughts are always general and consist in positions not limited by exceptions and in descriptions not descending to minuteness."

Again, said Johnson, "men have been wise in very different modes; but they have always laughed the same way." Also he said, "The artifice of inversion, by which the established order of words is changed, or of innovation, by which new words or new meanings of words are introduced, is practised, not by those who talk to be understood, but by those who write to be admired."

These quotations suggest rich themes for friendly debate: they are not tyrannical: they are full of thought and weighty in meaning. They are admittedly dogmatic but less so than much of the most impressive of teaching and leading.

In the Shakespearean comedies which were very tardily granted their own, the audience were concerned chiefly with the events: the deceits and mistakes and cross purposes were used to maintain suspense and prolong the interest: complications were introduced: much was sacrificed to the story. Fate and fortune revolve in the realm of comedy.

“O Time, thou must untangle this, not I,
It is too hard a knot for me to untie.”

An adventure may claim to be impersonal so long as it is confined to essentials.

To-day more than at any time during the last fifty years I feel isolated from medical students: sympathy and care for them rank higher than pastoral relics. Half a century's experience in medical school ought to bear the fruit of conviction and insight: courage ought to rise to opportunity.

The medical educational compartment is tightly packed: there are few of us who during recent travels have not often been pleasantly one of thirteen or more in one compartment of a railway train and who yet fail to grasp the essential and to eschew the extravagant.

Comedy and tragedy are antagonistic: history intervenes as a leveller in the old order of plays.

The surgeon's apprentice was often obliged to share the room and bed and basin with the qualified or unqualified assistant: both loyally viewed their chief as a clinician. The harnessing of the cob and the yoking to the gig, the surgery hours and the wait and the wobble over routine and emergency were as truly a whole time service as any of to-day's present or prospective units.

“What is fit for everything can fit nothing well” does not rule among the essentials, notably the fundamental practical training in obstetrics and gynæcology within the special department of the medical school.

The impulses and passions that shape departmental life are influenced by mutual appreciation and general criticism of materials and processes on which any lecture or book usefully asks the hearer or reader, “Have you not seen it or done it yourself?”

The most fatal of all faults—tediousness of description, is relieved by practical acquaintance at close range: no observed phenomenon is meaningless.

“The natural scientists,” said Henry Bradshaw, speaking from the regions of literature and bibliography, “have taught us all how we ought to work.”

DEPARTMENTAL EQUIPMENT.

The barren, the fallow and the cultivated portions of the field of knowledge that imply some real habit of first-hand learning equally invite practical effort that must lend life to our work. Practical equipment looms large among medical educational needs. It must be supplied.

In the earlier history of medical schools the more limited equipment in obstetrics and gynæcology, enriched, as it was, by the lives, qualities, and achievements of distinguished leaders, is still a precious heritage.

In 1898 the departmental heritage was by no means singular in Liverpool among medical schools: its equipment, adequate in the past, proved inadequate for the increasing number of medical students and their additional compulsory requirements. A practical class room and a laboratory were pressing needs.

In 1898 the class of practical obstetrics met at 9 a.m. in a cold joiner's workshop on a bare earth floor: for the students' use a leather doll and a solitary manikin, the latter of French design ingeniously fitted with a screw for flattening the brim of the pelvis were provided and placed on the bench, and antique instruments were lifted as required from a hamper on the floor. Antique instruments were less attractive in use than they are now in permanent preservation, labelled and hung, in their proper place within the glass case containing the historical collection of yearly increasing value for reference.

The initial laboratory accommodation was supplied by a temporary tenancy of a bench, later of a cubicle, at an easy rental, in the Thompson Yates Laboratory of Pathology. The laboratory service and research of Mrs. Budden (then Dr. Effie Prowse) earned a wide appreciation and was no slight incentive to the endowment of the Ethel Boyce Fellowship in Gynæcological Pathology, open to all students: as the first Ethel Boyce Fellow she was the first woman graduate in office within any department of the Liverpool Medical School in which she left enduring contributions to the equipment.

The welcome sympathy and energy of Professors Sir Rubert Boyce and Sir Charles Sherrington played a powerful part in the subsequent establishment of a departmental laboratory.

In 1898, the obstetrical clinical instruction issued its last flicker, dying of inanition.

Successive Deans of the Medical Faculty had preserved all letters, many laden with encouragements, promises, plans and prophecies from friends, critics and aspirants to possible and impossible places: these documents and the useful Regulations of other medical schools with the Reports of the General Medical Council are now bound for reference in two thick octavo volumes. The General Medical Council had offered the most loyal and efficient guidance and exerted the gentlest of patient pressure on all medical educational authorities.

The Medical Faculty in Liverpool, discouraged, if not dejected, by its own oft-repeated failures, realized its opportunity and placed on the shoulders of the Professor of Obstetrics, newly appointed, the re-organization of clinical obstetrics.

Personal responsibility in any sphere of public service is generally shunned, or shyly assumed by trained officials. Its acceptance risks censure even if its success allays widespread irritation and follows the failure of collective efforts.

The pinch was felt: the remedy was in sight: for five years the cumulative administrative and financial effort was borne in silence permissible in home, parish or diocese. The silence provoked curiosity, curiosity grew into suspicion, suspicion into rumour and rumour ended in an official inquiry which the University Council entrusted to the Vice-Chancellor, the late Sir Alfred Dale, a master of detail and of justice: humour and sympathy arose and thereafter towards the yearly financial deficit a University grant was given.

As a business undertaking the furnishing and maintenance of students' residential quarters, a University obstetrical hostel, Brownlow House, has been thoroughly justified as one of the means of terminating the compulsory journeys almost all the students made to Dublin and an odd one or two or three elsewhere each year. A new officer, an obstetric assistant, preferably a distinguished graduate with previous resident service in a general hospital, was installed in residence at the hostel directly to supervise the attendance of the students on their cases. A telephone was installed at each of the four district homes.

The year 1898 also saw the awakening to the most effective concerted action of the University and the Maternity Hospital with its affiliated District Homes of the Ladies' Charity, an ancient foundation dating from 1796.

This active association of University and Hospital affected the existing hospital staff: the old rule of ten-years tenure for each of the three honorary surgeons of the hospital was abolished and the age limit of 60 substituted; as an advance this step was unassailable: the addition, *ex-officio*, of the Professor of Obstetrics to the full honorary staff was consistent with advancing co-operation.

Last year increased accommodation for women students in their own residential quarters was satisfactorily and comfortably provided in one of the district homes adjacent to the hospital and in the latter a woman graduate of distinction, a second obstetric assistant, is now in residence.

The ample number of labours yearly attended in the clinic averaged 2,500.

During the first few years the obstetrical laboratory renewed its appeal for more pathological material: response to this was made by the private medical practitioners with their 2,000 cases, by the private practising midwives with their 13,500 cases, and by the staffs of all the general hospitals and Poor Law institutions.

From all these sources, and from similar sources at a distance, the Obstetrical Laboratory has been supported to the mutual encouragement of useful records; registration numbers and important details are entered in report books, so that questions about specimens may be answered at any future date or from a perforated page a duplicate report forwarded by request.

In a similar manner from hospital and private sources in Liverpool and district, gynæcological specimens, which are more abundant, are obtained: in their fresh state they are shown to, and handled by, medical students on all available occasions in the classes or in the laboratory: in the laboratory they are at the service of researchers and for the museum the suitable ones undergo preparation by the colour or other process.

The contributor always receives in a closed envelope a printed card acknowledging the receipt of the specimen and giving its register number.

The members of the Liverpool and District Trained Midwives Association have been loyally interested in the laboratory and practical class room: in the theatre they attend each year a series of scheduled post-graduate lectures on obstetrics, on ante- and neo-natal conditions, on diseases of the skin and on allied subjects selected by themselves: the museum of the department is open daily to their members: the members and officers have availed themselves of the option of holding any of their business meetings in the theatre or in the professor's room: in the latter the midwives' library is housed alongside the departmental library.

The admission of midwives for post-graduate study within a maternity hospital or within a University department is the equivalent of the higher education for general nurses in general hospitals with medical schools attached. Voluntary co-operation on an extensive scale has thus ensured a representative and adequate influx of material for post-graduate research and for the museum.

THE DEPARTMENTAL BUILDINGS.

The whole of the one-storey block of rooms on the west side of the passage leading in a northerly direction from the Medical School quadrangle to the Royal Infirmary grounds is occupied by the Department of Obstetrics and Gynæcology.

The ground plan is reproduced: it will be observed that there are no internal passages, a feature of converted rooms in old buildings such as these, the only remaining portion of the original Medical School.

The door from the passage referred to above opens into the Museum.

The Museum.—In two subdivisions of almost equal extent: (a) the Gynæcological Section on the right; (b) the Obstetrical Section on the left.

(a) *The Gynæcological Museum.*—Its walls are closely covered with life-sized water colour drawings of the recent appearances of pathological conditions of the vulva, vagina, uterus, Fallopian tubes and ovaries: a few of the bladder, ureters, kidneys, stomach, intestines and rectum in their appropriate relations: there are many photographs and photomicrographs; some of these illustrations are back to back enclosed in hinged frames which can be easily unhooked for demonstration purposes in the lecture theatre and practical class room. Every illustration has its history and description and almost every one the after-history to complete the educational value. The Museum jars are displayed on parallel tiers with intervening alley-ways. At the beginning of each section, in a vertical frame, the bound sectional type-written catalogue stands: this economic subdivision of the catalogue lightens its weight and multiplies the utility of one whole catalogue of which a carbon copy is always preserved.

(b) *The Obstetrical Museum.*—A lower-half skeleton of a scolio-rachitic dwarf; conjugate, $2\frac{1}{4}$ in., lodges its object lesson of prejudiced refusal, in 1889, of Cæsarean section (*Lancet*, 1890, i, p. 699).

Five large upright glass cases: (1) Obstetrical and Gynæcological instruments and pessaries, particularly those in common use, each labelled and the action of those more rarely used illustrated so that students may freshen their recollections. (2) and (3) Collections of plaster of Paris casts of stillbirths to illustrate presentations and their effects; the sites of the puffy tumour, mouldings and other recognition tests. (4) and (5) Normal and abnormal pelves; contraction and expansion; the split pelvis; fœtal skulls ranging from twenty-two weeks to forty weeks; depressions and fractures of the fœtal skull. A type-written description is legibly placed on or near every specimen. The specimens preserved in jars are displayed and catalogued similarly to those in the Gynæcological Section. The first jar contains the uterus and appendages, with the uterus laid open to show the menstruating endometrium from a girl who died from acute pneumonia: following this, specimens illustrating ovulation, pregnancy, the early decidua and the early ovum; the fœtus at the successive months; the full-term uterus and contents *in situ*: the normal cord and placenta; then the diseases and abnormalities of pregnancy, labour and the puerperium; a large series of abortions and moles; specimens and casts to demonstrate utero-fœtal adaptation: the eclamptic organs; induced abortion or labour with the bougies in position. On the walls are water-colour drawings, photographs, photomicrographs and radiographs of obstetrical interest such as fœtal oversize, induction of labour, facial paralysis, intracranial injuries, fractures of limbs, puerperal infective lesions with temperature charts attached: moles and their minute structure: all the illustrations are in fixed or movable frames according to requirements. A collection of wax models of the female generative organs, the uterus and its contents during pregnancy and labour; malpresentations, placenta prævia: the whole of these models in wax, in good preservation to-day, were a gift in 1860 from the late Mr. Edward Bickersteth: they greatly interest midwives and nurses. A section is devoted to the care and management of the neonate:

the stomach at seven, eight and nine months is on the top of a case containing ancient and modern feeding bottles, pipettes, syphons, nipple-shields, breast pumps with critical remarks thereon. Near by on one side a Soxhlet's sterilizing apparatus, on the other an incubator with directions for its use: and over all a short bound type-written explanation of the principles of infant feeding.

The Practical Class Room.—The provision of twelve manikins on twelve separate tables and a plentiful supply of modern obstetrical instruments in a special glass case and of embalmed fœtuses stored in two metal tanks, accessible to all students, encourages the training in operative midwifery. The manikins are copies of the normal and abnormal pelvis in the Museum: they stimulate practical observation and test manipulative skill; the damaged sacral and lumbar vertebræ at and above the brim with a $2\frac{1}{4}$ in. conjugate impress maternal risks on the operator; a 3 in. conjugate is not to be lightly regarded after its manikin and an average full term fœtus have been offered as an extraction test after perforation. The manikins have been more fully described in the *British Medical Journal*, August 14, 1920. A row of classical pelvimeters, each with its label, hang in a row above a row of photographs of the normal and abnormal pelvis. For external pelvimetry the ordinary beam calipers of the engineer are recommended and for internal pelvimetry the fingers to be trained by practice on the manikins. On a long bench lies a row of earthenware tanks containing specimens illustrating hydrocephalus in every variety of presentation; placenta prævia in varying degrees; prolapsus placenta; meningo-encephalocele as a misleading presentation; accidental hæmorrhage; fibroids and labour; rupture of the uterus: immediately over every specimen on the wall is hung an enlarged framed photograph with explanatory notes. A similar bench and the opposite wall are devoted to anatomical preparations and illustrations. A long glass case contains the historical collection of obstetrical and gynæcological instruments and appliances. Beyond the glass case on tiers of shelving similar to those in the Museum and in a wall cupboard the section of fœtal malformations and diseases reminds the student in his practical work of their possible importance in practice: a noteworthy instance is a cast of a fœtus with a catheter introduced through a spina bifida to drain the large after-coming hydrocephalic head.

The Laboratory.—The material received from the sources already indicated is examined by the staff as a routine and is available for research in special subjects: it offers to the students special daily opportunities of acquiring familiarity with morbid material. Suitable specimens are mounted for the Museum. Thirty M.D. theses have been accepted at our own and other universities.

The Professor's Room.—Students have been allowed its use for study: the staff and visitors have met each afternoon at tea: it has served as an office and library and as a temporary storehouse. The portraits on the walls include three of the former staff who gave their lives in war service.

The Dark Room.—Here the preparation of photographs has entailed an almost whole-time service.

To the preceding outline of the departmental equipment a few remarks should be added: an appreciation—a most grateful one—is offered to Dr. H. O. Forbes, formerly Curator of the City Museum of Natural History. on his experienced guidance the allocation of the walls entirely to illustrations, and the display of materials in jars and glass cases on tiers of shelving.

accessible from both sides, have been daily reminders of the great debt the Museum owes to him.

A departmental museum by the force of its situation excels in function a similar section of a grouped museum: it lies at hand amid the activities of teaching, study and research; it cements the truth and suppresses the phantasm: within special publications it is realized less as a museum and more as an equipment: in its display of the usual, the more and the less probable, down to the rare specimens, it invites attention at the right moments and makes more lasting impressions with the least expenditure of time by students.

As it now stands, the departmental equipment silently testifies to the sound character of the extensive researches by a continuously devoted staff ably working without invariably seeking a glorifying limelight.

Within the sphere of the Obstetrical Clinical School unostentatious assistance and efficient pilotage have been parts of the lay services of Professor Wilberforce, the Professor of Physics: throughout as arbitrator he has succeeded in promoting rights on every side: he became the University Representative on the Board of Management of the Maternity Hospital after important points had arisen at the primary instigation of the Ladies' Committee, such as the too limited admission of medical students to ward visits and operations owing to the too lavish admission of pupil-midwives: here was a genuine grievance which was soon settled by sending important cases to other hospitals: after Professor Wilberforce's accession less drastic remedies were equally efficacious in the establishment of reasonable facilities for medical students, whose conduct in the wards and at Brownlow House for twenty-three years has been marked by the absence of any serious breach of discipline and by the maintenance of club-like cordiality.

A summary of the total departmental advance ought also to describe the previous occupation of students' residential quarters by the factotum of the Anatomical Department with his six stalwart children: the patience and forbearance of Dr. J. H. Willett—the first obstetric assistant—during the early months of his residence at Brownlow House enabled the Department of Obstetrics to provide pleasantly elsewhere for the family without financial loss to them and to open the whole of Brownlow House for the purpose it now serves.

To each of the researchers in the laboratory this report cannot give adequate expression of their faith, loyalty and service which are written on pages of catalogue, on numbers of specimens and in an abundance of special literature.

An exception is warranted in the case of the present Lecturer in Obstetrics and Gynæcology, Dr. R. A. Hendry, who has won his spurs many times over in many years of office: his laborious published research on the bleeding uterus and his aptitude in practical obstetrics and in gynæcology ensure for him a future full of distinguished light and leading founded on vast and deep researches and repeated useful experiments. His activities, it is hoped by all, will endure to the immense benefit of the Department which to day is reporting its position after the twenty-three years' tenure of the Professor retiring on September 30, 1921, and offering on the part of the whole departmental staff a hearty greeting and loyal welcome to the new Professor.

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Section of *Odontology*.

President—Mr. MONTAGU F. HOPSON, L.D.S.E.

The Treatment of Pulpless Teeth.

By J. B. PARFITT, L.R.C.P.Lond., M.R.C.S.Eng.,
L.D.S.R.C.S.Eng.

THE treatment of pulpless teeth is perhaps one of the most pressing problems confronting dentistry. It is a problem so large that it will be impossible to do more than touch those aspects of it which appear to be of more immediate clinical interest.

That the treatment of pulpless teeth has so far been empirical need arouse no feeling of shame. Empirical methods almost always precede rational, for human needs are so pressing that they demand immediate attention and cannot wait for the application of a fully worked-out scientific system. If, however, our art is to attain to its adult stature empirical treatment must give place to rational, and we all know that for some years past our literature has been full of articles dealing, in the truly scientific spirit, with the subject of pulpless teeth.

Should pulpless teeth be retained at all? This question involves two others, the utility of the pulp and the value of a pulpless tooth. In these days we are loth to admit that any organ is useless, whether it be appendix, tonsil or tooth pulp. So many organs, the use of which was in former times unknown, have been proved to possess most important functions in the animal economy, so that we should not be surprised to discover that some new and altogether unsuspected rôle was performed by the tooth pulp, especially as its copious blood and nerve supply and cellular structure betoken anything but a degenerate organ. However this may be, and however important the tactile function of the pulp in connexion with the co-ordination of the movements of mastication, we know that so long as the pulpless tooth is free from infection it does useful work and is without danger to its possessor. Such a tooth should hardly be called "dead." Its dentine is dead and as such is in reality outside the living body, the boundary of which is the junction between the dead dentine and the living cementum, which is an integral part of the tooth.

It is of the utmost importance to know whether the interior of the root of a pulpless tooth, originally free from infection and cut off from access of organisms from the oral cavity, will become infected by way of the living tissue or remain permanently healthy. If infection is the inevitable, or even the most probable, fate of all pulpless teeth it is obvious that extraction is the only rational treatment.

Infection of a tooth root by way of the blood is possible; modern research has shown that living organisms are present in the blood more frequently than once supposed. The fear that such organisms may settle in and around the

apices of pulpless teeth is quite reasonable, and the systemic origin of apical septic foci has been held to be proved by their very frequent occurrence round carefully treated roots, by the large proportion of infection by streptococci of the *viridans* type and by actual experiments such as those of Novitsky [6]. Due weight must be attached to these reasons but they are by no means conclusive.

As to the frequency of septic foci around the apices of treated dead teeth, it will probably be some time before root canal technique is sufficiently uniform to warrant important conclusions being drawn from the results of any series of unselected cases. Any judgment based on the type of organism found in the septic focus appears to be as little conclusive, and Novitsky's experiments with boiled implanted teeth do not seem to throw much light on the condition of a tooth with healthy cementum and alveolo-dental membrane. On the other hand some pulpless teeth appear to be perfectly sound, both on radiographic and clinical evidence, and show no sign of any pathological changes when extracted for reasons unconnected with their own condition.

While living organisms have been found so frequently in the animal body, research has shown that the powers of the protecting mechanism are normally sufficient to cause their destruction. Hence I think that until the best modern methods of root canal technique have had a lengthy trial, and have definitely been found wanting, it is reasonable to conclude that blood-borne infection is the exception and not the rule.

The rational treatment of a pulpless tooth may then be defined as the sterilization of existing infection, the removal of internal soft tissue and its replacement with a root-filling that will neither irritate the living parts nor permit infection of the dead ones, including the root-filling itself. Removal of infection is essential, for without it the remaining treatment may be worse than useless.

For the purposes of treatment of infection pulpless teeth may be divided into three classes: (1) Those the pulps of which were alive at the commencement of treatment; (2) those with pulps dead but with cementum and pericementum alive in their entirety; and (3) those with partially dead apical cementum and detached and damaged membrane.

(1) The first class comprises those teeth which the operator devitalizes himself, which, having exposed pulps, are all more or less infected.

Our first aim is to kill the organisms in the tooth without driving any of them into the peri-apical space. Of course the peri-apical space may be already infected while the pulp is still alive, but at any rate we must not increase the amount of infection.

The method of devitalization is important in connexion with the possible spread of infection; "pressure anæsthesia" almost inevitably drives infective material through the apical foramina; arsenic, whatever may be said against it on other grounds, probably effects sterilization within a certain radius of action; destruction under anæsthesia by nerve blocking neither helps our efforts at disinfection nor does it hinder them.

As infection is present, merely "aseptic" methods, however carefully employed, are insufficient; some active germicide must be used. The qualities required of such an antiseptic are germicidal power, rapidity of action and absence of irritation to the living tissue.

Some of the higher members of the phenol series appear to be the best agents available; their antiseptic action is greater than that of phenol itself [1] while they are less caustic and less soluble in water, and so less likely to exert

any destructive action by passing through the foramina into the peri-apical space. The ordinary tricresol, which is a mixture of the three isomeric cresols, answers our purpose very well.

The technique of its employment is very simple. The tooth is isolated with rubber. If the cavity is so situated that it is impossible to insure against leakage it should be enlarged sufficiently to give retention and a water-tight filling of cement inserted. Rubber-dam can then be applied and the pulp approached through some more convenient channel. The whole field of operation is wiped over with the full strength tricresol. Any temporary filling is then removed and the tooth cavity immediately flooded with tricresol. All instruments and material introduced into the tooth cavity must be sterile, so that no further infection be carried to the inside of the tooth from start to finish. This requirement is obvious and is only mentioned to emphasize the necessity of a practical course of bacteriology being a compulsory part of the dental curriculum, so that every student may acquire the "aseptic sense" from his own experience of laboratory methods.

Cleansing of the cavity and removal of the remains of the pulp are effected in the pool of antiseptic fluid, débris being wiped away from time to time with wool saturated with the same liquid. Every instrument passing through the tricresol carries a portion of it into the deeper parts of the tooth, and as the remains of the pulp are removed tricresol runs in and occupies the vacant space.

When the canal is cleared of débris and is full of tricresol a paper point soaked in the same fluid may be placed in it, and covered with a water-tight cement filling; a small quantity of paraform may with advantage be included in this "dressing."

At the next visit the tooth cavity is opened with the same precautions as before and the paper point removed under a pool of tricresol, so that the canal is never left open or dry from the beginning of the treatment to the end. It is now ready for filling.

(2) The second class includes those pulpless teeth which the operator finds "dead" but which show no apical change in a radiogram and have never troubled the patient. It is not safe however to assume that the peri-apical space is free from infection although it may be so in some cases. The researches of Mr. Payne [7] and of Dr. Grove [2] into the deposition of cementum in the apical foramina are interesting in this connexion. Presumably nature hereby interposes an efficient barrier between the dead parts and the living, and in fact makes a natural root filling.

Our aim in these cases is to sterilize the dead parts of the tooth before manipulation, which otherwise would very likely cause extension of infection. In order to do this the entire dead part must be saturated with an antiseptic of sufficient strength to kill the organisms present. This is very difficult for several reasons, one of which is the structure of the tooth root, which may not only have aberrant pulp canal branches but is permeated by dentinal tubes, and all those spaces may be full of organisms, so that the whole root is septic throughout. Another difficulty is the old one that tissue cells are as vulnerable, or more so, than bacteria, and may suffer serious damage while disinfection is in progress.

Neither of these difficulties should be insurmountable. Wherever organisms can penetrate, a diffusible substance soluble in water can certainly follow them, and it ought to be merely a matter of strength of the antiseptic and time of its action for sterilization to be complete. Laboratory experiments have

shown that formalin has the power of diffusing right through dentine and exerting an inhibitory action on the growth of organisms in the medium surrounding the tooth [8]. The aniline dyes also penetrate the tooth substance. This can be shown in the living subject by sealing such a substance as proflavine in the pulp canal of a tooth; after a few days its yellow colour will appear under the surface of the visible part of the root, and I have one or two specimens of extracted teeth that were so coloured while in the patient's mouth. Hence, given sufficient time, sterilization of the dead dentine should be possible.

As to the other difficulty, which is very real, we are much favoured by the different physical conditions of the dead dentine and of the living tissues of the peri-apical space, including the cementum. In the pulp canal, and the channels opening out of it, there is a condition of stagnation, hence the concentration of antiseptic is not interfered with by the circulation that goes on in the living tissues. The only interchange between the fluid contained inside the tooth and the tissues outside is by way of the pulp foramina and such minute communications as exist between the terminations of the dentinal tubules and the canaliculi of the cement in the apical third of the root. On the outside the cementum and alveolo-dental membrane are bathed in the living tissue fluids which are constantly circulating and so carrying away any material which diffuses out of the inside of the tooth. Hence the concentration of antiseptic on the two sides of the dividing line between the living and the dead is almost bound to be very different, and while I cannot give any figures to support the statement, I think that both clinical experience and the specimens of teeth stained *intra vitam* indicate that, with reasonable care, a sufficient concentration to kill the contained organisms can be maintained inside the tooth without the strength in the peri-apical space rising to a point dangerous to the living cementum.

A convenient way of applying formalin for this purpose is to seal about $\frac{1}{4}$ gr. of paraform in the pulp cavity, or over the aperture leading into it. Paraform is a polymer of formaldehyde, and at ordinary temperatures tends to dissociate into that substance until a condition of equilibrium between the two bodies is reached. The strength of formalin maintained will be greatest near the paraform and will fall off in the direction in which it is free to diffuse away into the surrounding medium, namely, at the apical foramina. This one application of paraform sealed in with a watertight filling is therefore equivalent to repeated dressings with ordinary formalin of comparatively low concentration. No attempt is made to disturb the contents of the pulp canals at this first visit, but after a few days the tooth may be treated as if it were a member of the first class, and in my own experience such treatment is not followed by any trouble, whereas, had the canals been cleaned out at the first visit in the same bold fashion peri-apical inflammation would almost certainly have occurred.

If there should be infection of the peri-apical tissues, without permanent damage, the removal of the large mass of infection in the pulp canal and the diffusion of antiseptic of low concentration through the apical foramina would probably be the best treatment; any more drastic measures might precipitate the mischief we were trying to avoid.

(3) As to teeth of the third class—those with more or less dead apical cementum—the structure and relations of the tooth apex are such that it is difficult to imagine the possibility of sterilization of the cemental aspect by any treatment through the canal. The reduction of concentration of the antiseptic

outside the foramina, so desirable when the cementum is alive, is here a positive disadvantage. This difference between the condition inside the tooth and that in the peri-apical space is shown by one of the specimens already referred to, which had an application of proflavine for a week before extraction. The foramen was quite patent, so that there was no bar to diffusion through the end of the root, and yet, while the dentine is deeply stained, the cementum of the apex is still perfectly white, showing that the dye was not in contact with it in any useful concentration.

For this reason and in view of the possibility of systemic infection by such teeth, the only rational treatment may be to get rid of them. Clinical experience, however, suggests that when there is no evidence of systemic infection certain teeth of this class may be treated with sufficient prospect of success to warrant the performance of the operation. Many operators have treated them, both by way of the canal and by root resection, and produce radiographic evidence of improvement in the bony surroundings of the root apex.

It is difficult to imagine any action of ours resulting in a sterile apex, but we must not forget the patient's own immunity mechanism, and, as Mr. Livingston has shown [4], it is evident that in many cases nature does effect what may reasonably be called the cure of a radicular abscess.

Probably the chances of successful treatment depend upon the concurrence of several factors, such as the efficiency of the resisting powers of the patient, the amount and nature of the damage done to the tooth apex and possibly the nature of the organisms present. It appears that certain teeth with an external sinus respond to treatment as if they had received less damage than those with the so-called blind abscesses.

If there is reason to suspect systemic infection in our present state of knowledge all teeth of this class should be removed, although the mischief has already been partly done, and although we run the risk of sacrificing innocent teeth.

How are we, as dental surgeons, to know whether a given patient is suffering from systemic infection or not? Dr. Lowe has discussed this question in the *British Dental Journal* of August 15, 1921 [5] and points out that much light can be thrown on the case by a blood examination. The presence of leucocytosis will indicate the presence of infection and the proportion of eosinophil cells will show how far the patient's powers of resistance are rising to the occasion.

Expert diagnosis of the patient's condition, including blood examination, is the rôle of the physician, and in any individual case his opinion as to the patient's condition should determine the dental surgeon whether he shall advise the removal of all dead teeth, or only those that show radiographic evidence of bone changes, or whether on the other hand, some attempt shall be made to treat some or all of them conservatively.

After sterilization comes the clearing of the canals. The researches of Professor Hess [3] and others have shown that most canals are so tortuous or so branched that their complete clearance by any instrumental method is impossible; consequently, what is to be the ultimate fate of uncleared portions of canal which open on to the cemental surface of the root?

Many pulpless teeth, some of which must have had aberrant canals, appear by clinical and radiographic evidence to be in sound condition. I have already referred to the possibility that in some cases, at any rate, cementum grows in from the outside and partially or entirely occludes the foramina. When this occurs nature has itself constructed a root filling at the termination of the

canal, which is just the place where it is most wanted. When this has not happened, and still the tooth has remained free from infection, we can only conclude that the patient's own protective mechanism has kept the minute portion of dead material free from infection. Probably there is some question of quantity concerned, and it may happen that nature is able to keep a short length of canal clean, but not a long one, just as she can at times destroy bacteria up to a certain number or concentration, but not beyond that limit.

In any case the soft contents must be cleared out as far as possible, provided no risk be run of doing damage beyond the apical foramina. It has been well said that living tissues are often more resistant to the action of bacterial toxins than to that of gross chemical caustics, and there is little doubt that pushing strong acids or alkalis out through the foramina may do far more harm than leaving a small amount of sterile pulp tissue at the extremity of the canal itself, especially as that extremity is often constricted in diameter.

The problem of filling the emptied canals should not be difficult. Wherever instruments have penetrated the canal and removed pulp tissue from it they should be able to introduce some material to take its place. The method of cleaning already referred to leaves the canals full of trieresol, and the filling of them by substitution has, at any rate, the advantage of simplicity. This involves the use of some material which can be introduced in a plastic condition, and is miscible with the fluid already in the canals. Such a material is a pasty mixture of zinc oxide (or artificial dentine), trieresol, and carbolized resin. This material can be loaded with any desired antiseptic, such as paraform in any quantity up to say 10 per cent. If a small portion of the freshly-made paste be introduced into a canal on a fine flexible broach, it quickly mixes with the liquid already present, and repeated introduction of paste will thicken the contents of the canal to any desired degree. A cone of metal or gutta-percha may with advantage be introduced at the completion of the operation.

Such a paste will in time set into a moderately firm mass and appears to form a very satisfactory root filling. The total bulk of the whole material is so small, and is in contact with the living tissue at so small an aperture that no irritation appears to be produced by the paraform it contains, and yet, expressed in terms of formaldehyde, this amount of paraform is equivalent to an amount of a 25 per cent. solution of the strong formalin of commerce, sufficient to occupy the entire space of the canal.

Many writers advocate the forcing out of root-filling material through the foramen; reason and experience, however, seem to point to the limitation of contact between the foreign material and the living tissues to the smallest area possible, namely, the actual orifices of the foramina. Dr. Howe's silver method is much recommended, and may well prove a valuable adjunct to dentistry.

Transparent preparations of teeth so treated out of the mouth show that the black deposit is far reaching. It has been stated that the silver method specially selects the septic parts of the tooth substance; this would be difficult to prove, but it seems to be a fact that macerated teeth take both the silver stain and aniline dyes better than fresh teeth that had healthy pulps previous to their extraction.

The antiseptic action of the silver treatment appears to be unquestionable, as we might expect when the reduction is effected with an agent which is itself a strong antiseptic. In fact there is considerable danger of damaging the peri-apical tissues, if formalin be chosen as the reducer.

Some experiments made by Mr. Bulleid in the bacteriological laboratory of

Guy's Hospital suggest that the precipitated silver is itself an antiseptic. The experiments were made with precipitate which had been well washed and boiled and had thereby been turned into a black powder. The fresh, chocolate coloured, finely divided and perhaps partly colloid silver, would probably be much more active than the material actually experimented upon, which was intentionally freed from the reducing agent.

Should the method be styled root "filling" or only root "treatment"?

If reduced in a test tube the silver forms a bulky precipitate of a light brown colour. Under light compression it occupies about 50 per cent. by volume of the original ammonia silver solution. When diluted with the reducing agent under mouth conditions, the bulk must be considerably less than this amount. It may, of course, happen that, under favourable circumstances, the silver may form a coherent coating at the periphery of the canal, and so occlude the foramina in a perfectly efficient manner.

In a test-tube experiment it was found that by floating the reducing agent over the silver solution so that the two did not mix together, the reduced silver formed an impervious layer which prevented further access of one fluid to the other, even after the lapse of several days. On breaking through this layer and mixing the liquids the usual copious precipitate was immediately formed. This suggests that it would be better for the reduction to take place from without inwards rather than from within outwards, which is the probable event when the usual technique is practised.

Mr. Bennett, of the Chemical Teaching Staff of Guy's Hospital, suggests that, since the ammonia silver solution is reduced with extreme ease, the organic material present in the tooth would probably of itself be a sufficient reducer.

If reduction could be effected by the organic material of the tooth and the tissue fluids at the entrances of the foramina, it might quite well result in the deposition of a coherent layer at the periphery of the silver solution, which would not only line the canals, but also, as in the test tube experiment, bridge over all the openings with an impermeable sheet of solid metallic silver.

DESCRIPTION OF SPECIMENS SHOWN.

- (1) Rubber casts of pulps made by Hess's method.
- (2) Pulp of central incisor showing side branch reaching to exterior of root.
- (3) Teeth stained with proflavine: (a) *Intra vitam*; (b) freshly extracted; (c) macerated.
- (4) Teeth stained with brilliant green.
- (5) Root fillings made by substitution method.
- (6) Tube of Howe's silver solution and reducer showing formation of impermeable sheet of silver at junction of the two fluids.
- (7) Transparent preparations of fresh and macerated teeth showing penetration of silver by Howe's method.

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Section of Odontology.

President—Mr. MONTAGU F. HOPSON, L.D.S.E.

A Compound Composite Odontome.¹

By GERALD HARBOROW, L.D.S.E.

THIS odontome was growing in a boy, aged 4. He was brought to me at the Metropolitan Hospital for treatment on account of the swelling in the lower incisor region. It was just noticeable from the outside and gave the child the appearance of having a swollen lip. The swelling was first noticed by the parents when the boy was aged 8 months, and was incised three times at different intervals but with no result as to diminution in size of the swelling, which gradually increased. The growth was situated in the right side of the mandible, extending from the symphysis menti to the deciduous canine (3½ cm. over the swelling) and from the gum margins to the bottom of the sulcus (2½ cm.).

On examination a hard smooth non-fluctuating swelling was present. The right deciduous lateral incisor was slightly displaced, but this was the only tooth which seemed to be affected by the growth, all the temporary teeth being present and normal. It was impossible to make a definite diagnosis before operating without a radiograph. There was no history of pain, and the child at the time was quite healthy.

I had a skiagram taken and it showed a mass of denticles; by this I diagnosed a compound composite odontome.

The tumour was removed under chloroform and ether anæsthesia; this consisted in making an incision including the whole extent of the upper border of the swelling, and reflecting the muco-periosteum together with the levator menti, with a periosteal elevator. The bone was then chiselled away and the odontome exposed; this was successfully done without disturbing it. The operation was done by Mr. Peter Daniel. The bone covering the growth was quite thin; it was only 1 mm. in thickness near the symphysis, being slightly thinner in the canine region. There was no fluid nor visible cyst-cavity. There was no embracing capsule or lining membrane, each denticle or mass was attached to the posterior wall by its own capsule. The largest mass was situated directly under the thinnest portion of the outer wall near the canine. The skiagram of this mass shows it to contain several denticles. One denticle was lying at the bottom of the cavity in a crypt by itself near the symphysis. At the root of the central incisor a small denticle was situated, but was not in direct contact with it. The posterior wall showed signs of septa corresponding to the separate masses of the tumour. The central and lateral deciduous incisors were extracted, as they were in relation to the cavity. Their roots showed no absorption. The canine was not extracted as it was

¹ First communicated May 31, 1920. Publication postponed to enable Mr. Howard Mummery to make further histological investigation.

covered by its own bony socket and not in communication with the cavity. The cavity was allowed to heal by granulation.

The only treatment prescribed was frequent irrigation by syringing with hydrogen peroxide vols. 5. The child was discharged from hospital in a week, the wound healing well.

There was no family history to account for any departure from the normal. His two sisters have normal dentition, except that the maxillary first molars show over-developed cingula.

This odontome was removed five years ago and I had radiographs taken at different periods and was then surprised to find teeth growing in the space previously occupied by the tumour, because the cavity was thoroughly cleared and it did not seem possible that the germs of the permanent dentition could be in the remaining bone.

The latest skiagrams show the canines in position for eruption. The models of the mouth show the present condition of the teeth. The right lateral incisor is in malposition, due probably to displacement of the tooth germ caused by the growth. I have since extracted the milk molars and right canine; the canine shows no absorption but a septic condition around the apex caused possibly by infection when the wound was open.

As all the incisors and canines are present in the second dentition and the milk dentition was also present and normal, it is reasonable to think that the odontome was caused through abnormal development of a supernumerary tooth germ. I will give reasons to support this statement later. This case has some very interesting features, viz. :—

- (1) The growth of the teeth after the removal of the tumour; the early age at which the swelling was first noticed.
- (2) The first compound composite odontome reported in a subject so young, the next youngest patient being aged $8\frac{1}{2}$ years [1].
- (3) The fortunate possibility of being able to follow the case up until the permanent teeth erupted.

In the twelve cases reported in man in "The Report on Odontomes," two only occurred in the mandible. One was in the incisor region, the other in the molar region. Therefore, by deduction, the mandibular incisor region is an unusual site for these odontomes.

The cases recorded have mostly come under observation when suppuration has been a complication, so that it has been impossible to describe the capsule and the arrangement of the denticles. In one case recorded by Sir John Bland-Sutton [2], he refers to a soft mass of vascular tissues containing a large quantity of irregular pieces of bone. There was no tooth tissue at all, and although this case is classed with this kind of odontome, I do not consider its description justifies its place in this list. It certainly does not show any resemblance to my case.

In Mr. A. A. H. Johnson's case he states that six denticles were present in one or two cysts. This must refer to the capsules which surrounded them, as there appeared to be no cystic formation about my odontome.

A case under the late Mr. Stanley Boyd does not appear to be on record. It was shown in a clinical lecture at Charing Cross Hospital and referring to the notes I made then and relying on my memory I am certain the odontome comes under this class. It was in the region of the maxillary right central incisor, which was present but very late in erupting. The swelling was above this and high in the sulcus. The patient, a girl, was aged 20. Several denticles were removed from the cavity.

10 Harborow and Mummery: *Compound Composite Odontome*

I favour the assumption of the supernumerary germs giving rise to this condition because in my case all the teeth in the temporary dentition were present and are present in the permanent set. In Mr. Boyd's case just referred to, the permanent central was present.

In Dr. J. Ward Cousins' case [3], after an extensive operation for removal of the odontome "a tooth mass is seen surrounded by thickened gum" in the position of the right second and third molars in the mandible.

In Mr. Tellander's case [4], eight months after operation "a tooth was making its appearance in the spot from which the last of the teeth were removed," and two years after, a model taken of the mouth showed in the former position occupied by the growth "a supernumerary and a bicuspid tooth." It is possible that the supernumerary tooth was a denticle from the odontome and erupted in the usual manner.

In a case reported by Dr. M. H. Cryer [5] in which the tumour was in the maxillary central position, the central incisor was high in the palate, lying horizontally but descended into its normal position after the operation. In another case of his, in a skull, the central is shown lying horizontally, in the palate. The eruption undoubtedly had been arrested by the odontome.

The other cases reported do not mention teeth erupting after operation. Thus it seems to me quite reasonable to think that compound composite odontomes are caused by abnormal development of supernumerary tooth germs rather than by abnormal development of normal tooth germs.

Mr. Mummery kindly made sections of this tumour for me and will demonstrate them to you.

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HISTOLOGICAL REPORT BY J. HOWARD MUMMERY, C.B.E., M.R.C.S.

WHEN Mr. Harborow made his communication last year on this very interesting case of a compound composite odontome, only a small portion of the growth had been examined microscopically, but he afterwards kindly consented to my request that the tumour should be decalcified and sectioned, thinking with me that it would be of more value in microscopic slides than as a museum specimen.

The investigation gave some very interesting results, the chief one of these being the demonstration of the presence of the sheath of Hertwig in connexion with one of the larger denticles.

In fig. 1, a portion of the tumour is shown containing numerous denticles and the largest one shows the epithelial sheath extending along the margin of the pulp. Under higher magnification (fig. 2) the structure of the sheath and the epithelial cells of which it is composed are very clearly seen. This is a very interesting confirmation of the statement of von Brunn that all dentine is laid down under the influence of epithelial cells; as he says, "Where there is no epithelial sheath there is no dentine." In this case although the dentine is present there is no enamel; the sheath must, however, have proceeded from the epithelium of the tooth-band and enamel may also have been formed in this denticle, but if so, it has been encroached upon by the mixed tissue of which the odontome is chiefly composed and has disappeared. The enamel has

remained in many places after decalcification. It would appear that it had not been completely calcified, as sections cut without decalcification show the structure of the enamel perfectly, but it took the stain with greater ease even than the dentine.

A curious mass of partially developed enamel is seen in fig. 3. At the upper margin the fibrous capsule is seen enclosing a rudimentary enamel organ and a mass of calcospherites of various sizes. This appears to be an abortive attempt at enamel formation. The section was stained with orange rubin, which coloured the mixed tissue of dentine and cement a brilliant red while the mass of spherites I have described was of a duller brownish colour, appearing to indicate a different tissue structure and probably showing that the whole of this central mass of spherites is of epithelial origin.

At one margin of the tumour a developing tooth is seen projecting from the surface showing its imperfect enamel organ and calcified enamel and dentine (fig. 4).

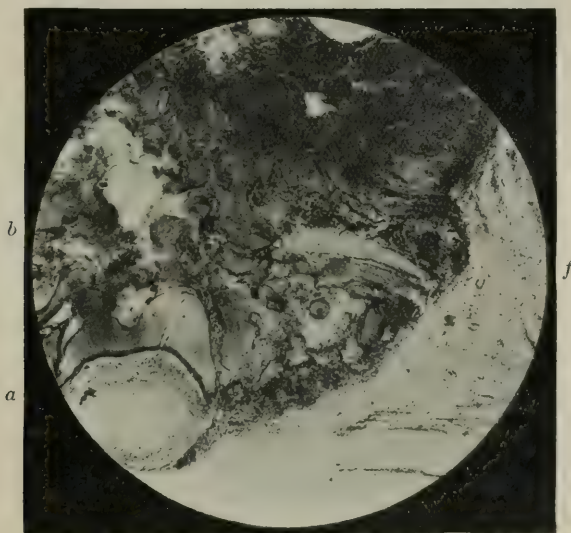


FIG. 1.—A large denticle showing at *a* on the right the sheath of Hertwig on the outer side of the dentine pulp; *b*, mixed tissue and denticle; *f*, fibrous capsule.

Fig. 5 shows a portion of the growth crowded with small denticles. In one of these the fibrous enamel is in position and has only been partially decalcified by the acid.

Fig. 6 shows the margin of one of the larger denticles with remnants of enamel projecting from a mixed tissue containing many large calcospherites. A very marked feature in this specimen is the very complete fibrous capsule in which are seen scattered epithelial strands and larger masses of epithelial cells (fig. 7).

This odontome is remarkable clinically in being, I believe, the only recorded case in so young a subject and one also in which all the teeth were erupted. The only apparent abnormality in the teeth was a smooth nodule of enamel projecting from the mesio-labial aspect of the central incisor and blending evenly with the enamel of the crown.

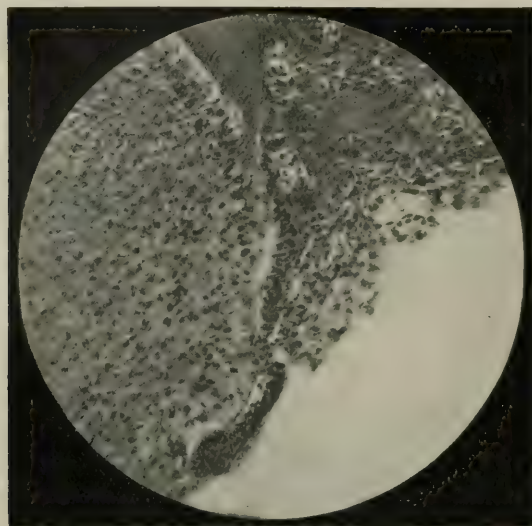


FIG. 2.—The sheath of Hertwig enclosing the dentine pulp from the denticle (*a*) in fig. 1. ($\times 150$.)

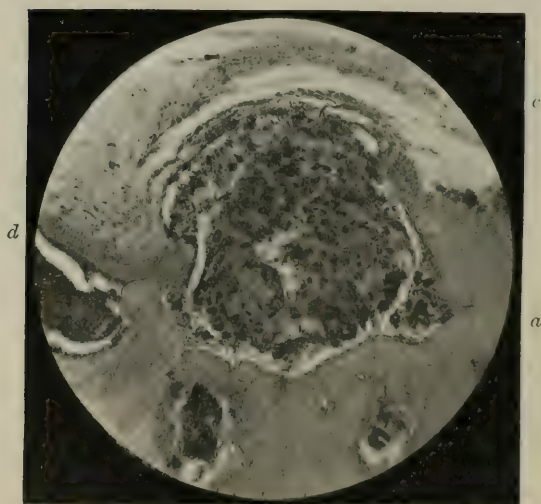


FIG. 3.—Mass of calcospherites apparently representing an abortive attempt at enamel formation; *c*, capsule; *a*, mixed tissue; *d*, dentine. ($\times 50$.)

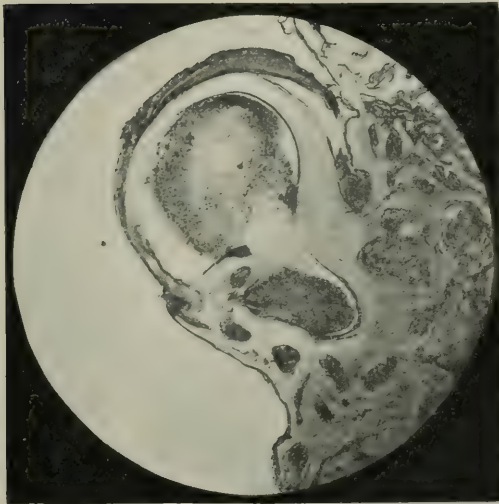


FIG. 4.—A developing tooth projecting from the margin of the odontome. ($\times 50$.)

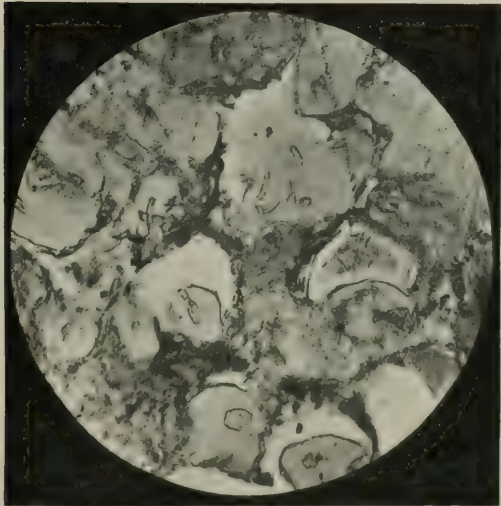


FIG. 5.—A portion of the growth containing numerous denticles; *e* points to a portion of enamel left in position. ($\times 50$.)

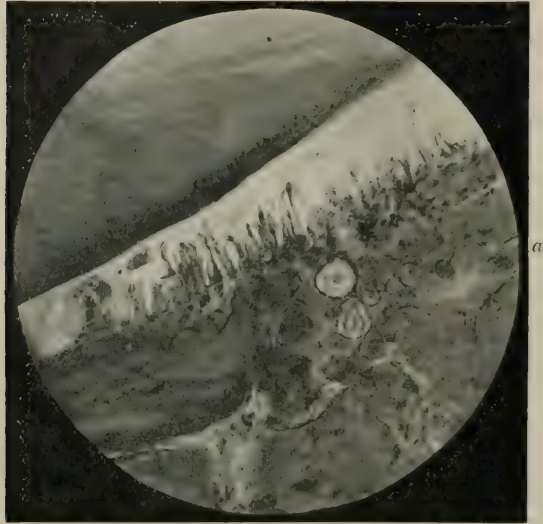


FIG. 6.—Margin of one of the larger denticles (*a*) showing portions of enamel and large calcospherites. ($\times 150$.)

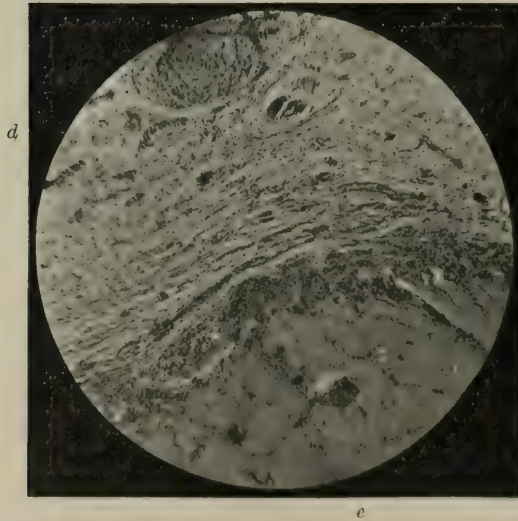


FIG. 7.—The connective tissue capsule surrounding the growth: *e*, mass of epithelial cells; *d*, mixed tissue of the odontome. ($\times 50$.)

Section of Odontology.

President—Mr. W. H. DOLAMORE, L.R.C.P., M.R.C.S., L.D.S.E.

Some Points in the Growth of the Face.

By GEORGE G. CAMPION and JOHN MILLARD (Sculptor).

DURING birth the head is subjected to pressure which alters it somewhat in form. It often takes some weeks for it to recover from the effects of this pressure and for it to resume its normal shape, yet at birth certain characteristics are apparent in which it differs greatly from the head of the normal adult.



FIG. 1.

The face is relatively very small in comparison with the cranium. There is no depression at the nasion, but the nasal bones are themselves depressed in relation to the frontal bone and cheeks, and this gives a retroussé character to the nose. The lips are more projecting than in the adult, this being due to sucking and to the lack of development and consolidation of their muscular tissue. There is a layer of fat on the chin—the chin pad—which in young children takes the place of the mental prominence in the adult, and which persists for the most part through life. There is also a double chin due to a large pad of fat posterior to the chin pad; in the adult male this has usually disappeared, but it is often retained by women.

What is known as the sucking pad is a large pad of fat occupying the cheek region; it is so prominent that when the head is viewed laterally it is seen to cover part of the ala of the nose and also the corner of the mouth, the cheek appearing in this view like an inflated balloon and contrasting strongly with the more depressed appearance of the fore part of the adult's cheek (fig. 1).

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At the age of 1 year the frontal eminences overhang the face so that the slope of the forehead from the centres of ossification to the nasion is downwards and backwards, and a line drawn downwards continuing this slope would pass through or behind the base of the nose and through the body of the mandible, about one-third of the distance from the symphysis to what will develop later into the angle.

At this period the face measurement from the nasion to the lower border of the mandible at the symphysis is considerably less than that from the nasion to a line joining the two external auditory meatuses, and the same distance falls well short of the distance to the frontal suture.

These conditions apply to children under the age of 1 year.

Such are the conditions of the face at birth and within a year afterwards. Its growth may next be considered conveniently under six heads: (1) Vertical growth, (2) lateral growth, (3) forward growth of the whole face in relation to the base of the cranium, (4) the growth of the frontal region, (5) the growth of the oral and mental region.

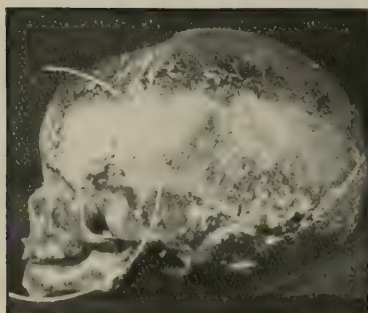


FIG. 2.



FIG. 3.

(1) *Vertical Growth.*—Fig. 2 shows the relation at birth of the height of face (nasion to lower border of mandibular symphysis) to the distance between the nasion and the transmeatal axis, and also to the frontal suture. It also shows that the level of the zygoma is just above that of the base of the nasal fossæ.

The brain at birth weighs about 400 grm.; this weight has doubled or increased to about 800 grm. at the end of the first year, and trebled itself or increased to about 1,200 grm. at about the end of the sixth year. Its weight is then only about 200 grm. less than that of the adult brain, but the growth of the face is much less rapid and does not attain its adult proportion to the cranium till at about 18-20 years of age.

At about 8 years of age the height of the face (nasion to lower border of mandibular symphysis) has increased so as to extend from the nasion to beyond the transmeatal axis, and this same measurement, when compared to the distance from the nasion to the frontal suture, reaches almost to the latter.

The growth downwards of the face has also at this age resulted in the nasal fossæ being carried downwards so that the line of the zygoma is now at about the centre of the nasal orifice (fig. 3).

In the adult the height of face (nasion to lower border of mandibular symphysis) is such as to extend from the nasion to almost the posterior border of the mastoid process and to beyond the frontal suture (fig. 4).

An examination of these three figures shows admirably the varying relations, up to adult age, of the height of the face to the cranial capacity.

(2) *Lateral Growth.*—At birth the widest part of the skull is found at the parietal eminence, at which time it greatly exceeds the zygomatic measurement (fig. 5, p. 18). At 8 years this disparity has disappeared, indeed even at this age the zygomatic width may be equal to that across the skull at the parietal eminence (fig. 6), and in the adult the zygomatic width is considerably greater (fig. 7).

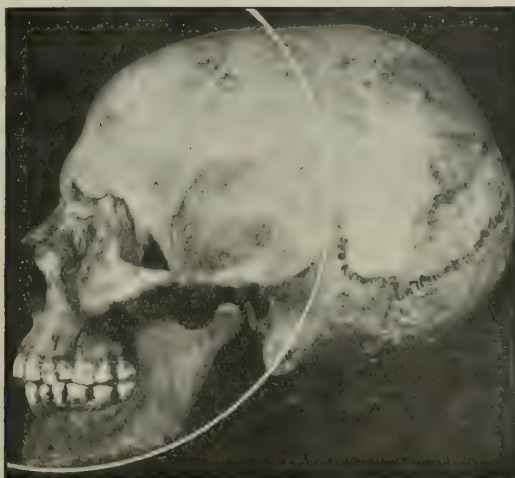


FIG. 4.

These figures (figs. 5 and 6) show that at birth the upper part of the nasal orifice is at the level of the centre of the orbits, and at 8 years at about the level of the junction of the upper three-quarters with the lower quarter.

(3) *Forward Growth of the Whole Face in Relation to the Base of the Cranium.*—The means of a number of measurements of normal and sub-normal cases show that there is steady progressive growth of the face forwards and downwards reaching its maximum in the adult, although, as we shall show presently, there is considerable variation in this growth and apparently early cessations of growth at some points and excessive growth at others: and these are probably responsible for different types of face and for some well known abnormalities.

(4) *Frontal Growth.*—The overhang of the forehead visible at 1 year gradually becomes less with the growth forward of the face, until at an undetermined age, which apparently varies greatly in different individuals, the forehead becomes vertical, pointing directly downwards to the chin. Fig. 8

shows the profile of a child aged 3 with the lower part of the frontal sloping backwards to a point behind the chin. Fig. 9 at 12 years shows the forehead nearly vertical, but here the lower part of face, owing to a slightly larger and more prominent mandible, is slightly in advance of the upper part of the nose and forehead. This will in part be corrected at a later age by a forward growth of the nasal bones and cartilages and by a growth forward of the lower

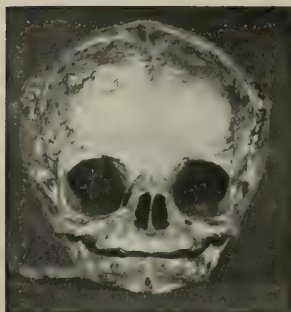


FIG. 5.

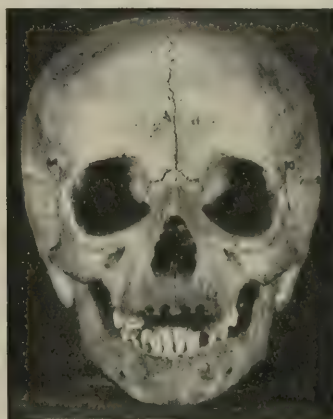


FIG. 6.



FIG. 7.



FIG. 8.

part of the frontal bone with the increase of the frontal sinus; but it may also in part remain as a distinctive character of this type of face.

Adult Woman.—Fig. 10 shows that the slope of the forehead has now definitely become forwards and downwards, and that the nasal bones have come forward, bringing the nasion more into line between the frontal eminence and the tip of the nose. There is no trace of glabella.



FIG. 9.



FIG. 10.

Adult Man.—Fig. 11 shows the same slope of the forehead with the formation of glabella.

The glabella is a distinctive characteristic between male and female. When present it indicates a male; when absent, the subject may be male or female.

(5) *Growth of Oral and Mental Region.*—Mandible at birth. We shall not refer to the well-known stages of growth in the mandible as shown by the gradual formation of the ramus and the accompanying raising of the condyle. These points are well known and are figured in all the text-books, but we would call attention to the importance of the presence or absence of the mental prominence as determining different types of face. At birth the mental prominence may or may not be observable.



FIG. 11.

Fig. 12 shows profiles of two faces as exactly alike as they can be drawn from the mouth upwards, with the exception of a slight difference in the modelling of the lips and corner of the mouth. But below the mouth the presence or absence of the mental prominence determines two entirely different types of face. [This difference in facies may apparently be due either to a lack of development of the mental prominence or to a lack of growth of the entire mandible.

We have traced in outline some of the larger features of the facial changes which successively present themselves in studying the growth of the face from birth to adult life. To fill in this outline and consider it fully in all its variations and details would afford material for a life study. We have seen that the growth of the face comes later in life than the growth of the cranium, and that the face attains its full size and development much later in life: that, as shown by

the mean of a relatively small number of cases, there is a progressive growth forwards, downwards and in width, and that this takes place sporadically in different regions at different times. And it seems clear that causes which are at present mostly unknown may interfere with these normal features of growth and cause inhibition in growth or excess of growth, which may manifest themselves in permanent features of the adult. It remains for future research to explain the mechanism by which this growth is achieved and the influences of pathological processes and abnormal habits in producing the various abnormalities which are known to us.



FIG. 12.

“An Anomalous Tumour in connexion with the Deciduous Teeth,” by A. T. PITTS, D.S.O., and J. HOWARD MUMMERY, C.B.E. (May 23, 1921). This communication will shortly be published in the *British Dental Journal*.

Section of Odontology.

President—Mr. MONTAGU F. HOPSON, L.D.S.E.

The Endocrine Factor in the Production of Immunity and Susceptibility of the Teeth to Caries.

By F. W. BRODERICK, L.R.C.P., M.R.C.S., L.D.S.Eng.

THE modern trend of thought amongst members of our profession is one of dissatisfaction with the theories of the ætiology of dental caries on which we were brought up, and the dental literature of to-day reflects this dissatisfaction. I desire, therefore, to-night to draw your attention to the possibility of the endocrine apparatus playing a part in the causation or the prevention of this disease.

Although Miller's theory will explain the onset of caries, and Sim Wallace's conclusions help us to see the reason for its prevalence, and although these theories may be true, many of us feel that they are by no means the whole truth of the matter. They leave too many questions unanswered, too many undoubted clinical facts unexplained; and, above all, if they in any way approach the whole truth, the results, bad as they undoubtedly are, should be infinitely worse. If the destruction of enamel, due to chemical action, is a simple matter of the solution of lime salts by organic acids—if there is no other factor in the equation—then the extraordinary thing is not that dental caries should be rampant, but rather that it should be possible for teeth to exist at all under modern circumstances. Then, the fact that we see good, sound, hard teeth in adult life, seems to be the problem needing elucidation, before that of the rapid destruction of the teeth of children and young people. For, knowing the affinity of lime salts for acids, and the certainty that acids can be and will be formed in close proximity to the teeth, whatever precautions we may take, we must necessarily postulate an immunity in the mouths of some, to account for the condition that we know to exist, and assume the necessary corollary, that the absence of this immunity engenders a susceptibility to dental caries. In other words, we presume a predisposing cause, without which tooth destruction cannot take place.

On this matter, Dr. Kirk has said :—

“Every practitioner knows that susceptibility to dental caries is a thing that comes and goes. So clearly and so broadly is this fact recognized, that it is generally conceded amongst dentists that youth is the period of greatest susceptibility, and that, assuming normal conditions of health, the tendency is markedly diminished, if, indeed, a period of immunity does not normally supervene, when adult age is reached. It is also a well-established fact that pregnancy tends to inaugurate a period of susceptibility, the old axiom ‘for every child a tooth’ has its equivalent expression in practically every civilized tongue. It is also known from wide clinical observation that dental caries is not of necessity a filth disease. Some teeth kept as clean as patient and dental operator can keep them, decay, and decay recurrently; other teeth, in mouths into which a toothbrush has never entered, and which are offensively filthy, do not decay. Our theories must be sufficiently comprehensive to fairly explain these peculiarities, otherwise we have not arrived at the whole truth of the matter.”

[November 28, 1921.]

This being so, from a preventive point of view at any rate, we must turn our attention to the predisposing cause, for if teeth are, or can be made, immune, exciting causes do not matter; on the other hand, if they remain susceptible, our daily labour is for the most part in vain, for we know that there is little hope of saving them beyond to-morrow.

In an earlier paper, I put forward the theory that the predisposing cause of dental caries was a diminution of the calcium-utilizing power of the body, brought about by a loss in balance in the secretions of the endocrine glands, resulting in a greater or less degree of calcium starvation of the tissues of the body. I now wish to amplify this theory, and to show how this calcium hunger is brought about, and how it reacts upon the teeth.

The first point I would emphasize is the all-important necessity of a complete covering of the erupted portion of the tooth with enamel. However thin this may be in parts, so long as it is complete, no tooth destruction can take place; conversely, once the enamel has been removed throughout its whole thickness, over however small an area, the tooth is doomed, unless artificial measures are taken for its preservation, except in that extraordinary condition known as arrested caries, to which I shall refer presently. Consequently, in considering the ætiology of dental caries, it is to the enamel alone that we need direct our attention.

Clinically we all recognize differences in the enamel of various teeth in colour, in opacity, and in hardness, the extremes being perhaps the teeth of the gouty man and of the consumptive child: even in healthy patients there is a noticeable difference in the extremes of life. Further in certain cases, we can predict with much certainty, that some teeth will in later life fall victims to pyorrhœa, and that any fear of caries is groundless. Pickerill proved experimentally that some teeth were hard, and some soft, using the terms "sclerotic" and "malacotic" to differentiate the two types; and although analysis seems to show that the quantity of lime salts is not much greater in the immune than in the susceptible tooth, we know that there is a clinical difference, and that, other things being equal, a tooth that has erupted ten years is harder than one that has erupted two years; consequently, it appears probable that a vital change from a soft to hard condition is slowly taking place in these teeth, and it is this vital change which produces the immunity to caries.

In considering this subject the body chemistry of the salts of lime must be briefly discussed. Lime is found in the body in two forms:—

(1) Fixed in the tissues as inorganic salts which help to build up the framework of the body.

(2) A quantity serving as part of the alkali reserve of the body, as an ionic salt, and having many uses. The fixed lime has passed through this embryonic stage; some of this ionic lime will later be used for the purpose of body building or repairing, but in the meantime it has other necessary functions to perform, not only as an alkaline salt (its necessity as such will be considered later when discussing the question of acidosis), but also on account of its calcium ions, which are necessary for giving tone to muscle, for the proper functioning of the heart, and also for keeping nervous tissue in a proper excitable state, for without these calcium ions there can be no proper transmission of nervous excitability from nerve to nerve cell or from nerve cell to muscle.

Now lime salt must enter the body by way of the food supply, but the question of ingestion and of a balanced diet is in itself insufficient. It is necessary to remember that "within the intestine" does not, by any means,

imply "within the body," nor is absorption by itself always sufficient, as, without utilization and fixation the salts may still be lost. Lime salts may be, and often are, excreted unchanged in the feces, or they may in part be absorbed into the blood stream, utilized or not as the case may be, as ionic lime and excreted in the urine. In this case also they are useless for tooth-building (I here mean not simply original calcification of tooth tissue, but the constant hardening process that I believe to be continuously going on); for this purpose they must be definitely fixed in the tooth substance. Thus we see that both absorption and fixation are necessary. Increasing the quantity in the diet, and care in selecting an easily absorbed quality are in many cases, by themselves, quite useless for dental prophylactic purposes, a fact well recognized by the medical profession in treating conditions such as rickets and osteomalacia, both of which are regarded as being due to lime shortage. In fact it would seem that the amount of calcium required to keep the body in health is extremely small; it has been estimated as about 0.75 gm. daily for an adult, and it would be a simple matter to determine how much milk alone would supply this amount, if all the lime contained therein could be absorbed, utilized, and fixed. Of course if this irreducible minimum is not given in the food it is quite probable that dental troubles might ensue, but it is difficult to imagine a diet that will not contain this, short of actual semi-starvation.

Gies found that a diet ordinarily considered to be deficient in lime salts produced no effect on the teeth of puppies in one hundred and twenty-seven days, and that it was necessary to reduce the calcium content of the diet of rats to 3 mg. a day for seventy days before signs of demineralization were found in the teeth. This appears to show conclusively that as regards lime salts any diets that we have to consider provide an ample sufficiency, and it is for the body itself to take, utilize and fix, what is provided: if this is not done it is the fault of the body itself rather than of the diet. (I am purposely leaving out of consideration now for the moment the question of vitamins to which I shall refer later, and am considering only the question of lime salts as a part of the diet.)

The next point for consideration is the need of lime salts during different periods of life; for this purpose a normal lifetime may be divided into three stages—viz., growth, reproduction and decay. During the first period the body is hungry for lime salts; bone and tooth building are active and the whole body is living at a great rate. In a healthy child nature has seen to it that this supply is forthcoming, but the margin is small and any little upset in absorption and utilization is quickly felt. Calcium hunger may change to calcium starvation, with its deleterious effects, both from the point of view of a shortage in the laying down of inorganic lime and its fixation in the bones and teeth, and from that of the need of ionic calcium, as shown by the tetanic convulsions so commonly seen during this period.

During the second period, that of reproduction, the balance between intake and output of calcium is more stable, and in men at any rate, lime is only needed to make good wear and tear and to replace waste; in women, however, there is a difference which Blair Bell summarizes as follows:—

"In men the reproductive calls on the metabolism are not great, their metabolism during the reproductive period, after growth has been established, is mainly concerned with individual needs; compared with women, the nerves must be steady, the mind stable, and the physical strength great, and all the endocrine glands are adjusted with this end in view—the preservation of a steady uniform metabolism in which no sudden alterations occur. There is, therefore, little reason for pathological lesion of hyperplasia

or hypoplasia to supervene in his organs of internal secretion, and this is what we find, provided that the changes at puberty do not overstep the mark. On the other hand in women, during this period there are bound to be sudden and rapid alterations, due to the demands of pregnancy and lactation, and with menstruation there is a large excretion of calcium and other substances previously required for growth of skeletal and other structures, but which are now no longer required until pregnancy occurs, when a fresh body is to be built up and nourished by the maternal metabolism. Thus, to a certain extent, a woman by her catamenia is kept in practice and protected from too sudden and great demands on her metabolic activity, and the periodic fluctuations in her economy are provided for by the capabilities of her endocrine apparatus."

Thus we see that during the reproductive period of life in both sexes, pregnancy excepted, there is less calcium hunger, less danger of calcium starvation, than during the period of growth, but that there is always a greater tendency to that state in women than in men, and if pregnancy supervenes, even the greater conservation that this condition entails may be insufficient to counteract this tendency; especially in women who were near the border line before.

That the calcium drain in pregnancy is considerable, is well shown by the experiments of partial parathyroidectomy in cats; these showed no signs of tetany unless they became pregnant, when the convulsions occurred, showing that the condition was latent and that conception only was necessary to upset a balance held with difficulty; indeed so well is this fact recognized that the fetus has been well termed a calcium parasite.

Lastly, the period of decay. Now the balance of calcium equilibrium is in the opposite direction—the tendency is for the body to receive and utilize more than is necessary; lime is not required for growth, less repair and replacement is wanted, and the excess is liable to be stored in awkward places, arteries degenerate and joint troubles commence, the tendency to calcium hunger is replaced by a tendency to calcium saturation.

If with these facts in mind we consider the periods of life subject to susceptibility or immunity to dental caries as put forward by Kirk, we see that the greater tendency to calcium hunger gives rise to a greater susceptibility, and vice versa; growth and pregnancy are the periods of susceptibility, later life and old age of immunity.

The metabolism of lime salts is believed to be controlled by the endocrine apparatus, a term I use advisedly, for modern knowledge of the various glands of which this apparatus is built up shows conclusively that they are all part and parcel of one complicated machine working together for one end, which may be summed up in the word "life" in its broadest sense. With special reference to the calcium salts, these glands may be divided into two groups—one regulating absorption, utilization and fixation; and the other excretion.

In full health there is a perfect balance of calcium metabolism—just such amounts as are required at the time are utilized and the remainder excreted; any upset in this balance, whether by increased excretion, diminished utilization or otherwise, will lead to signs and symptoms of disease. In the first group of glands, the utilizers, we place the parathyroids, the pars anterior of the pituitary, and the medulla of the suprarenal, possibly also the thymus; in the second group the gonads (ovary and testis) and possibly the thyroid.

With regard to those glands in the first group, possibly they are not interchangeable, each may have its appointed part to play; the relation between parathyroid insufficiency and tetany seems to point to the fact that this gland presides over and in some way regulates the conversion of food lime into ionic calcium, for Howland and Marriott found that whereas healthy blood

contained 10 to 11 mg. of lime per 100 c.c., in rickets this was reduced to an average of 9.4 and in tetany to 5.56 mg. per 100 c.c. In acromegaly, amongst other symptoms there is an increase in the deposit of fixed inorganic lime, and as this disease is probably due to hyperplasia of the pars anterior of the pituitary, this may be the gland that converts the ionic calcium produced by the parathyroid into fixed inorganic material in bones and teeth. This point I do not unduly emphasize, as I have no authority to quote, but I would point out that these glands acting together are necessary for the utilization of these salts and the building of them up into the tooth structure.

We have considered the age incidence of susceptibility of the teeth to caries, and have found that it coincides with the period of threatened calcium starvation; we will discuss the normal activity of the various endocrine glands and the gland groups, with these various periods of calcium hunger.

In childhood, the most noticeable feature is that the gonads are inactive, have not reached the stage of functional activity and that the thymus is present in an active state. The child is thus placed in an exceptionally fortunate position for receiving and utilizing a large amount of lime. As the thymus deteriorates the gonads become active; thus there is an imperceptible passage into the second stage—that of a more stable balance between assimilation and excretion.

The question of menstrual flow in woman, if normal, is to her advantage as we have shown, but if excessive or if her tendency is towards undue calcium hunger, this may upset the balance, and may well account for the undoubted fact that as a sex the teeth of women are worse than those of young adult men of the same age group. But in ill health there is an interesting interaction between the ovary and the menstrual cycle which may to some extent regulate the loss.

In pregnancy the ovarian activity is in abeyance and as a rule remains so during lactation; this places the mother in a favoured position for conservation of her lime salts for the benefit of the child and incidentally of herself.

In old age again the gonads lose their potency and calcium excretion is diminished. We see therefore that in health the whole endocrine apparatus regulates the metabolism of lime salts, to the benefit of the individual, at all periods of life; but that an upset in the balance between utilization and excretion will have a greater effect, or produce that effect with a milder upset, at certain periods than at others, and that these periods are similar to those in which we find an increased susceptibility to dental caries.

The following points illustrate the question of the metabolism of lime salts in pathological conditions:—

I have shown that a normal balance between the assimilating and excreting glands is necessary for health. What will upset this balance, and in which direction will it be upset? For an upset balance in favour of the excretory group will result in calcium starvation and one in favour of the assimilators will produce calcium saturation.

I started my researches in pre-war days with the infectious fevers, for it is a recognized fact that the endocrine apparatus is concerned with bodily immunity, and should the patient succumb to an acute infection the glands will be stimulated to increased activity, to produce excessive secretion which acts as auto-antitoxin (Sajous). It is reasonable therefore to suppose that if the patient recover, the period of overwork may be followed by a reactive period of underaction. The late Rupert Farrant in his study of the thyroid post mortem found this to be true, so true indeed that he considered it was

possible from post-mortem examination of the thyroid alone, to say from what disease or group of diseases the patient had died.

With children, this reaction of the endocrine apparatus from overstimulation is almost certain to be in one direction, for in their case the assimilators work practically unopposed, consequently the reaction should show itself—if it shows itself at all—in the direction of calcium starvation, which will affect all bodily tissues more or less. I found that the saliva of children convalescent from measles and scarlet fever was deficient in alkalinity, and that this deficiency was due to, or at any rate concomitant with, a lessening of the lime salts in the saliva. (As time is limited I omit discussion of figures; they can be found elsewhere.) I also found that a suitable plurigland preparation, administered to these children by the mouth, had a rapid and considerable effect both on the alkalinity of the saliva and on its calcium content. I myself contracted scarlet fever during this time, and confirmed these findings on myself, beguiling the tedium of convalescence with the burette and the microscope. This then was a point gained, and would seem to indicate a reason for increased susceptibility to caries in acute illness, a condition that we know to exist. For increased alkalinity of the saliva would doubtless act by neutralizing organic acid at the moment of formation, whereas a diminished alkalinity, below a certain point, would be less able to do so; and although these acids would readier combine with the alkali of the saliva, if that is not present they must perforce neutralize themselves with the lime salts of the enamel.

I next attempted to discover what could reasonably be considered a satisfactory alkaline index for the saliva, by taking saliva from all patients with whom I came in contact. As expected, I found considerable variation, but these variations fell into definite groups and all seemed to be explainable. For this purpose I estimated the hydrogen ion concentration of the saliva using the colorimetric method of standard tubes, which gives better and more accurate results than, and is simpler than, titration.

I found that in good health and in people without progressive caries (that is active destruction of enamel) the alkaline index was higher in children than in adults. It was highest of all in old people, especially in those suffering from pyorrhœa with subgingival tartar formation, but in children and adults with progressive caries and in adults suffering from ill health, especially active tuberculosis, the alkaline index was low; in some of the latter cases I found an hydrogen ion concentration to be as low as Ph 5.8, indicating a definite acid saliva.

I was unable to combine these estimations with that of the hydrogen ion concentration of the blood, or to make any estimations of the calcium content of this tissue owing to the lack of opportunity and apparatus; the results would have been interesting. These results are what would be expected; healthy children with active assimilatory glands are able to keep free from calcium hunger, in fact they have lime salts in reserve, ready to be used for body building, an excess not required and therefore not present in healthy adults; on the other hand as age increases this excess again appears, not because it is required, but because of defective elimination; where, however, the children or adults are in bad health and caries is active, we find signs of deficient assimilation.

This brings me to the first stage of my hypothesis that an upset of endocrine balance predisposes to dental caries by causing a diminution of the alkalinity of the saliva.

But there is a second and I believe more important manner in which the lack of usable lime salts may effect the dental tissues. There are two well recognized diseases exhibiting in some directions opposite symptoms, namely, acromegaly and osteomalacia; in acromegaly, one of the most pronounced symptoms is an increase in the deposition of lime salts in certain bones; the lower jaw is perceptibly enlarged and thickened, the skull enlarges and if the disease arises before the junction of the epiphyses of the long bones, these are considerably increased in length, producing the phenomena of gigantism. In osteomalacia, on the other hand, there is a definite demineralization of the skeletal tissues; unfortunately in reading up this matter from a dental standpoint, I could find but little as to the state of the teeth, our medical confrères in the past have not considered these organs sufficiently, and it is rare to find any statement of their condition; if they are mentioned at all, it is some vague remark to the effect that the teeth decay rapidly and that pyorrhœa is present. This we know to be a contradiction of terms, for both conditions are rarely found at the same time; as we dentists recognize that these two states are antagonistic, and that the teeth that succumb to pyorrhœa are, generally speaking, the hard sound teeth that have withstood caries. I have never seen a case of osteomalacia, so I cannot speak as to the condition of the teeth; I have, however, during the last twelve months, had under my care two pronounced cases of acromegaly, both in women, the diagnosis of one of which I was able to confirm by a radiograph of the skull, which showed an enlarged sella turcica and great increase in the thickness of the bones of the vault of the skull, and both of these women had had their teeth removed for pyorrhœa.

Now the symptoms of acromegaly are caused by an increase in the secretion of the pars anterior of the pituitary body, and although I have no personal experience of the teeth of patients suffering from osteomalacia, a condition the reverse of acromegaly so far as the deposition of lime salts is concerned, I have lately had a patient whose condition seems to point to a disease known as dystrophia adiposo-genitalis, due to hypo-function of the pars anterior of the pituitary. She had lost all her teeth from caries; her first child was an anencephalic monster but she also had bonny fat twins of 6 years old, whose temporary teeth were a mass of small amalgam fillings. These results of observation in endocrine disease are interesting and not without point as will be seen later in the argument.

Various ductless glands have been made answerable for the onset of osteomalacia, the suprarenal by diminished secretion (Bossi), the ovary by hypersecretion (Bell), and the pituitary by hyposecretion (Knowles); and Erdheim found a hyperplasia of the parathyroids in this disease, which however he believed to be due to an antagonistic response to ovarian hyperplasia—at any rate it will be seen that it is an upset balance of metabolism in the direction of lime starvation. Thus we see, in the comparison of these two diseases, that under certain conditions the body has the power of fixing lime in excess (in the bones at any rate) or of yielding it up, that is of either hypercalcification or of decalcification as the case may be; and as the hypercalcification or decalcification is very excessive the results are most noticeable.

Bearing these facts in mind consider for a moment the hard "gouty" tooth and the soft tooth of acute tuberculosis, the enamel of one so hard that a steel drill will penetrate it with difficulty, that of the other so soft that it can be scraped with an excavator, the edges of the cavities so cheesy that one does not know where to stop in preparing them, and the resultant filling so unsatisfactory as to make the conscientious dentist despair. Consider again the teeth

of some children, where the greatest and most intelligent care of parent and dentist is insufficient to preserve the temporary dentition in good working condition until it is replaced by the permanent dentition. Were it not for the fact that we know that in many instances a time will come when conditions will improve, we should be inclined to refuse to spend time and labour on what seems a hopeless task.

Consequently we know definitely that not only bones, but teeth also, harden or soften in pathological stages. As to how this hardening or softening process takes place I shall not discuss, except to say that there are two possible ways. Each of these ways in turn has been declared to be impossible by presumably competent observers; one route is by way of the pulp, the other by way of the saliva; the pros and cons of the matter I must leave in more capable hands, but that the hardening or softening does take place must be obvious to all; it has been suggested that the normal hardening may be analogous to crystallization but this does not account for abnormal softening. I may however mention here the experiments of Dr. Head, quoted by Gies. Dr. Head placed extracted teeth in a solution of organic acid (fruit juice) and found that the surfaces turned chalky white, and that the enamel softened. This was proved by placing the teeth in a suitable machine containing a needle with a weight superimposed and by altering the weight and measuring the depth to which the needle was driven into the tooth. After this the teeth were immersed in saliva and the chalky white appearance of the surface disappeared and the normal enamel surface again presented, with an increase in hardness.

Head postulated that fermentation in the mouth caused an abstraction of calcium from the tooth, so that one out of every three atoms of lime was removed, and that if lime was returned to the surface of the tooth, in sufficient quantity, the abstracted portion was replaced, particularly so if the saliva were alkaline. This point is interesting and may possibly account for a condition of things which actually takes place.

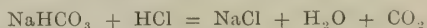
We now see that pathologically, at any rate, bones harden or soften to extreme degrees and that this change is due to endocrine derangement; we know also that teeth both harden and soften in pathological stages. May not the same causal agents underlie the alteration in the consistency of the teeth as well as of the bones, that is, that the teeth are affected in this way by an alteration in the endocrine secretion? A fact to be mentioned before I pass to the next point is that the hardening process, to some extent, is physiological and the softening pathological; I will return to this later in the argument.

Have we any knowledge as to why this change in the fixation or the withdrawal of lime salts is brought about? I believe we shall find the *modus operandi* in the study of that pathological symptom-complex, somewhat badly termed "acidosis" which accompanies certain diseases; to make my reasoning clear the pathology of this condition must be briefly discussed.

Acidosis does not necessarily mean an increased storage of acids or acid salts in the blood and body tissues, nor need it mean an increase in the production of acid in bodily metabolism. Sellards' definition of acidosis is that "it is a diminution, from any cause, in the reserve supply of the alkaline bases in the blood and other tissues of the body, the physico-chemical reaction of the blood remaining unchanged, except in extreme conditions." Except in extreme cases, which we are not now considering other than to use them as illustrations, where their very severity renders results clearer, this must be so, for a very slight alteration in the reaction of blood towards acidity as measured by the hydrogen-ion concentration is incompatible with life.

In the normal metabolism of food-stuffs there is a constant tendency towards the production of acid radicals, arising from the combustion of carbon phosphorus and sulphur, which exceed the basic radicals present: consequently, to preserve the alkaline reaction of body fluids this excess acid must be got rid of. This is accomplished normally by several different methods—e.g., acids from the blood can be removed from the body by the lungs, by respiration in the form of carbon dioxide, by the kidneys in the form of acid salts, by neutralization with ammonia formed in the metabolism of nitrogen, and lastly by using up the body supply of alkaline salts extracted from the foods. Each and all these methods are utilized in health, when a constant fight is being maintained to keep down an ever-threatening acidosis, and for the victory, which is essential to life, the carbonates and phosphates of the blood must be in sufficient quantity to enable them to undertake their appointed task.

The first line of defence lies with the carbonates. Acid in the form of CO_2 is being constantly formed in the tissues; respiration lowers the concentration of CO_2 in the lungs and allows the higher concentration in the tissues to escape by way of the blood to the lungs and thus to be eliminated. Any excess of CO_2 in the blood at once stimulates the respiratory centre to increased activity, and soon brings about an equilibrium. This is seen constantly in the threatened acidosis of hard physical exercise; the CO_2 is carried by the carbonate of the blood from tissues to lungs, and there released and eliminated. Other acids can be and are excreted the same way: they simply displace the CO_2 from the carbonate, setting this free to be eliminated by the lungs, and leaving a neutral salt to be excreted by the lungs, e.g.—



Here, however, the process robs the body of alkali, for every molecule of acid would use up a molecule of carbonate, and if this loss is not compensated the animal would rapidly succumb to the effect of an acidosis produced by reason of the insufficiency of carbonate present in the blood to carry the carbon dioxide formed in the tissues to the lungs. This difficulty is partly overcome by the power of the kidneys to excrete an acid urine from neutral or slightly alkaline blood, through the chemical properties of the phosphates; for whereas the phosphates in the blood are in the form of the di-sodium monohydrogen phosphate, which is alkaline, that in the urine is the monosodium di-hydrogen phosphate, which is acid. If you work out the equation you will see that the reaction saves sodium carbonate, and thus renews this alkaline salt periodically to the blood, e.g.—



On the other hand, acids may be renewed by the kidneys, taking with them, however, body alkali in the process, e.g.—



Porter points out the importance in bodily metabolism of this di-sodium salt of phosphoric acid, which he believes to be the true physiological alkalinizer of the blood, and definitely states that without these salts nature is unable to hold the other mineral substances in fixed relation to the tissues (bones, teeth, &c.); especially is this true of lime salts. Porter goes on to say that this salt is not met with as such in the food, but is formed continuously in the body, in health, through the interaction of HCl and trisodic phosphate,

and slowly and continuously enters the blood stream. This suggestion is upheld by the work of Sherman, who found that the substitution of a 0.4 per cent. secondary sodium phosphate (Na_2HPO_4) for a small part of the calcium lactate present in the diet prevented the appearance of rickets in young rats fed on a diet deficient in fat soluble A.

Thus we see that even in health acids either formed endogenously within the body, or taken in food, do slowly and gradually act as a drain on the alkali reserve of the blood and tissues, and if health is to be maintained this reserve must be continuously replenished, and this is done by the utilization of the salts of sodium, potassium, magnesium and calcium, the absorption and utilization of which is under the control of the endocrine apparatus. In health this reserve, though never large, is sufficient, but in disease several factors may cause shortage—e.g., there may be an increased production of normal acids, or even abnormal acids may be formed; some diseases of the kidneys may upset the delicate mechanism by which its power of discrimination and alkali saving may be lost; alkali itself may be lost in other ways than in neutralizing acid, such as by excretion in acute diarrhoea, or even the absorption and utilization of alkali may be interfered with. At any rate this reserve supply of alkali becomes diminished in quantity and insufficient for its purpose.

Extreme conditions of acidosis are met with clinically in such diseases as diabetes, and in the last stages of some cases of nephritis; it is also seen in certain food-intoxications in children in pregnancy, and certain other conditions, and is easily recognized by its physical signs. There are, however, many conditions in which an acidosis has been found by chemical laboratory methods to exist where there have been no acute symptoms, that is, where it has been proved that the alkali reserve of the body is reduced to a greater or lesser extent; these cases have been termed by Sellards "compensated acidosis." Now it must be remembered that this compensation has been brought about at the expense of the patient in other ways, e.g., the alkali reserve, though sufficient to preserve life, may be at a dangerously low ebb and unable fully to carry out the purpose which it was originally intended to perform. By using the bicarbonate toleration test (which is performed by giving large doses of sodium bicarbonate until the urine turns alkaline) Sellards has shown that a diminution of the normal alkali reserve equivalent to 40 to 50 gm. of sodium bicarbonate was unaccompanied by any noticeable clinical symptom, that a deficit of from 75 to 100 gm. produced definite dyspnoea, whilst a deficit of 150 to 200 gm. was required to produce definite air hunger and coma.

From this we see that there must be many cases in which an acidosis is present which by its very compensation gives a lower alkali reserve, with all that this may mean to the patient, without any definite signs or symptoms of disease. As Langdon Brown has put it "a living organism is like a spinning top, only in equilibrium whilst it keeps going, but unlike a top it is provided with a delicate mechanism by which it may readjust its balance if this is disturbed. Disease is the resultant of the action of some external force and the reaction of the body; this reaction may be sufficient to restore the equilibrium—i.e., a return to health, or the external force may overcome the balance altogether—i.e., death supervenes. Between these two extremes a new though inferior position of equilibrium may be attained—i.e., the disease is more or less compensated; in this position the machinery works, but with more friction and the reserves are diminished."

I am not suggesting that calcium is the most important element in the

alkali reserve, it is not; but in proof that these salts form an important part, I have authorities to quote. Marriott and Howland, working on the acidosis in the terminal stages of nephritis, found a marked diminution of the calcium in the blood serum, as low in one case as 1.5 mgr. per 100 c.c. of blood, as compared with 10 to 11 mgr. in normal cases; this low calcium content they believe to be due to an excess of acid phosphate which the damaged kidney could not excrete, at any rate administration of phosphate caused a further diminution of calcium of the blood serum, whereas administration of calcium reduced the acid phosphate, presumably by neutralization.

Again, Osborne, Mendl and Ferry have produced coma by feeding rabbits for a long period on a diet poor in salts, which was overcome by adding calcium and potassium phosphates and the citrates of sodium, magnesium and calcium to the food, and Peabody believes that the muscular twitchings of uræmia are due to lack of lime salts.

I think, therefore, that we are justified in believing that calcium salts form part of the alkali reserve of the body, and so long as they are a part they perform important functions, one of which is the gradual and continuous hardening of the teeth, both enamel and dentine. If, however, they are commandeered for a more vital purpose, namely, keeping the patient alive by overcoming or compensating an acidosis, they are not available for their primary purpose, and the teeth must, for the time being, remain unhardened—i.e., susceptible. Further, it would seem probable, and I find several authorities for my suggestion, that should the acidosis be more severe a definite demineralization may take place associated with the separation of lime salts from bones and teeth—i.e., the teeth themselves may have to surrender some of their lime salts that have been built up into definite tooth structure. The result is not merely a stoppage in tooth building but a very definite breaking down.

What clinical evidence have we for this suggestion? The condition in osteomalacia has already been referred to; here the bones may become so soft that they can be cut with a knife, or squeezed like a sponge; lime salts are certainly extracted from them. From a dental point of view, the condition of arrested caries may start in much the same way, and as I have previously pointed out we have here something quite different to caries as we ordinarily see it. The tooth that we finally see as an example of arrested caries commenced its downward way, not as a minute destruction of enamel followed by a hollowing out of the dentine and then a cracking away of unsupported enamel, which is the usual way in which a tooth cavity is formed—rather is there here a wholesale destruction of enamel and practically none of dentine, which, contrary to all expectations, instead of being rapidly disintegrated, becomes hypercalcified. The enamel has evidently been destroyed *en bloc* and I suggest that the cause of this is twofold, first, loss in the alkalinity of the saliva caused by a diminution of its lime content, and secondly through a definite withdrawal of lime for an important and vital function; later on the dentine has become hypercalcified by a definite improvement in health and a consequent flooding of the blood with salts of calcium.

The history of these patients suggests that this may be so. In measles for example, the necessity for neutralization or elimination of toxins causes an increased activity of the endocrine apparatus; this will be followed by a reaction, resulting in a lessened assimilation of lime salts, which, combined with an acidosis associated with all acute fevers and caused by increased katabolism, gives a lowered alkali reserve. The demand of the body for alkali leads to rapid

tooth destruction; then, during and after convalescence, the gland balance reasserts itself with perhaps a compensating swing of the pendulum in the opposite direction, and we get a hypercalcification of the dentine. If however this beneficial reaction is too late to save the pulp we get the dead six years old molar level with the gum in a child of from 8 to 10 years old.

I have not been able to find much literature on the connexion between endocrine derangement and acidosis, but references are continually cropping up, and it would not seem unreasonable to connect the two; for the satisfactory working of the endocrine apparatus is an important factor in metabolism of the alkaline salts, on which depends the alkali reserve, which is the final source of safety in a threatened acidosis. This idea is also supported by the fact that the periods of calcium hunger, which as I have shown are also those of the greatest risk of endocrine derangement, are the periods of life during which there is most chance of an acidosis without definite disease, viz., childhood and pregnancy.

Respecting the former period Howland says that "the low level of carbon dioxide tension and the low hydrogen-ion concentration in the young explain their susceptibility to acidosis, which," he contends, "is not an uncommon condition in infancy and childhood." Sellards says: "Some chronic acidosis during the period of growth might conceivably bear a relation to some of the effects of malnutrition seen in children"; and pregnancy is, as we know, a state in which the risks of an acidosis are considerable. Again Crile has shown that a state of acidosis was present combined with signs of exhaustion of the adrenals, the thyroid, and the pituitary in a number of diseases, in infection, in physical exhaustion and various psychical states. And lastly findings are reported after experimental parathyroidectomy similar to those of Howland and Marriott quoted above, namely, a decreased elimination of phosphate in the urine combined with a diminished amount of calcium in the blood serum and tissues.

Sajous also believes that the alkalinity of the blood is intimately connected with secretion of the adrenal glands. "Adrenoxidase," by which term he designates adrenal secretion circulating in the blood, laden with oxygen, he believes to be the main alkalizer of the tissues, and the blood platelets as drops of adrenoxidase he finds strongly alkaline.

There is one point in the consideration of acidosis that should be noted by those members of our profession who are obsessed with the idea that the amount of carbohydrates in modern diet is the important factor in the production of caries, and would cut this down to a minimum, and this is that carbohydrate starvation is a very direct cause of acidosis. This frequently appears in diabetes when carbohydrate metabolism is interfered with, and for this reason, that unless the carbohydrate in the diet be sufficient, complete combustion of fat is prevented. Fat is split up into glycerine and fatty acid and unless sufficient carbohydrate is present the breakdown goes no further and the acid appears in the blood. It is contended that for the complete oxidation of three molecules of fat, at least one molecule of carbohydrate is essential. Wyatt has said that "unless the mixture of metabolites oxidizing in the body contains more than thrice fatty acids to one of glucose the body smokes with acidosis like an automobile run with too much oil."

In starvation the body lives on its own fats and carbohydrates and as the latter may give out first, the fat may be incompletely broken down and the alkali reserve is called upon to neutralize the fatty acids formed.

As a result of an investigation of an epidemic of acidosis in out-patient

tonsillectomy cases Ross found that 22 per cent. of cases showed acetone and 15 per cent. diacetic acid in the urine before operation, and that after operation this was increased to 50 per cent. acetone and 35 per cent. diacetic acid (these abnormal products show a condition of acidosis) but with glucose feeding and no purgative before operation these figures were very considerably reduced. This would seem to show that not only does the condition necessitating this operation predispose to acidosis, but that the operation or the anæsthetic increases that predisposition. An investigation was then instituted into the condition of all patients coming to operation in the Winnipeg General Hospital with the following results: all patients who showed either clinical symptoms or laboratory signs of acidosis were those who had been ill for some time with infections, toxæmias of pregnancy, conditions causing degrees of starvation, or were ill-nourished children. All emergency operation cases showed symptoms of acidosis, which was increased with the length and severity of the disease. Women were more liable to be affected than men, and children than adults, and the acidosis was increased by fear. Doubtless this, then, is the rationale of the pre-operation treatment of children with large doses of glucose to prevent post-anæsthetic vomiting, and it should be borne in mind when laying the blame for caries on the large carbohydrate intake in children, and perhaps the fact that children like sweets and dislike fat may be explained physiologically.

How then may this endocrine derangement be brought about? The classification adopted is mainly McCarrison's:—

- (1) In direct disease of any one gland or group of glands—e.g., (a) myxœdema, (b) acromegaly, or (c) diabetes.
- (2) Functionally as a result of over-stimulation due to acute exanthematous fevers, acute or chronic infections, bacterial toxins.
- (3) Defective or improper feeding—e.g., semi-starvation or lack of vitamins.
- (4) (a) Residence in unsanitary surroundings; (b) intestinal toxæmias, stasis and constipation; (c) consanguinity and heredity; (d) psychic causes. fear, rage, pain, worry, insomnia.

This classification is of course incomplete, but it gives an idea of the variety of causes which may bring about the condition. Limitation of time prevents discussion of these causes, except for calling attention to the question of vitamins, which from our point of view, is to-day very much in evidence. Mrs. Mellanby has shown that feeding with foods otherwise sufficient but from which vitamin A has been abstracted leads directly to deterioration of the dental tissues, and just lately Howe has even produced what would appear to be definite carious cavities in the teeth by a vitamin-free diet, but so far as I know, neither have shown the morbid process by which these effects are produced. McCarrison, however, provides the connecting link, for he proves conclusively that food deficiencies have a definite action on the endocrine apparatus, and it is through the various glands which form this apparatus that these changes are brought about in the teeth. His book on "The Study of Deficiency Diseases" gives further particulars.

Summarizing briefly, my points are these:—

- (1) So long as the enamel remains intact there can be no caries.
- (2) Enamel in health progressively hardens as life proceeds.
- (3) This hardening is due to a progressive laying down of lime salts, taken from the body store of ionic calcium.
- (4) This body store is, in health, equivalent to the need of the individual at the time, and is preserved by the endocrine apparatus, which is also the fixer of lime salts in the teeth.

(5) If the endocrine apparatus is thrown out of balance in the direction of calcium starvation, this reserve store is diminished, and fixation of lime salts in the teeth is interfered with.

(6) An upset in endocrine balance in childhood, youth and pregnancy will be in the direction of calcium starvation.

(7) Calcium starvation will lead to a diminished calcium index in the saliva, with a lessened alkalinity of that secretion, thus directly promoting caries.

(8) Endocrine derangement, leading to a loss in balance towards calcium starvation, will tend to produce a condition of acidosis by lessening the alkali reserve of the body; in the compensation of this condition the calcium salts, together with other alkaline salts, will be utilized for acid neutralization, and therefore not be available for the hypercalcifying the teeth.

(9) If the acidosis be more severe, built-up and fixed inorganic lime will be torn away from bones and teeth to help build up this alkali reserve, and thus preserves life, lowering the resistance of the teeth to caries.

(10) That without this susceptibility to caries, exciting causes such as food fermentation do not matter; but if immunity be removed hardly any reasonable care and attention are sufficient to preserve the teeth entire.

(11) That endocrine derangement, as described in this paper, will account for all the conditions leading to dental caries, whether they be diet, lack of vitamins, or altered salivary secretion.

In conclusion, other facts that I should have liked to have brought forward I must keep for another occasion. I hope, however, that I have said enough, and said it well enough, to interest you in this matter. I can hardly hope in the course of a single evening to have done more. I quite realize that much of this theory is built on hypothesis, and that many points are at present "not proven," yet much of the ground on which it rests, though uncertain, is not without probability, and is accepted as more than probable by competent observers and workers, both clinicians and pathologists. I contend also that this theory answers most, if not all, questions and difficulties that daily arise in our practice in a way that no other theory does. If, then, I have convinced even a minority of my audience of the possibilities of these suggestions so that they will not dismiss the matter, as report says it has been dismissed by one at least of the leaders of our profession, as "this rot about the ductless glands," I am satisfied.

Perhaps when the Dental Board gets to work and has funds at its disposal to organize a definite research into these matters, the factor of the endocrine glands in relation to the production of immunity and susceptibility of the teeth to caries will not be forgotten.

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DISCUSSION.

Professor HALLIBURTON said that as he had not the special technical knowledge to discuss purely dental questions, he would address himself to the general problems raised in Mr. Broderick's suggestive paper. In all of these more information was needed, and this must be based on experimental research. The principal problems were concerned with (1) calcium metabolism, (2) endocrine organs, (3) vitamins and (4) acidosis. The first in this list was the bed-rock, and until this important subject was unravelled, it was premature to attempt to explain how it was influenced by Nos. (2) and (3), important factors as both undoubtedly might be. Knowledge of both was so fragmentary and imperfect that it was unwise to formulate theories as to how they acted. There was always a tendency among enthusiasts to magnify at the start any new conceptions, and this was taking place to-day in reference both to vitamins and hormones. The whole question of acidosis was equally immature, and Mr. Broderick had only quoted those authorities whose views fitted in with his theories. While not wishing to disparage Mr. Broderick's work or to discourage a young investigator from doing more, his advice was to get your facts first and theorize later. The converse procedure was generally disastrous.

Dr. LEONARD WILLIAMS said that he had been asked to contribute something from the point of view of the endocrinologist to this important discussion, introduced with so much interest, instruction and suggestiveness by Mr. Broderick. The passage which had struck him most forcibly was the one which told of a leader of the profession referring to endocrinology as "this rot about the ductless glands." Quite a distinguished physician, too, who had written a book on dietetics, had spoken contemptuously of the "stunt of the vitamins." This was a species of irritation that progressive seekers after truth had always to encounter. If they were to take a broad view of the subject which Mr. Broderick had dealt with in such admirable detail, the question would be "What were they to do to be saved from this deadly heritage of caries." Dr. Harry Campbell, who had been discussing this question for years, contended that if they were to cease feeding their children on carbohydrate pap and to feed them as they fed puppies, on material which compelled vigorous mastication, they would ensure a normal development of the jaws and thus abolish malposition and malnutrition of the teeth with the consequent caries, pain and pyorrhœa. He (Dr. Leonard Williams) agreed with all this, but with certain reservations. The first was contained in the aphorism that they could not make bricks without straw, that was, that teeth could not be sound unless during the period of their development the calcium metabolism was good and vigorous, and there could not be good and vigorous calcium metabolism without normally acting endocrine glands. The calcium co-efficient was claimed by the protagonists of the various glands to be a function each of his own particular gland. One said it was the thyroid; another affirmed it to be the pituitary; yet another was convinced it was the adrenal, and so on; the truth really being that it was not any one of them individually, but all of them collectively. If, then, a good supply of calcium had to be ensured for the teeth, a good team of endocrine glands must also be ensured, and all this must be begun while the child was still *in utero*. In gout, as was known, they began with the grandfather; in defective teeth they need go no further back than the mother. Now, what were they to do with this mother? The answer was quite simple. They must feed her on vitamins. Vitamins were to the endocrine glands what the endocrine glands were to the economy, namely, an urgent and paramount and ever present necessity. He was therefore in agreement with Dr. Harry Campbell but he went further back. A great deal could be done with the child by means of correct feeding, but the foundations must be well and truly laid, and this could only be effected by seeing that the mortar had plenty of lime in it from the time of conception onwards. Lime was necessary to the teeth and jaws; endocrines were necessary to the lime, and vitamins were necessary to the endocrines. As soon as the child had erupted its teeth, it must be taught to masticate, and here was his second reservation to the acceptance of Dr. Harry Campbell's teaching. The child must masticate, he must work for his

living, on that they were agreed. But he would further insist that a child must not only chew, but it must chew vitaminous foods. If it chewed devitalized foods it might develop its masseter muscles and to some extent its mandible, but unless its endocrines were working normally it would not do full justice to its facial development, and its endocrines would never work normally without vitamins. The endocrine gland of most interest to odontologists was undoubtedly the pituitary. He said this, even while emphasizing the fact that the endocrine glands were like a team, no one member of which ever played for its own hand, but always for the side, the work of each member of the team reacting mutually; the cohesive element in the team, the *esprit de corps*, was represented by the vitamin. To the pituitary, therefore, he would direct very special attention. The high arched palate and the undeveloped mandible, which were so often associated with adenoids, were usually what mathematicians called a "function" of pituitary insufficiency. He did not deny the implication of the thyroid factor, but it was usually only an accessory; the real culprit was generally the pituitary. The lack of development about the bones of the face which was the sure precursor of dental troubles, if due predominantly to the thyroid, was accompanied by mental dullness, nocturnal enuresis and stunted growth—the typical cretinoid child. Where the pituitary was at fault the picture was very different. In spite of the adenoids, if any were present, they had before them a seemingly well grown child of bright intelligence. But if the mouth were examined badly-developed outside upper incisors would often be found, and inspection of the hands would reveal nails with no half-moons at their roots. In addition, there was often in these cases some vagary of pigmentation which was usually manifested by little black moles dotted indiscriminately about the otherwise fair skin. Dentists treated these undeveloped jaws by very ingenious mechanical contrivances. There was nothing against these except the inconvenience which they often occasioned to nervous patients, but if such children were treated, as they so easily might be, for their endocrine defect, the success of the instrumental treatment would be greater and the period of inconvenience much less. If dentists were going to prescribe pituitary, two points of importance needed emphasis. The first referred to preparation and administration. They must choose a reliable preparation of the whole gland, neither pars anterior nor infundibulum separately, but the whole gland, and they must give the whole gland in adequate doses. The advertised doses were far too small. To obtain any satisfactory results they must start with 5 to 6 gr. three times daily, and they need not be afraid of increased blood-pressure. Blood-pressure in children did not matter, and what was more important was the too little appreciated fact that pituitary taken by the mouth did not raise the blood-pressure. The pressor substance was destroyed in the stomach. The second point needing emphasis was that they would be disappointed with pituitary therapy unless at the same time they put their young patients on an intensive vitamin dietary. To give endocrine substances without intensive vitamins was like giving iron without aloe. A success was possible, but not certain. An intensive vitamin dietary consisted exclusively of uncooked foods, "unfired foods" the Americans called them, dairy produce and uncooked fruits and vegetables. Primitive man at one period knew nothing of the cooking stove, and yet it was precisely at that period, when living on herbs and roots and fruits, and probably on raw meats, all of which required vigorous mastication, that he had raised himself from the level of the brute to the position which he now occupied. The diet of children consisted too much of boiled milk and devitalized pap. No wonder they grew up with rickety backbones and limeless teeth. The profession had become too puffed up with pseudo-scientific pride, and in their futile endeavour to conquer truth with a test-tube they had only succeeded in boiling their common sense in a sterilizer. Man was not built up on artificial foods but on milk, and milk was what it was because it contained in itself the physiological trinity—calcium, endocrines and vitamins. And the greatest of these three was vitamins.

MR. JOSEPH A. WOODS desired to encourage the reader of the paper to continue with his researches, because although he felt that the investigations so far were not entirely convincing, yet the results were very stimulating to thought and were well worthy of further elucidation. He agreed with Mr. Broderick that the present theories

of causation of dental caries did not explain all the conditions sometimes met with in clinical experience. Mr. Woods found it difficult to accept the proposition that enamel was capable of undergoing the changes suggested; at the same time the theory that enamel once formed was incapable of any further vital change seemed out of keeping with other tissues of the body. He was not sure that he had ever observed the dental conditions during pregnancy which were referred to in some text-books, and which the author seemed to have come across. He had seen one case which he believed to have been osteomalacia. The patient was a man of middle age, and during the progress of the disease the teeth did not seem to be affected in any way. No loss of lime salts was to be observed, and softening was not to be detected when the teeth were cut by instruments.

Mr. J. G. TURNER said that Mr. Broderick appeared to try to establish two main points in the dental aspect of his thesis: (1) That diminished alkalinity of the saliva led directly to dental caries; (2) that enamel, after eruption of the tooth, underwent both increase and, under certain circumstances, a normal or at least purposeful decrease of its lime salts. Both these points were susceptible of examination from a clinical and matter-of-fact standpoint. Dealing with the first point, was it certain that saliva of whatever alkalinity ever reached the sites of dental caries? Dental caries depended immediately on the lodgment of sticky, starchy, and sugary food, together with acid-producing germs. After a meal these deposits were always to be found on the parts of the teeth unreached by any of the friction of mastication, and they were at once covered by a layer of mucus. In point of fact decay always began in these stagnation areas, never on the well-rubbed areas, a fact which at once showed that susceptibility to decay was particular to particular portions of each tooth, not general to the whole tooth or to a whole set of teeth. This meant that saliva had practically no influence over dental caries in the presence of sticky food, it never reached the occluded acid which was being formed in contact with the enamel under a covering of sticky food and mucus. It was literally like "water off a duck's back." Mr. Broderick talked of an increased susceptibility to caries in acute illness, during which the lime-content of the saliva was lowered. Apart from the fact that, supposing such an increase to exist, it was entirely conformable to the germ-carbohydrate stagnation theory of caries, such a connexion had never been proved. Observers had failed to note that the decay which became apparent during the six weeks or three months of an illness began months or years before, and, irrespective of health, was due to show itself at this time. Caries of enamel was a slow process. He (Mr. Turner) did not know of any published observations tending to show its rate of progress, but by grinding sections of children's teeth, chiefly six-year molars and bicuspids, the age of eruption of which was approximately known, estimating the length of time the tooth had been in the mouth from the age of the child and the age of eruption, and noting the depth to which enamel destruction had progressed, he had been able to determine that the least time required to perforate the enamel at the buccal gum edge (this was not the extreme edge of the enamel) was nine months, and at the abutment area two years. Such figures entirely put out of court all observations hitherto made on decay of illness or pregnancy. Turning to the second point, that the enamel underwent changes of composition, Mr. Broderick asked them to believe that enamel hardened continuously during life, unless some untoward influence such as disease or pregnancy interfered, when it might suffer a diminution of lime salts and apparently rehardened later. Mr. Broderick seemed to believe these changes were brought about by lime salts in the saliva. Certainly this was a hard point to prove! If the contention already advanced, that mucus was impervious to saliva, were correct, salivary action fell to the ground at once. Clinically he did not know of the hardening. He (Mr. Turner) knew of the difference between hypoplastic and well-formed enamel, and in defiance of Mr. Broderick's opinion, the hypoplastic enamel remained soft throughout life. Dentine certainly hardened with age, so long as the tooth-pulp persisted, and it was to this increased density of dentine that the deeper colour of older teeth was due, not to alteration of enamel. Purposeful withdrawal of lime-salts from enamel seemed too fantastic for argument, but he might point out that by the time, during the illness, the decay was observed, the lime-salts had mostly

disappeared. Mr. Broderick suggested this purposeful destruction of enamel as an explanation of the absence of enamel in so-called arrested caries. This was a very unfortunate suggestion. In the human subject arrested caries was found under two conditions—in the temporary molars where the enamel was normally not very hard and readily broke away when undermined by decay; and far more frequently in hypoplastic permanent first molars, where the missing enamel had practically never been found. Moreover many of the subjects of "arrested caries" had never been ill again since the original malnutrition of childhood which caused their hypoplasia. As to the dentine, he was extremely doubtful whether either hardening or absolute arrest ever occurred. He had never indeed seen truly arrested caries: destruction of dentine always continued, modified by the smooth, clean surface produced by attrition. Mr. Broderick quoted Head's experiment, but here it was at least possible that the saliva had completed the unfinished work of the acid, and what had been left was only a deeper layer of the enamel. The hard gouty teeth and the soft tubercular teeth Mr. Broderick talked of were quite unknown to him (Mr. Turner); and he would controvert another point Mr. Broderick put forward as if it were current dental knowledge. He did not find that pyorrhœa and dental caries were mutually exclusive: on the contrary he found them commonly associated, and there was one form of dental caries so directly associated with pyorrhœa that he had called it "decay of pyorrhœa," namely, that found at the necks of the teeth and on the roots of the teeth, as the destructive processes of pyorrhœa lay more and more of the tooth open to the attack of caries. Mr. Broderick quoted Dr. Kirk, that youth was the period of greatest susceptibility, the tendency thereafter being towards immunity in adult life. That dental caries was pre-eminently a disease of youth was certain, but in his experience the predisposition to decay remained unaltered throughout life. The observed lessening in the incidence of decay was to some extent the result of fewer sweets and some effort at cleanliness, but was due more to extraction of decayed teeth which allowed of "drainage" of the remainder. In spite, however, of this lessened incidence, the expectation was that in the absence of efficient cleaning every tooth would eventually become carious. Finally, he must challenge Mr. Broderick to substantiate almost his opening sentence. Mr. Broderick asserted that "these theories (i.e., the germ-carbohydrate stagnation theory) . . . left too many questions unanswered, too many undoubted clinical facts unexplained." What were they? He regretted having to present nothing but destructive criticism, but he was sure Mr. Broderick would none the less continue his search for truth.

Mr. HOWARD MUMMERY observed that he had not expected to be called upon to take part in the present discussion, but would confine his remarks entirely to the histological question, as this was of great importance in determining if there were channels in the enamel through which such interchanges as were suggested by Mr. Broderick could take place. He had never found it possible to believe that they could have in the living human body an absolutely inert, impenetrable, dead substance incapable of change or nutrition. The descriptions of enamel in the text-books had usually been confined to enamel of perfect structure which did not appear to show any channels or spaces in the calcified tissue; at all events refraction did not allow of their detection. Enamel, however, despite emphatic statements to the contrary, was not usually of perfect structure, and showed many areas which were distinctly penetrable by stains. Professor Walkhoff, although denying the presence of organic matter in the enamel, said there were scarcely any teeth of civilized man which did not show some defects of structure in the development of the elements of the enamel either in their size and arrangement or with regard to their calcification. Both Walkhoff and Leon Williams had found similar defects in the enamel of the anthropoid apes. If there were spaces in the enamel communicating with living tissue in the dentinal canals, and staining methods certainly showed that such communication did take place, they must be penetrated by lymph, and even if not occupied by solid organic material, would probably serve as channels for the interchanges of the body fluids. Mr. Mummery had recently repeated the staining experiments first described by Mr. Douglas Caugh in 1904, by several different methods, and he had not found a single specimen that did not show staining areas in more or less abundance. With regard to penetrability from without,

the progressive hardening of the teeth with the growth of the body had, he thought, been established by the experiments of von Ebner, von Beust, Pickerill, Gottlieb and others, as the stain had been shown to penetrate less and less deeply into the enamel as age advanced.

Mr. BRODERICK, in reply to Mr. Woods, suggested that Mr. Mummery's work showed the possibility of the changes in the enamel which he had mentioned. With reference to the increased susceptibility to caries during pregnancy, it was pointed out that there was, in this condition, a physiological change in the endocrine balance, the results of which would be a greater conservation of lime salts; consequently it would only be in those women with a tendency to calcium hunger that tooth destruction would be noticeable. Mr. Broderick also suggested that possibly Mr. Woods might be mistaken in his diagnosis of osteomalacia; the sex and age of the patient pointed rather to Paget's disease or osteitis deformans, a condition in which, although in the early stages the bones were apt to become bent, there was actually an increase in thickness. Many of Mr. Turner's conclusions were so contrary to those of his own that he must leave it for the dental profession to judge between them, but he would point out that, whereas Mr. Turner fixed his mind on exciting causes, he (Mr. Broderick) was considering predisposing causes only, which might account for the disparity. At any rate he had found that food lodged quite as much between the lower front teeth, especially in older people with some recession of gum, as elsewhere, yet these teeth were almost immune to caries. Mr. Turner mentioned the "decay of pyorrhœa," by which term he designated cavities at the neck and roots of teeth laid bare by absorption of alveolus; but, as already pointed out, by caries, in this paper he (Mr. Broderick) meant progressive destruction of enamel, which was not present in these situations, and the destruction of cementum did not in any way upset the premises that caries and pyorrhœa were antagonistic. With regard to the questions unanswered and the clinical facts unexplained by the carbohydrate stagnation theory, he would refer Mr. Turner to previously published communications he (Mr. Broderick) had made on the subject.

Section of Odontology.

President—Mr. MONTAGU F. HOPSON, L.D.S.E.

Causes, due to Vulcanite, of the Misfit of Dentures, and Some Remedies.¹

By DOUGLAS GABELL, L.R.C.P.Lond., M.R.C.S., L.D.S.Eng.

MY paper contains one or two original practical suggestions, but I propose to review the whole process of vulcanization in the order of its execution in the workroom.

Impression-taking has improved sufficiently to demand better vulcanizing methods in order to obtain better practical results at least in those cases in which (1) the mucous membrane is thin and the mouth not perfectly flat; (2) the overlaps of the plate into the sulcus are considerable; (3) the bite is high and it is desired to use long cusped teeth; and there is in bulky cases the risk of the rubber drawing away from the porcelain teeth. I presume the cast to be perfect.

The difficulties arise from the series of physical changes which occur in the rubber during vulcanization. On heating, it becomes soft and viscous, flowing easily in wide channels, but with great difficulty over rough surfaces; it swells with great force, expanding 10 to 12 per cent. from 60 to 320° F. But at 250° F. chemical changes cause a contraction of 4 per cent., which takes place more rapidly than the vulcanization and is practically complete before vulcanization is half done. As it cools rubber both shrinks and also becomes less and less fluid. The shrinkage is about 10 to 12 per cent. and is irresistible, and part of it must occur after the rubber has lost its fluidity. This will produce local hollows or a general shrinkage and an internal stress which may reappear, either at once or a considerable time afterwards, as a warp. Also rubber increases in weight (and in volume) 0·2 to 3 per cent. in proportion to the surface area. The expansion and shrinkage are, in practice, very irregular, as it is practically a contest between the rubber and the plaster, complicated by the force used to pack and close the flask and the gain in weight.

Rubber during vulcanization gains weight, sometimes as much as 2·5 per cent.; this occurs when invested in plaster, tin foil or French chalk, and appears to vary according to the surface area far more than to the bulk. It has been said to be due to absorption of water, but I know of no evidence in proof of this. I am quite ignorant as to the reason for this increase in weight, but it is a practical matter as compensating for the shrinkage due to chemical action, and being variable according to surface area, the same rubber will appear to shrink more or less according to the shape of the object. Thus tables of shrinkage are unreliable, and we see why a thin, even plate will fit better than a more coarsely waxed up one.

¹ Most of the experiments on which this paper is founded were carried out at the Research Department of the Royal Dental Hospital at the expense of the School.

FLASKS.

Flasks must be rigid and strong and afford good support to the investment, even when partially open, and yet leave the work accessible for packing. Expansion and contraction with heat constitute no defect. The parts should fit each other well and open and close with the least possible friction. To hold the rubber in shape heavy metal flasks are employed, but if a flask were filled with rubber and vulcanized, the rubber would burst the flask open in some way and become distorted. This trouble is prevented (*a*) by allowing the flask to open under some form of spring pressure; (*b*) by the porous, compressible plaster investment between the rubber and flask; (*c*) by providing spaces to temporarily accommodate the rubber and by using as small a bulk of rubber as possible. All my experiments with tightly bolted flasks show a distortion of the investment, small alterations in the bite, and contraction or stress on the cast. I have noticed a raised bite in a bolted flask from the pressure on the plaster which did not occur under precisely similar conditions when only spring pressure was used.

To protect the investment allow the flask to open slightly. This obviously allows room for accidents. It is essential that some pressure should be still kept on the rubber and that the opening should be equal all round; $\frac{1}{2}$ to 1 mm. is enough.

I am not going to invent any new flasks, but would like to show you a few modifications of the fastenings of old ones.

I do not approve of the heavy spiral spring placed loose on the flask and compressed by a clamp of some sort or other, because it can be easily tilted and readily allows, even encourages, uneven opening, and it is important to avoid this. No amount of pressure later on can rectify unequal opening. If a central spring of this sort is used it must be so fixed to the clamp that it cannot tilt.

Flasks closed by bolts may be easily converted into spring flasks by inserting spring washers under the nuts.

I have also had good results by drilling slightly oblique holes into a flask and clamping the halves together with a still more bent piano-wire staple, but it is rather troublesome work keeping the holes clear and fitting the staples.

I prefer a piano-wire pin (1 mm. thick) through the lugs of the flask, the latter having a full millimetre bevel to allow of opening.

There is also the Brunton flask.

A box of thin very resilient metal enclosing a low vacuum, and nicely fitting into a bolted flask would be almost ideal if practicable.

INVESTMENTS.

I do not think that the elasticity of plaster has received its fair share of credit, neither do I believe that it alone is equal to the stress often put upon it. Dry plaster might be equal to the strain, but when deteriorated by the wet steam it is often distorted during vulcanization. Superheated or dry steam is far less harmful and I strongly advocate its use; how to get it I will explain later on.

Spence plaster, and—far more—Alston stone, deteriorate under steam very distinctly.

The part of the investment formed by the cast is of course of the greatest importance, and should be an accurate reproduction of the impression and less liable to distortion than the rest of the investment, and be smooth and polished.

It is possible to foresee where the vulcanite shrinkage will cause a bind or rock and the cast may be padded to allow for this. Plaster of Paris is weak and liable to slight expansion. Alston stone expands 8 per cent. and weakens far more than plaster. Spence plaster is stronger and does not expand but is not very smooth. Hollow tin is strong, insoluble, does not expand and contract appreciably and is polished; and in spite of Professor Gysi's work I hold it to be the best. It will yield to a very heavy strain, and so must not be overtaxed. A hollow tin cast does not act like tin foil because it is stiff; the tin foil separates from the plaster and moves with the rubber. A flask and investment having an expansion and contraction equal to the vulcanite would be of very great help.

A very thin hollow tin cast will not show any contraction, the shrinkage on cooling being only in the thickness of the metal. A thicker cast shows the normal 0.5 per cent. contraction. To get a thin tin use the sand rather wet and as cold as possible; the tin should be perfectly fluid but no hotter than necessary; hold the mould in one hand and the ladle in the other, fill the mould to the depth of 1 in. over the highest part and at once pour back the surplus. The back of the tin should be smooth (crystalline means tin too cold or left in too long) and complete (holes mean tin or mould too hot). Fill in with plaster of Paris and when that is set remove from the mould and trim off surplus tin.

Whenever possible invest the case in one half and the cast in the other, commonly called the "inverted" method.

It may be said that I have overlooked the obvious relief from pressure to be obtained by cutting gates, and I may be expected to discuss the rival patterns advocated; I totally disapprove of any gates, and I think that their inapplicability should be obvious to you.

With a bolted flask and gates we will presume the case to be well packed, holding 100 per cent. rubber. On heating the rubber it expands 10 per cent., and this amount escapes by the gates; then on vulcanization the rubber contracts 4 per cent., and rubber cannot flow back into the mould, it is too viscous and the surface friction is too great, and so shrinkage occurs somewhere; next, in cooling, the rubber shrinks 10 per cent. more, and again shrinkage occurs, 14 per cent. in all. It is not so bad actually because the rubber cannot get out with ease and the plaster investment yields, often permanently; in any case, the better the gate the greater the final shrinkage. Gates are bad, and the second new point I wish to present to you is a very simple device for keeping all the rubber, indeed a little extra rubber also, in the mould even when the flask opens slightly.

It consists of a 1 mm. vertical step all round the mould before the horizontal separation. I show it made at the edge, but it may be made at any level. With this a 1 mm. opening of the flask will give the mould itself a 10 to 20 per cent. greater capacity, and when the rubber shrinks the mould can be closed again without strain. It may leak a little, but nothing like the gate leaks, and by this device and dry steam I can make the fit and height of bite more accurate. It is a simpler method than the temporary metal diaphragm to be spoken of later.

The investment is important in other ways than retaining the rubber in the mould. A rough surface holds the rubber as it cools, especially at the lower temperatures, more than a smooth surface, and so when the rubber contracts it draws away at the smooth parts first, and if this takes up the volume of shrinkage, at the smooth part only. Here then is a way of directing the

shrinkage which we are unable to prevent, and putting it into the less important places. Price, Snow and Gysi have taught this, the two former by advocating pits and roughness to hold the rubber, the latter by using tin foil to liberate the rubber; French chalk and soluble glass have much the same effect as tin foil. So that a polishing plate has a very important effect on the fit of the denture to the cast as well as giving a good surface to the vulcanite; it, so to speak, absorbs the shrinkage.

There is another point with regard to investment, which is material but little considered, viz., the rate at which it conducts heat. By investing the cast in a good conductor (e.g., copper wire and plaster) or very close to the bottom of the flask, and the superstructure in a bad conductor (e.g., pumice and plaster), and after vulcanization rapidly cooling the lower half of the flask, the surface against the cast can be stiffened first and so made to resist contraction; perfectly against all small pitting, and very largely against a general bend. Here then we have another way of directing the shrinkage which we cannot prevent. Internal strain will I fear be large.

I prefer to regard the tin foil and heat conduction methods as side issues, useful reserves, and as complications liable to spoil our work if we neglect to consider them and prevent their unnecessary occurrence in undesirable places; I have seen a very bad misfit arise from keeping the cast hot longer than the rest of the rubber.

Obviously anything that preserves the strength of the investment is very important. Use a thick mix of plaster, allow it time to set well, and put the flask under pressure immediately after filling the last half to prevent the expansion of the plaster bulging the parts open.

Boiling out presents no opportunity for improvements, but remember that plaster is soluble in hot water.

After boiling out the wax little pits may be made in the mould wall at any points where it is particularly desired to hold the rubber to place. Also, it should be remembered that any escaping rubber will effectively hold the rubber near it, and so tend to prevent shrinkage in that plane.

Polishing any part of the mould with French chalk or soluble glass will rather favour separation of the rubber from that part, i.e., encourage local shrinkage.

Gates should not be cut, nor the edges of the mould trimmed or bevelled.

HEATING OF FLASK AND RUBBER.

I prefer dry heat for warming up the flask on account of preserving the plaster investment; the rubber cannot be overheated so long as any moisture remains in the plaster. It is difficult to heat the rubber evenly and quickly. For very careful workers I might suggest the use of a water-jacket oven with a lump of lead to rest on the flask whilst warming up, and a saturated solution of common salt which boils at 108°C ., sodium nitrate (120°C .), potassium carbonate (133°C .), calcium nitrate (150°C .), or calcium chloride (178°C .) in the water jacket. Use a "cosy" to keep the flask hot whilst packing and squeezing up. I have no improvement to suggest for rubber heaters.

CHOICE OF RUBBER.

We all know that black rubber is the strongest and lightest, but most liable to shrink, whilst pink is weak and heavy, but shows very little shrinkage; but I have not seen any actual figures as to the comparative strengths of rubber, so I append a short table that may help to the best choice of rubber for any case:—

	Specific gravity of vulcanized rubber	Shrinkage (chemical change)	Stress (recovered from)	Bend (under stress)
Black ...	1.2	5.0%	20 lb.	22°
Red ...	1.8	4.0%	18 lb.	18°
Pink ...	2.4	2.0%	12 lb.	6°
"All pink" ...	1.8	3.5%	12 lb.	11°

These are average results from several brands, and for well vulcanized bars 5.5 mm. square and 32 mm. long. It does not show the breaking strain, but the stress the rubber could stand without permanent set.

PACKING.

Cleanliness is essential whilst packing. It is not generally known that new rubber will join thoroughly to old rubber without any dovetails if both are quite clean. Air and water bubbles can become accidentally enclosed between layers of rubber, or between rubber and any impervious surface such as teeth, tin foil, &c. The porosity of plaster is in this respect a great protection from evil results to the careless packer.

Even packing is a great safeguard against distortion of the cast and investment; given, however, ample time, heat and gentle pressure, a large error in distribution can be remedied whilst closing the flask in thick cases, but only small errors can be so corrected in thin cases. It is very important that flasks under spring pressure shall open evenly, and therefore also even packing is important. Rubber and flask should be kept hot during packing and closing. Trial closing should be done with great care and slowly. Closing the flask with the centre screw after the flask has been in the vulcanizer is not the best method; the case should be correctly packed before going into the vulcanizer.

The quantity of rubber used is very important. There are several ways of estimating it. (a) Weighing the case in air and then in water with gramme weights, the difference in weight will give the volume of the case in cubic centimetres. Deductions will have to be made for the teeth, clasps and strengthener, and an addition for the misfit of the wax case on the cast. (b) A voluminometer may be used; capillary attraction makes the readings rather difficult, and the same allowances have to be made. Both methods are troublesome, but I have used them largely for checking my results. (c) The Hall flask is a newer method of securing the correct amount of rubber, but it is left to your judgment to estimate the overpack. (d) Lastly I come to the old rule of thumb method of judging by eye after trial, closing with wet linen between the halves. It is easily the best method for a careful worker; the least troublesome, most reliable and surprisingly accurate.

But in packing it is not just a 100 per cent. full mould that we need, but 105 per cent. for black rubber, 104 per cent. for red, and 101 per cent. for pink rubber in order to allow for the chemical shrinkage of the rubber and also about 3 per cent. more to allow for leakage of the mould. Too little will cause shrinkage of the case, too much straining of the investment.

In the British method after securing a correct pack you have to learn to judge the volume of the case and that of a gramme of rubber and then add the desired amount, spreading it evenly over the work, or use a temporary diaphragm sufficiently thick to give the required overpack. In the use of the Hall flask you must loosen the bolts and inject more rubber. One and a half full turns equal 1 gm. of black rubber. With the other methods it is just a case of arithmetic. Let me repeat that accurate packing is of very great importance.

Various metal skeletons or strengtheners have been suggested for pre-

venting the shrinkage in important places (e.g., the cast surface and bite) and diverting it to less important parts (e.g., the contour and thickness). The holding out of one part will increase the shrinkage in another. The strengthener itself may be bent and convert a local misfit into a general warp. The thing must be looked at from a comprehensive point of view and an efficiently rigid and extensive skeleton provided and room be left for accumulated shrinkage and internal stress to have play in harmless parts. Skeletons meant to preserve the general shape often need bracing outside the denture during vulcanization, e.g., a straight bar from right to left molars soldered to the internal skeleton and cut away after annealing and polishing. It is not a very trustworthy method.

Diluents can be used to reduce the very objectionable bulky masses of rubber; they are better than skeletons in that they actually reduce the amount and force of expansion and contraction. Hollow aluminium boxes of different sizes and adjustable lengths are sold by the depots, old rubber, pumice and cork are sometimes included within the new rubber.

Air spaces are difficult to produce accurately. The walls are liable to collapse and it is difficult safely to control their thickness. Retention pits in the investment are more reliable than rubber solutions. A mild solid explosive that would go off on lowering the steam pressure would be useful; methylated spirit is the nearest I have got in actual work. When using air or gas spaces be sure to let off steam quickly and chill the surface of the case as soon as possible.

Close the flask to within 1 mm. of being shut and so that the springs have a grip; the springs should be strong enough to resist the swelling rubber sufficiently to press it into all crevices, yet weak enough to save the plaster investment from injury. I would like to remind you that a great many packing errors are righted by the swelling of the rubber if under pressure—it is another great protector from evil results for the careless worker, an advantage not frivolously to be discarded. Unless you have a central closing vulcanizer the spring must be strong enough to close the flask against friction, when the rubber cools. One of the greatest uses of a spring is to protect the investment from damage during the swelling of the rubber.

VULCANIZING.

Vulcanize at a low temperature for a long time to prevent shrinkage. It

90 per cent. of the expansion and contraction trouble and so spares some

I prefer to stress on the investments, also a slowly increasing stress is easier plaster investment—there is time for adjustment. Practically it is easier to get a remains in the 175° F. for eighteen hours than even two hours to reach and one For very careful I have not tested rasher methods but have reason to believe a lump of lead to worse results. The strength of the rubber is equally good by of common salt when 6 mm. thick pieces, but below 300° F. there is safety carbonate (133° C.), c

the water jacket. Use be turned into expansion by overheating bulky rubber to squeezing up. I have viscosity. I have accidentally obtained a good fit this way it as a practical method. It does explain some good

We all know that bla closing the flasks whilst in the vulcanizer is useful, it to shrink, whilst pink is or springs and applies the pressure at the desired time. I have not seen any actuy spring between plunger and flasks. so I append a short tablul your attention to the alterations I suggest in the case:—

vulcanizer. With the flame placed under the boiler and the top open to the air there is uneven temperature at different levels in the metal work and as the flasks are in contact with the hot bottom there is conduction of heat by the metal as well as by the steam. The bottom flask is hotter than the top one in a three flask vulcanizer and should the steam escape by accident the vulcanite will be soon charred. Also the steam is wet, being always cooler than the water. By putting the flame at the side of the vulcanizer and putting a loose "cosy" over the top several advantages are secured:—

(1) The plaster investment and cast are far less injured by the superheated dry steam and resist distortion and leakage better. I have not yet seen a difficult full black rubber case made to fit the cast and bite at one vulcanization in the old form of vulcanizer. I have with this new form.

(2) The temperature throughout the vulcanizer is equal, top and bottom flask alike.

(3) In case of accidental leakage of the steam, so that the vulcanizer runs dry, the vulcanite is not destroyed.

Even with a small flame at the bottom a dry vulcanizer very quickly means a burnt case from heat conducted through the metal, but with the side flame the heat must travel down through the air-cooled quarter of the boiler before it reaches the flask. With a big gas supply damage would doubtless occur in either case but by using a safety tap this can be obviated and cases left all night to vulcanize without any fear. I will return to this device presently.

(4) The thermometer in the lid becomes a more accurate record of the temperature in the flasks, but it remains very fallacious, registering say 15° F. too low at low temperatures and even more than 15° F. too low at high ones. If the vulcanizer run dry it will fall further still below the temperature of the flasks. Use a thermometer as a check on the gauge, which may get out of order, but a good gauge is a very accurate record of the temperature in the flask.

(5) The vulcanizer appears to keep much cleaner.

(6) The rate of heating up is under better control.

A too big flame is apt to fuse the safety plug sooner than one in the old position, but very quick heating up is bad for vulcanite; I admit the urgency in repair work.

Returning to discuss the safety tap and its uses: It is a quadrant tap with the plug filed out on each side so that for about 80° of turn it varies the volume of gas passing (ordinary taps only vary the flow for about half this range), and it can be used in place of the ordinary tap or in addition to it. The brass quadrant has a few arbitrary numbers on it for reference. The full bore of the tap should be sufficient to run the vulcanizer up to 90 lb. in half an hour. As the heating capacity and pressure of gas is liable to considerable variation, the sizes of vulcanizers vary, and also the temperature around them; there is no use in putting actual temperature markings on the tap. After a few trials it will be easy to find approximately:—

(1) To what temperature each figure on the quadrant will raise the vulcanizer.

(2) How long (starting from 212° F.) each figure will take to raise the temperature to this maximum.

When about to vulcanize a case light the flame, put in 2 or 3 oz. of water, loosely lay on the lid and open the steam tap and turn the safety tap full on so that when the flask is ready to go in the whole vulcanizer is at 212° F. There is nothing gained by running the heat up slowly below 212° F. Put in the flask, close the lid firmly and soon afterwards close the steam tap. Adjust

the gauge to the desired temperature and the safety tap to give a little higher temperature than the gauge for long low temperature vulcanizing (e.g., gauge 275° F., tap "1½," and allow eighteen hours from the time of closing the steam tap), or for quicker work put the gauge at the desired temperature for vulcanizing and the tap at a mark that will take one or two hours to reach this maximum (e.g., gauge at 90 lb., tap at "3," in about an hour's time move the tap to "4," and time to turn out one hour after the gauge registers 90 lb.). I advise the slower method for all cases where it is desired to get an accurate fit.

When vulcanization is complete open the steam tap and let down the pressure quickly; if you have a centre closing vulcanizer *now* is the time to put extra pressure on the flask; if you rely on spring pressure only, it is well to get the flask out quickly and close it more firmly in the vice or a spring protected press whilst it is still hot. Do not open it. Turn off the gas if you are not going to anneal the case in the vulcanizer.

ANNEALING.

There will certainly be internal stresses within the vulcanite from the hindrances of the investment to shrinkage during the last part of the cooling, and I like to release these as far as possible before removing the case from its cast or investment, so instead of opening the flask as soon as it is cool I prefer to loosen the springs, replace it in the vulcanizer and slowly raise to 25 lb. pressure, then turn off gas, retighten the springs and allow the case to cool slowly and thoroughly. If the case is on a tin model, and does not come off perfectly easily, I place the tin cast and case, freed from plaster, in a saucepan and boil it and chill it alternately until it parts readily. I have not been able to relieve all internal stress in difficult cases.

I believe I have been able to prevent the small hollows which are apt to occur in the fit over the ridges by investing the tin cast in a heat-conducting investment, and the other half in plaster, and after taking the hot flask out of the vulcanizer placing the bottom half of the flask in cold water, and so quickly chilling the fitting surface, afterwards annealing as described above.

A little careful observation will soon show you that a lot of the warping attributed to careless polishing is really due to the recovery of the vulcanite from the restraint of its investment. A case that fitted its cast nicely directly after removal from the flask, if left for a short time, will be found to fit less well. Again, large pieces of continuous gum mounted in vulcanite that were sound when removed from the flask, and even after polishing the case, may be found cracked twelve hours later. I have seen a case warp appreciably in five minutes immediately after removal from its investment. This warping is the most difficult problem to solve.

Double vulcanization is a practical, if "patchy" way of securing a better fit. If determined on from the first, I would suggest the cutting off of all overlap in the first bake, so that the palate may settle more freely into place, then at the second bake add the overlaps and soundly fit the top of the ridge. Or the method may be combined with the production of hollows where bulk and strength will permit. Or again the case may be cut free where it binds or rocks and be just relined. Contrary to what has been stated, I do not believe that revulcanizing causes any further shrinkage in rubber. I think warpage from internal stress has been mistaken for actual shrinkage. Warpage from internal stress is very little relieved by relining.

Half vulcanizing the case with a diaphragm to keep the flask a little open,

then removing it when the shrinkage from chemical changes has occurred and completing the vulcanization is troublesome with wet steam, though easier with dry; and it fails to solve the bigger trouble of cooling shrinkage, unless central closing is available.

ADJUSTMENTS.

There remains one more remedy for misfit that is very commonly practised, viz., "easing the case." It is objectionable from the point of view that the patient has to suffer to provide the indications, and may be resentful and intolerant, perhaps slightly contemptuous of your failure to fit the case properly at the first attempt, but at least his confidence in you will be tested. The depth, position and extent have to be judged, and so may be over or under done; owing to the inflammatory swelling produced it is often over done, and valuable holdfast sacrificed. It may displace the denture. It has to be diagnosed from other causes of pain that should not be "eased," e.g., pressure due to incorrect bite, locked articulation, or sloping foundations. A "rock" is not always easy to locate, and may be broad enough not to cause pain or redness, though it sometimes may be detected by being the only clean area of a dirty plate. It is popular because it is simple, and effectual in relieving pain. With a little study of the shape and size of the mouth and its softness, and observation of the bulk of the denture, the places most likely to rub may be foreseen, and the easing done at once and with greater accuracy than by waiting till inflammation has occurred. The subject is too complicated for much discussion, and indeed is unworthy of it, but I will show you the probable effect of 3 per cent. even shrinkage in a few cases.

Instead of easing the case the cast may be padded where a bind is to be expected or a rock will have its axis, and this padding—or making a cast that is permanently or equally expanded by heat—is the only method I know of that will perfectly abolish the effects of internal stress.

Heating the case and refitting it by pressure in a shot swager or similar device, or even doing it by hand is a poor method, apt to disturb the articulation and to recur very readily, though it is useful for correcting accidental warps which have already a tendency to return to their old shape.

I hear that a cold vulcanizing method has been invented, and this may be of use to us.

To recapitulate: Remember the powerful expansion and contraction of the rubber, and the weakness of the resilience of the investment, and do all you can by craftsmanship to protect the latter from overstrain.

(1) Use meter-metal base plates, and trim the wax as small as possible before flasking to avoid bulk.

(2) Use thin tin casts on which to vulcanize.

(3) Tin foil the palate and labial and buccal surfaces almost up to the teeth.

(4) Use a spring-closed flask that is safe to open evenly.

(5) Invest in strong plaster with a 1 mm. vertical step all round.

(6) Notch the plaster near the teeth.

(7) Pack the rubber evenly, and about 7 per cent over full.

(8) Insert strengtheners or diluents.

(9) Vulcanize in dry steam for 18 hours at 275° F. protected by a safety tap.

(10) Increase the spring pressure with centre closing screw as the case cools, not as it warms up.

(11) Anneal the case before taking it off the cast.

(12) Ease with discretion.

Mandibular Sarcoma in an Infant.

By STANLEY MUMMERY, L.R.C.P., M.R.C.S., L.D.S.

THE patient was a baby, aged nine months, the daughter of a medical man. In February of last year, a swelling developed in the left side of the mouth, and the local doctor who examined it, finding that it was cystic in character, inserted a trocar and cannula, and drew off a quantity of serum. The swelling very soon re-formed, and on March 18 the patient was brought up to Sir Arbuthnot Lane, who, thinking that it was possibly dental in origin, asked me to examine the child.

The condition when I saw it was as follows: There was a large swelling in the left cheek, pressing up the lower eyelid, and filling the left side of the mouth nearly to the middle line. The swelling was tense and fluctuating, and appeared to spring from the posterior portion of the lower left mandible. No œdema was present. A hard body, resembling a tooth, could be felt on the outer side of the tumour next the cheek. A radiograph showed the swelling to be cystic, with a definite limiting wall; the second temporary molar was embedded in its outer wall and was raised half an inch above the mandible. A shadow, which I took to be the germ of the first permanent molar, could just be distinguished in the mandible below the swelling, but, owing to the imperfection of the radiograph, which was unfortunately damaged, it was not possible to determine whether the crown of the tooth was within the cyst. For the same reason I regret that I cannot show it on the screen.

On the strength of the clinical and X-ray evidence, I thought that the growth was possibly a dentigerous cyst, and after discussing it with Sir Arbuthnot Lane, it was decided to operate the next day. At the operation the mucous membrane was incised disclosing a purple cyst wall beneath. This was gradually dissected out until the posterior part was reached, which was found to be firmly adherent. The tumour was then opened, and a quantity of blood-stained mucilaginous material and serum evacuated.

Portions of the wall and contents were forwarded to Dr. Eastes for examination, and a second sample, including the displaced temporary molar was sent to my father, Mr. Howard Mummery. The result of the two examinations was interesting.

The specimen of cyst wall examined by my father showed epithelial strands, and traces of an epithelial lining in places, thus bearing out the clinical resemblance to a dentigerous cyst. The mucilaginous material, examined by Dr. Eastes, proved to be sarcoma of the small round-celled variety.

Upon receipt of this report, Sir Arbuthnot Lane decided to try radium, and a small tube was inserted into the wound for six hours.

A week later, when I saw the child again, the whole tumour had practically disappeared; beyond a slight fullness in the sulcus, perceptible to the finger, there was no sign of any swelling whatever.

No further change took place for several weeks, but on June 14, nearly three months after the operation, the child was brought up with a recurrence of the swelling. The tumour was then of the same size and in the same position as formerly.

On June 16 a second operation was performed by Sir Arbuthnot Lane, of a rather more extensive nature than the first. The contents of the growth were less fluid than before, and resembled transparent jelly. The rough exposed bone of the mandible could be felt at the bottom of the wound.

A further report from Dr. Eastes corroborated the earlier one.

Radium was again employed, and the tumour again shrank almost to vanishing point.

On October 22 I saw the patient again, as the lower temporary canine on that side had become loose, and was giving trouble. The tumour had reappeared but was of nothing like its former dimension. I removed the canine, which was embedded in sarcomatous tissue and inserted a small radium tube into the socket for a few hours.

This was the last time that I saw the child, though the father kept me informed of its state from time to time. It lived until January of this year; by that time the sarcoma had made rapid progress, entirely filling the mouth and pressing on the fauces. Towards the end feeding became impossible, and partial asphyxia hastened the inevitable fatal termination.

Small round-celled sarcomata are frequently cystic in character, and in Green's "Pathology" the author, when referring to these growths, says: "They are exceedingly vascular, the vessels are often dilated and varicose, and from their liability to rupture, frequently give rise to ecchymosis and to the formation of blood cysts."

Is it possible that this was primarily a dentigerous cyst arising in connexion with the developing first permanent molar, and that the sarcoma started afterwards either in, or in close connexion with the cyst, or is it more reasonable to suppose that the whole growth was sarcomatous from its origin, but of an unusually cystic nature?

A Maori Skull of Dental Interest.

Shown by MONTAGU F. HOPSON, L.D.S. (President).

IT will be remembered that nearly ten years ago, Professor Pickerill¹ read a paper on "Some Pathological Conditions in the Teeth and Jaws of Maori Skulls in New Zealand," in which he drew attention to the fact that all Maori skulls present a remarkable degree of attrition of the teeth, the first permanent molars being the chief sufferers. Caries is comparatively seldom seen. Evidences of multiple alveolar abscesses are frequently met with, such suppuration being due to attrition overtaking adventitious dentine, and thus leading to pulp exposure. He described the attrition as not being due to hard food, but to a combination of a slightly fibrous diet combined with the acids derived from fruits and berries. Further, he showed cases in which the first permanent molars had undergone a slow dislocation lingually, and had become so worn by attrition, that occasionally an almost complete longitudinal section is apparent.

Dr. Bernard Myers has very kindly placed in my hands a Maori skull which exhibits this particular pathological condition in a more marked degree than in either of the specimens shown by Professor Pickerill. In the maxilla all the teeth are present, and free from caries. All, with the exception of the second and third molars, show excessive attrition. The second premolar, and the first molar on the right are dislocated lingually. Both had given rise to alveolar abscesses. The buccal roots of the left first molar have disappeared, the palatine root alone persisting. There is no dislocation and no sign of acute suppuration in connexion with this tooth.

In the mandible both second molars have been lost. The first and second premolars on the left had given rise to acute suppuration, and so had the right

¹ *Proceedings*, 1912, v (Sect. Odont.), p. 155.

first molar. Both of the first molars are dislocated lingually. The right molar is practically sectionized as the result of attrition, and has a large absorption cavity in the alveolus. The left first molar shows a slightly less degree of dislocation and attrition, and the absorption area around the apices of the roots indicates a more chronic type of inflammation.

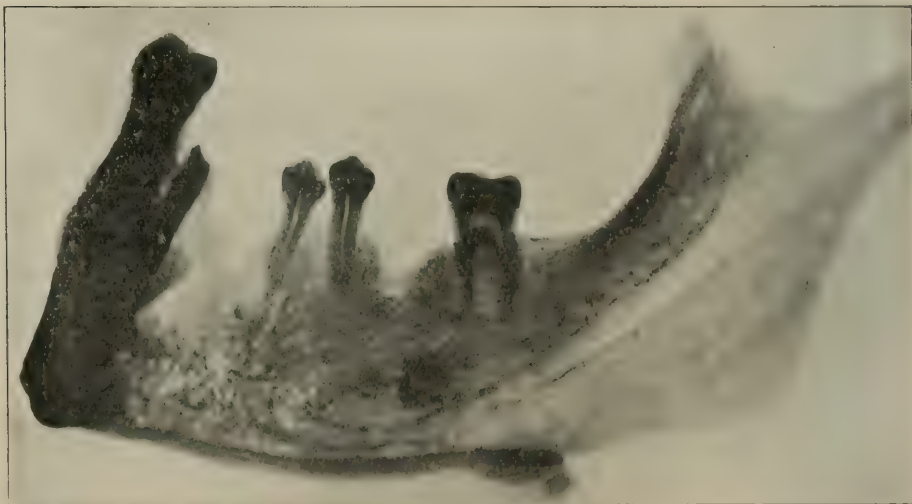
In the paper already referred to, Pickerill suggested that the dislocation was due to the following factors: The pulp having been opened up through attrition, the subsequent alveolar suppuration destroyed a portion of the external alveolar plate. Pressure on the offending tooth, to relieve pain, would be effected by the mandible being thrust outwards on the affected side, and then drawn forcibly upwards and inwards. Thus, with an already weakened outer alveolar plate, the crown is gradually tilted lingually. The buccal cusps would be worn down first, and the condition having been once established, it would tend to get rapidly worse.

Looking at the lower molars and their sharp edges, one can but speculate as to the effect they probably produced on the tongue.

A Mandible from a Bronze Age Barrow at Portland, Dorset.

Shown by W. DE C. PRIDEAUX, L.D.S.

THE mandible has a bony growth extending below the left lower border, with a thinning of the ramus from it to the angle of the jaw; premolars are present and second molar. A radiograph taken by Mr. Charles A. Clark shows the absence of third molar.



Sir Frank Colyer remarked on the contrast shown in size and density between the anterior border and the portion behind the growth; long-standing injury being probably the cause of the growth and wastage.

Sir Arthur Keith remarks: "The exostosis is at the anterior border of the left masseter, and is probably the result of a blow or tear of the muscle fibres caused by the blow. It is the result of an accident long before death."

Section of Odontology.

President—Mr. MONTAGU F. HOPSON, L.D.S.E.

A Note on the Treatment of Hæmorrhage after Tooth Extraction.

By F. ST. J. STEADMAN, D.P.H., L.R.C.P.Lond., M.R.C.S.,
L.D.S.Eng.

IN the somewhat extensive literature on the treatment of hæmorrhage following tooth extraction very little, if any, mention is made of the drug which I have found to be the most efficacious and reliable, namely, oil of turpentine. The following four recent cases in practice are interesting as demonstrating the great value of turpentine in controlling troublesome hæmorrhage.

Case I.—Woman, aged 40, stated that she “bled for hours after the slightest cut” and had “nearly died after the extraction of her four upper incisors.” Her doctor had been up with her for several hours during the night upon this and upon another occasion after tooth extraction. Her father was also a bleeder and had died of hæmorrhage. She had had her “blood tested” and had been informed that she was a hæmophiliac. Her doctor supported these statements. She had a very septic mouth; all the upper roots were present (with the exception of $\begin{array}{c} 2 & 1 & 1 & 2 \\ \hline & & & \end{array}$) but cut off with a plate over them. Both she and her dentist had been afraid of the consequence of removing these stumps. She had advanced periodontal trouble round all the lower teeth present, namely: $\begin{array}{c} | \\ \hline 8 & 5 & 4 & 3 & 2 & 1 & | & 1 & 2 & 3 & 4 & 5 \end{array}$. Her health had suffered from septic absorption and she was constantly suffering from toothache. It was decided to attempt the extraction one by one of all the teeth and stumps, with the exception of the two lower first premolars, at intervals of about a fortnight. She was put on a course of calcium lactate, 10 gr. three times a day, for three days before, and two days after, each extraction. The extractions were performed under local anaesthesia, an isotonic solution of 1 gr. of novocaine and $\frac{1}{4}$ gr. of adrenalin being injected at each operation. Great care was taken to extract the various stumps as neatly as possible. It was decided not to plug the sockets until it was proved to be necessary, the prevention of hæmorrhage being trusted to the calcium lactate and adrenalin. The whole of the upper stumps were extracted, also the stumps of the lower third molar and second premolar, two or three at a time; and all went well (that is to say, none of the sockets required plugging) until the right lower canine was removed. Upon this occasion I was sent for at 1 a.m. and found the patient sitting up in bed bleeding profusely. The socket was washed out with cold water and thoroughly emptied of blood clot and packed with gauze soaked in turpentine. The hæmorrhage ceased almost immediately, and the plug was removed three days later. As the patient had

lost a fair quantity of blood, further extractions were postponed for two months, when two lower incisors were removed, and the remaining teeth at intervals, with the same precautions as before. Neither of these sockets required plugging.

Case II.—Woman, aged about 30, had suffered from prolonged bleeding from the “tiniest cut” all her life. Her mother and her four brothers and three sisters all suffered from this tendency to bleeding. It is interesting to note that her mother had most of her teeth cut off and a plate put over them, as her dentist dare not remove them in consequence of the difficulty he has experienced in controlling the hæmorrhage after extraction in her case. Her father appears to be normal. One brother is stated to have died of hæmorrhage during the War. Since, however, he had been wounded in the leg and died while a prisoner in the hands of the Germans, it is impossible to state whether the cause of death was due to deficient blood coagulation or to the wound he had received. Her youngest sister suffers considerably from epistaxis, which lasts for days and is always difficult to stop. The elder of her two boys, aged respectively 3 and 1 years, has this tendency to bleed also. This patient has had a very septic mouth with several broken-down carious teeth which had not been removed on account of the great difficulty which had been experienced in controlling the hæmorrhage after previous extractions. She had been advised by her doctor to get rid of these teeth if possible as in his opinion her general health was being affected by them. She had postponed treatment until the left lower third molar began to ache severely. She was put on a course of calcium lactate, 5 gr. every four hours for three days, and then this tooth, together with the second left lower molar, were extracted carefully, under an inferior dental regional anæsthesia and the sockets immediately packed fairly firmly with gauze soaked in turpentine. Bleeding ceased within two minutes of inserting the plug. The plug was removed after twenty-four hours and the socket gently syringed with cold water. She was left on the calcium lactate for another day. There was no recurrence of the bleeding. A fortnight later the right upper premolar and right central and lateral incisors were removed, after the same three-day course of calcium lactate, under a local anæsthesia (1 gr. novocaine and $\frac{1}{400}$ gr. of adrenalin). These sockets were not plugged as there was no excessive hæmorrhage following these extractions. Two days later, during which time the calcium lactate was continued, the right lower third molar, second premolar and canine were removed under an inferior dental regional anæsthesia. She returned a few hours later with profuse bleeding from the molar and premolar sockets. These were packed with turpentine gauze and the bleeding ceased within a minute of the plugging. The plugs were removed twenty-four hours later. Bleeding did not recur.

Case III.—Woman, aged about 36, who had had prolonged hæmorrhage after all previous extractions. She has two children living, a girl of eighteen who has shown some tendency to abnormal bleeding, and a girl of eleven who appears to be normal. A son died at the age of four from uncontrollable hæmorrhage after a circumcision. This child had already nearly lost his life two years previously from bleeding after a fall in which he tore the frænum of his upper lip. She does not know whether her father was a bleeder as he died when she was quite young. Her mother undoubtedly had this tendency to bleed. It is interesting to note that her mother strenuously opposed the operation upon the boy but would give no reason. Indeed she successfully prevented the operation taking place during her lifetime. Looking back, the patient feels convinced that her mother knew of this tendency to bleed in the family history but did not disclose the fact to her. She has brothers, but has not seen them for a long time, and can give no information about them. This patient was put on calcium lactate, 5 gr., three times a day, by her doctor, for three days before and two days after extraction of the left upper canine under a local submucous anæsthesia (1 gr. novocaine, $\frac{1}{400}$ gr. of adrenalin). No abnormal bleeding followed. Four months later the left upper second molar was removed (after a two days' course of calcium lactate) under the same dose of local anæsthetic. The socket was not packed but she returned a few hours later with profuse bleeding. The socket was then packed with turpentine-soaked gauze, and bleeding ceased immediately. The plug was removed twenty-four hours

later, but the bleeding recommenced as profusely as before, so that a fresh packing had to be inserted. This was removed by the patient herself on the following day as it was paining her. Slight oozing recommenced three days later when the socket was again lightly packed.

Case IV.—A woman aged about 30. Referred by her doctor on account of severe pain in the right lower third molar. She stated that the septic condition of her mouth was due to the fact that no one had dared to extract a tooth since her experience of ten years before when she had "nearly died" after an extraction. On this occasion a Harley Street surgeon had been sent for in the early hours of the morning and he had had great difficulty in arresting the hæmorrhage. She had had severe bleeding after some extractions previous to this occasion. She was in great pain when seen at twelve o'clock noon and it was therefore decided after consultation with her doctor to remove the tooth that day. She was given 5 gr. of calcium lactate at 1 p.m. and another 5 gr. at 3 p.m. The lower third molar, together with the second and the stump of the second premolar, were removed under an inferior dental regional anæsthesia at 4.30 p.m. The sockets were packed with turpentine-soaked gauze and the plug stitched firmly into place. She was kept on calcium lactate (5 gr. every four hours) for forty-eight hours longer, when the plug was removed and the socket gently syringed. The wound healed well. There was no bleeding from the moment the plug was inserted.

While it is not claimed that these four women were true cases of hæmophilia, which is said to be rare in women, I think it will be admitted that they showed an abnormal tendency to bleeding; that the test of the efficiency of turpentine as a local hæmostatic was severe and that the drug stood the test most satisfactorily. Indeed had I not known of its great value and reliability as a hæmostatic I should not have risked operating in these four cases.

I have been using oil of turpentine as a local hæmostatic for the past twelve years. During this time I have had about fifty cases in which sockets have required packing, and in the majority of them calcium lactate was not given as the bleeding was usually unexpected. In every instance the bleeding ceased within a minute or two of the packing.

Apart from its hæmostatic action, oil of turpentine is a powerful anti-septic, so that when the gauze is removed two or three days after its insertion, it still has a clean faint smell of turpentine and the wounds are very much cleaner than they usually are when they have been plugged. I would emphasize this fact that the sockets are sweet and generally free from sepsis when the plugs are removed. The wounds heal very well indeed. I usually treat them for a few days by syringing with a weak solution of carbolic acid (1 in 80) in those cases in which I have been forced to pack a socket.

I regret having to admit that my use of oil of turpentine in this way is purely empirical. I do not know how it acts except that it lowers blood-pressure, but this obviously cannot be the full explanation. Oil of turpentine when given internally in the form of an emulsion to arrest hæmorrhage from lung, stomach, bowels or uterus, has been known to succeed when all other drugs have failed. It is possible that a course of this drug, given internally before tooth extraction in cases such as I have described, may prove to be even more efficacious than calcium lactate; or possibly both these drugs might be used with advantage and thus save the necessity of packing the sockets. But it must be remembered that oil of turpentine given internally may produce irritation and congestion of the urinary organs, lumbar pains, distressing and ineffectual attempts at micturition, and even hæmaturia.

Case of Multiple Dentigerous Cysts in the Mandible, and some Remarks on the Pathology of such Cysts.

By EVELYN SPRAWSON, M.C., L.R.C.P., M.R.C.S., L.D.S.

A. O., male, aged 18, somewhat mentally defective, presented himself at the London Hospital Dental School in August, 1920, for treatment of pain and swelling in the region of the left mandibular canine and the submental region. On examination, much thickening of the mandible was found, extending from the left first permanent molar to the right second permanent molar. Three teeth were unerupted, namely, the right second premolar and both canines, and the tip of the left second premolar was just erupting. The right first permanent molar was displaced upwards, so that on closing the jaws it nearly bit on to the opposing gum, and on the left the first premolar was displaced so that it leaned forward at an angle of 45° . The incisor teeth were in place and nearly, though not quite, in correct alignment. There were three sinuses from which pus issued, situate almost directly over the three unerupted teeth, though slightly towards the buccal aspect. There was much pain in the region of the left canine, and the external skin was reddened and œdematous, as if an abscess was about to point there.

Skiagrams taken of either side showed the three unerupted teeth in the substance of the mandible and the left second premolar mainly within it; also the outlines of cystic cavities adjacent to or partly surrounding them. The cyst outline on the right side extended under the first permanent molar to just beyond the second molar. The cysts were apparently unconnected with each other, though this was doubtful. A diagnosis of suppurating multiple dentigerous cysts involving four teeth was made.

It was vacation time, but as local treatment in the region of the left canine was urgent, he was anaesthetized with C.E., and the bone overlying the buried left canine and that tooth removed; the cavity scraped and packed, and the projecting bony edges trimmed. The cavity was dressed daily and the pain and inflammatory œdema rapidly cleared up. About a fortnight later it was found that it was possible to pass a slender silver wire probe into the cystic cavity of the left canine and right across the middle into the cyst cavity involving the right canine. The incisor region was therefore skiographed separately, and a large cystic space immediately under the lower incisor teeth was found; there were no teeth in it.

The patient, acting on advice, then came for further treatment, somewhat unwillingly, as the symptoms for which he came to be treated in the left canine region had, of course, disappeared. On September 23, 1920, he was again anaesthetized, and all teeth on the right side from the canine to the second molar, both inclusive, were removed, their extraction being rendered considerably more difficult than normally owing to the thickened mass of the bone. The intervening bony septa were broken down by means of curved elevators and a sharp spoon; the cavities of the cysts involving the premolar and canine were formed into one large cavity and scraped, and the wound firmly packed with sterile ribbon gauze, as there was a good deal of hæmorrhage. Among the clot, cyst lining, spicules of bone, &c., removed by the sharp spoon, there was a small complete cyst about 1 cm. in diameter; hardly any pus was encountered, and that only in the region of the second premolar.

The left side was then dealt with through the opening already made in connexion with the canine cyst on that side, the two premolars being first removed. The cavity connected with the second premolar on this side did not seem to be infected, and a globular mass of cyst lining about 2 cm. in diameter was removed almost unbroken. The cavity under the incisors was then explored and similar lining material scraped away, this cavity being freely united with that involving the left canine—which had already been united to that involving the left second premolar.

It would have been desirable to remove the front wall of the cavity under the incisors, but a good deal of the outer wall of the mandible had already been removed, and as this portion of bone was thick, it was considered that without special instruments there would be some risk of fracturing the mandible; and besides that there had been a good deal of hæmorrhage.

All parts removed were immediately placed in saline formalin.

The healing was uneventful and remarkably rapid; packing was discontinued after the third day; a vulcanite denture covering over the cavities was worn, and free irrigation applied.

A few weeks later the patient left the neighbourhood, and so we have lost touch with him; if he had remained, we had hoped at a later date to delete the channel under the incisors by removing its labial wall; this channel was quite large enough to admit an ordinary lead pencil.

Examination of the material removed showed that: (1) of two teeth examined, both had a normal Nasmyth's membrane (specimens shown); (2) that the small cysts and parts of the lining of the larger ones had a lining of a few layers of a well-differentiated epithelium, which was merely what might be expected (specimens shown).

Remarks.—Multiple cysts of this nature are not common, and in man they are not often found to be infected—possibly because people usually come for treatment before that stage is reached. Sir John Bland-Sutton states that suppuration is commoner in the lower animals, which, in the absence of treatment, is what one would expect to happen, as the cyst grows larger and so becomes more vulnerable.

Nasmyth's Membrane.—It is stated in some text-books that Nasmyth's membrane is absent from the teeth found in these cysts, and in others that it is sometimes absent. I have not had the opportunity of examining many of these teeth, but in those examined from four other single cysts the membrane was present in each case. The former statement cannot, therefore, be correct. At the time when that statement was first made, it was believed that the cellular layer of Nasmyth's membrane was derived solely from the external epithelium of the enamel organ—that these cysts were produced by an aberration of growth and subsequent degeneration of some underlying part of the enamel organ and that the epithelium lining the cysts was derived solely from the external epithelium, which became distended by this means. Now we know that the enamel in nearly all—all as far as I know—of these teeth appears well formed, of normal thickness and to the naked eye perfect. It would seem, therefore, almost inconceivable that in so small a structure as an enamel organ, any part underlying the external epithelium should be able to grow so aberrantly without interfering in some way with the normal calcification of the enamel, particularly as the part which used to be thought to be the origin of the cyst—the stellate reticulum—is now believed to play an important part in this calcification. Besides this, we now believe the cellular layer of Nasmyth's membrane to be made up of more than the external epithelium of the enamel organ.

It would seem, then, that on these counts one may reasonably conclude that these cysts can but seldom—if ever—originate from aberrant growth of any cells of the actual enamel organ; besides, there is some evidence that aberrant growth of the actual enamel organ gives rise to a different form of tumour altogether. There are other reasons to be mentioned later which I think tend to confirm this view.

I suggest that these cysts are all primarily dental cysts, usually formed in connexion with septic deciduous teeth, and in the same manner as that in which we believe the ordinary dental cyst connected with a septic permanent tooth to be formed—except that the epithelial cells from which the cyst arises may originate from three possible sources; that in their growth and extension in the direction of least resistance they meet with and surround an adjacent developing unerupted tooth, gradually enveloping it—at first to the extent of the enamel-covered crown only, but later in its whole extent by causing absorption of the surrounding bone; in this way its eruption is prevented. In other words: a dentigerous cyst is a dental cyst enveloping an unerupted tooth, and is usually caused by sepsis from an adjoining erupted tooth, and occasionally by trauma.

There are several factors which appear to support this view:—

(1) *Position*.—It is well recognized that the teeth most commonly involved in man are canines and premolars; the deciduous predecessors of which are those generally found in that dentition with septic pulp chambers, and are retained longest in the mouth under normal conditions—and often considerably longer when their pulps are dead and the normal absorption of their roots interfered with. Conversely, teeth such as the lower incisors, the deciduous predecessors of which are comparatively rarely found with dead pulps, are but rarely involved in man, and in the one case recorded in which they were involved they were apparently only involved by extension.

(2) *Age*.—The age incidence, so far as it is known, also supports this view, though of course it varies much in the case of either variety of cyst. The dental cyst is stated to occur from the ninth to the fifty-sixth year of life, but usually between the twentieth and fortieth years. So far it has only been found in human beings, and always after the eruption of permanent teeth. The first manifestations of the dentigerous cyst are said to occur from the second to the thirtieth year, and mostly about the fifteenth year.

(3) *Granulomata*.—Upon the extraction of a permanent tooth with an infected pulp canal, it is by no means uncommon to find a growth attached to the apical region known for want of a better name as a granuloma or root tumour. Some regard these in all cases as incipient dental cysts: and such without doubt they very frequently are. Now these root tumours are rarely found attached to deciduous teeth, but that does not mean that they do not often occur. They probably, indeed almost certainly, do occur frequently: in fact, as far as we know, the pathological sequelæ of pulp-infection in deciduous and permanent teeth are identical, and it would be strange if this were not so. Let us suppose that these root tumours *are* formed on deciduous teeth—what happens to them? I assume that the process of absorption of the deciduous tooth to make room for its successor, delayed though it is when sepsis is present, separates the tooth from the root tumour formed in connexion with it, so that when the deciduous tooth is in course of time shed or extracted, the root tumour—a potential cyst—remains behind, and may by its growth eventually envelop the underlying permanent tooth as already described.

(4) *The cyst may be quite separate from the underlying tooth involved.*

indeed two cysts were so in the case I have already described; the same has also been recorded by Mr. Dolamore, in fact one of his cases is still further confirmatory, for the cyst membrane had again to be divided after the cyst was opened before the tooth could be removed. This would represent the commencement of the envelopment process which I have mentioned; later on, that portion of the cyst wall adjacent to the tooth—being cut off by distance from its plasmic source of nourishment—would have degenerated and disappeared, and the tooth would then, as usual, have projected into the cavity of the cyst. The cyst in its very small stages must always be outside the unerupted tooth, but it is not very often found at this period of development.

(5) *The perfection of structure of the tooth in the cyst tends to show that nothing can have interfered with its early development; cementum, dentine, enamel and Nasmyth's membrane not being different in structure from those*

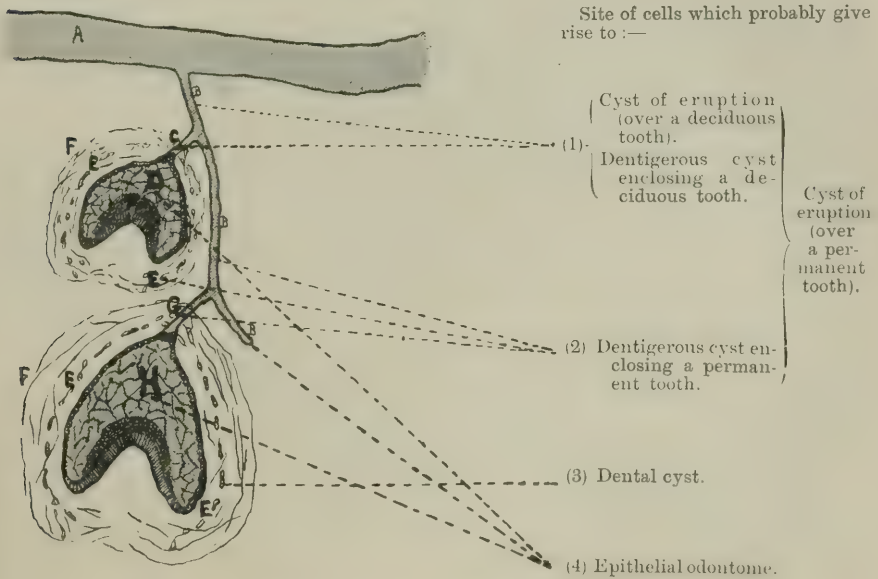


FIG 1.—Diagram of named parts of epithelial dental formative organs. This diagram is not true of any age. A, surface epithelium; B, tooth band; C, remains of neck of deciduous enamel organ; D, deciduous enamel organ; E, sheath of Hertwig; F, tooth follicle or sac; G, remains of neck of permanent enamel organ; H, permanent enamel organ.

of a normal tooth, at any rate in the cases I have examined. The incomplete calcification of the apical portion of the roots of some of these teeth is no doubt due to the envelopment of the tooth by the cyst preventing its completion. The natural inference is that the cyst arose from something altogether external to the tooth it encloses, and that the teeth only became involved secondarily.

(6) *Histologically* both dental and dentigerous cysts are identical as regards the cyst wall, its lining and the viscous fluid contents. Personally I have found the epithelium lining the dental cyst to be rather of a flattened squamous character, and that of the dentigerous cyst more often of a low columnar type.

I have not seen such difference remarked elsewhere, but it is not constant; even if it were it is of small account, for from whichever cells either form of cyst originates (*see* diagram, fig. 1, p. 59), whether from the sheath of Hertwig of the deciduous tooth, from remnants of the tooth band or neck of the permanent enamel organ, or from the sheath of Hertwig of the permanent tooth, we know that metaplastic changes can alter an epithelium profoundly. Pathologists tell us that the ciliated epithelium lining the trachea and bronchi may, in chronic bronchitics, lose its cilia and become of the squamous type, and even show signs of keratinization. Another point also is that the cells from which the dentigerous cyst originates would be younger cells, derived from the sheath of Hertwig of the deciduous tooth or possibly from the epithelial remains in the coronary part of the follicle of the permanent tooth, and therefore more active and likely to retain the Malpighian columnar cell characteristics, particularly as active growth is taking place at that part and it has therefore an increased vascularity. Whereas those cells giving rise to the dental cyst, the "rests" of Malassez, would be older cells and presumably somewhat degenerated, and so more likely to show the flattened type. The size and amount of distension of the cyst may also be a factor governing the type of cell found lining the cyst.

(7) *The tendency of the permanent tooth involved to go through the normal process of eruption* also seems to bear out the idea that the cyst does not in any way originate with it. One of the teeth in the case here recorded—the left second premolar—was in process of eruption; the cyst in connexion with this tooth was a small one, situate quite external to the tooth. There would seem to be a race between the growing cyst, to envelop the permanent tooth, and the tooth to escape its obstruction and erupt; but it appears that the growing cyst usually wins.

(8) *Dental cysts are not described as being formed in connexion with deciduous teeth*; my suggestion is that they *are* so formed, but that they become separated from them by the normal process of absorption of the tooth, and remain to grow after the deciduous tooth is lost. Eventually, by enveloping adjacent developing unerupted teeth, they become typical dentigerous cysts.

(9) *The finding of a deciduous tooth in a dentigerous cyst* is apparently extremely rare, and not altogether well authenticated. Four such cases are referred to in the "Report on Odontomes" but none are quoted, and a fifth case is quoted where a "deciduous or supplementary" tooth was involved. At this point it is well to remember that other causes besides sepsis can stimulate epithelial cells to grow, and that trauma has in a few cases been thought to be the exciting agent. Thus trauma, by stimulating the epithelial remains in the coronary part of the follicle of a deciduous tooth—which incidentally are a probable source of the "cyst of eruption"—may quite conceivably cause an epithelial lined cyst to form which might envelop an underlying deciduous tooth.

In the ordinary way sepsis from one deciduous tooth could not give rise to a cyst involving another deciduous tooth, because the eruption of the whole of the deciduous dentition is completed in too short a time to allow of the envelopment of any of them by the relatively slow growing cyst. It is interesting to note here that in pigs and sheep the lower incisor region seems to be a relatively common site for the formation of dentigerous cysts; one imagines that trauma is probably the exciting cause of their formation in this exposed and vulnerable part of these animals. In other words, sepsis in man and trauma in animals would appear to be generally the respective exciting

causes of growth, that is, if the incidence of these two growth-stimulating factors and the site of the cyst are to be taken into account. When these cysts involve the third molar, which is not rare, or the second molar, it is quite conceivable that either of these teeth, at an early stage of its development while still deeply placed and when nearer the front of the mouth, may become involved in a dental cyst arising from a first permanent molar. This is all the more likely as we are told that dental cysts are found most commonly in the molar region. Dentigerous cysts may however burrow considerably, as in the case I have recorded above, and it is quite conceivable that a septic deciduous tooth may be the starting point even in the case of the third molar, the germ of which at the earlier age would be some way beneath the cells which may ultimately be the origin of the cyst.

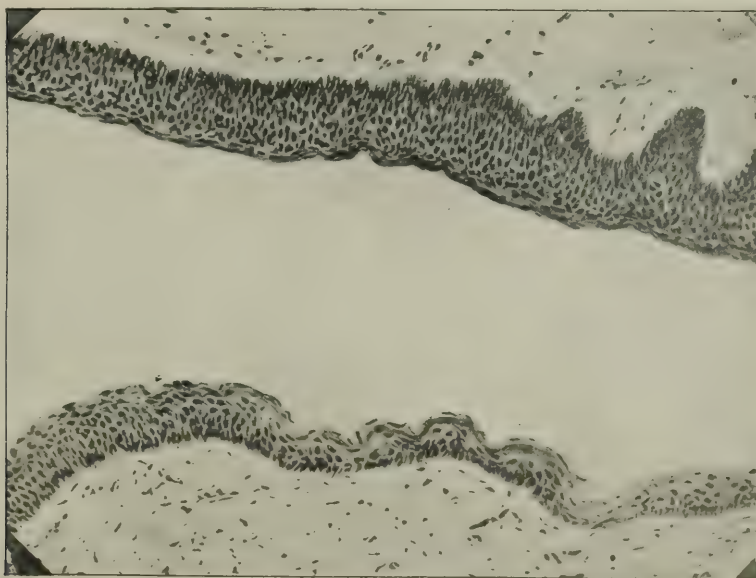


FIG. 2.—Paraffin section of cyst lining, from slide 2. $\times 165$.

(10) *Finally*, we know that it is by no means unusual for some permanent teeth to remain fully formed but buried in the substance of the jaws during the whole life of the individual, and yet no cystic growths are associated with these teeth. If it be remembered that the age incidence of the dentigerous cyst is given as from the second to the thirtieth year, I take it that this also lends weight to the idea that the stimulatory origin for these cysts must arise from something entirely external to the buried tooth.

I have only seen one case in which the septic deciduous tooth, underlying cyst, and permanent tooth, were all *in situ* at the same time; it occurred in a boy, aged $10\frac{1}{2}$; the deciduous left mandibular canine was dead and *in situ*, and the permanent canine—very deeply placed—was endeavouring to erupt low down in the labial sulcus, and was considerably misplaced. It was only upon removal of the deciduous tooth and owing to the escape of a small

amount of cystic fluid, that the cyst was discovered; it was a small one which had caused some absorption of the outer alveolar plate but did not bulge the overlying soft tissue; it lay in contact with the crown of the permanent canine but did not envelop it.

Among the specimens shown in connexion with this paper besides the skiagrams and diagram you have already seen are:—

Slide 1.—Showing paraffin sections of the small entire cyst removed from the right side of the case recorded; degenerating central epithelial cells are seen in this section. There is also a small portion of the lining of one of the larger cysts.

Slide 2.—Showing a paraffin section of the small entire cyst removed from the left side (*see fig. 2*).

Slides 3 and 4, showing preparations of Nasmyth's membrane raised from the two teeth in the cysts on the right side.

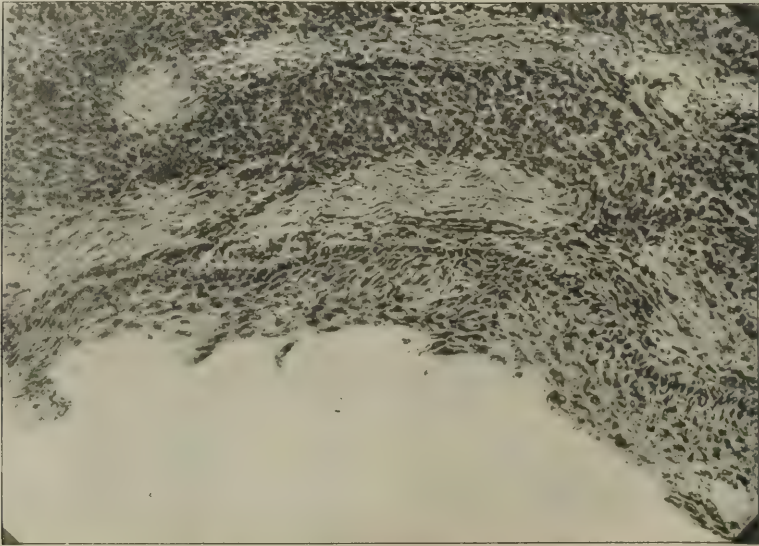


FIG. 3.—Frozen section showing epithelial lining with basal cell differentiation of granuloma on deciduous tooth, from slide 6. $\times 165$.

Slides 5, 6 and 7.—These I regard as having a rather important bearing on the suggestions put forward. They show sections cut from a dead left maxillary second deciduous molar, to which a small portion of a root tumour was attached on its removal.

On the mesial aspect of this fragment of root tumour, which can be seen to be mainly of inflammatory origin, is a continuous lining of epithelial cells some eight or nine layers deep and presenting interpapillary processes on its deep aspect, which can be traced upwards to alongside the cementum of the tooth, which in parts has undergone absorption; this thick layer of epithelial cells apparently lined the abscess cavity produced by the dead pulp, and the cells are differentiated, the layer forming the deep or external boundary being distinctly columnar (*see fig. 3*). There is also a small mass of epithelial cells in actual contact with the tooth at the point where its roots bifurcated.

This, I think there can be no doubt, is part of a potential or incipient dental cyst formed in connexion with a deciduous tooth, and almost certainly originating from the sheath of Hertwig of the deciduous tooth with which it is apparently continuous. One might equally well regard it as a potential dentigerous cyst—it is, I think, the same thing. I forwarded one of the sections (No. 5) to Professor Turnbull, Director of the Pathological Institute, London Hospital, asking him for a report upon it. His report, which is of great interest, is appended.

The remnants of the two small complete cysts and other cyst lining, &c., removed from the case recorded are also shown.

REPORT OF PROFESSOR H. M. TURNBULL, M.D., F.R.S. ON SECTION
No. 5.

The specimen is a frozen section, stained with hæmatoxylin, of part of a tooth including one root. Attached to the outer side of the inferior extremity of the root is a portion of soft tissue, the greater part of which hangs free beneath the root. The attachment is to a layer of cement. The lowermost part of this layer fills an area of lacunar resorption into the dentine. Immediately above this there is some lacunar resorption of the layer of cement. The lower and mesial surfaces of the root also show eroded lacunæ.

The soft tissue, where it is actually attached to the lateral aspect of the root, consists, in all save its lower extremity, of a dense fibrous tissue, in which there are some groups of free round cells. In its lower extremity it is greatly infiltrated by round cells, which appear to be plasma cells and lymphocytes. At this level there lies immediately against the eroded cement a layer of three to four rows of lozenge-shaped cells. The cells are usually separated by a narrow space, and these spaces are in places crossed by prickles. From this layer there pass outwards two branching and anastomosing trabeculæ of cells which differ only in that prickles cannot be detected.

This layer of cells is continued over the internal, or mesial surface of the free, dependent portion of soft tissue. Here it forms a layer some five cells deep, from which pass outwards long, stout interpapillary processes. Some of these processes are bounded by a single layer of regularly arranged, cubical, or short columnar, cells. Sharply defined intercellular spaces are usually present; prickles crossing these spaces can be detected in places. There can be no doubt that this is a layer of transitional, squamous epithelium.

In the angle between the surface of this epithelial layer and the inferior eroded extremity of the root, is a small mass of polymorphonuclear leucocytes. Numerous leucocytes also infiltrate the epithelium. The fibrous tissue external to the epithelium is infiltrated by a few polymorphonuclear leucocytes and by a very great number of plasma cells. The great majority of the leucocytes lies in the immediate neighbourhood of the epithelium.

The inferior surface of the body of the tooth at the mesial origin of the root shows much lacunar absorption. On this eroded surface is another cellular mass. This consists of degenerate, polymorphonuclear leucocytes (pus), and of swollen lozenge-shaped cells forming an interrupted layer with one stout process. In places, the latter cells lie against the eroded dentine, in others they are separated by pus. They do not show inter-cellular spaces or prickles. It is difficult, therefore, to be certain whether they are epithelial cells or large, swollen, osteoblasts.

REPORT ON SECTION NO. 5 (*continued*).*Summary and Conclusion.*

The inferior portion of the root and body of the tooth has been extensively resorbed. Attached to the outer surface of the inferior extremity of the root is a dependent mass of infiltrated tissue. This tissue is a prolongation of the periodontal membrane. The mass is bounded towards the centre of the tooth by a layer of transitional, squamous epithelium. This epithelium lies against the cement at the attachment of the mass. Pus lies in places upon its free surface, it is itself infiltrated by numerous leucocytes, but the subjacent, external fibrous layer is infiltrated by numerous plasma cells and by few leucocytes. The most intense inflammatory reaction occupies, therefore, the space towards the centre of the tooth internal to the free surface of the epithelium, and also implicates the epithelium itself. This favours the view that the free surface of the epithelium formed the lining to an abscess cavity situated beneath the tooth. This view obtains support from the presence of pus upon the eroded inferior surface of the body of the tooth. In this position, the abscess is bounded by a layer of cells which suggest epithelium but which may be osteoblasts.

Section of Odontology.

President—Mr. MONTAGU F. HOPSON, L.D.S.E.

The Experimental Production of Arthritis.

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(From the Thompson-Yates Laboratory, the University of Liverpool.)

IF, clinically, removal of areas of sepsis round the teeth will cure arthritis, and if recovery is assisted by the injection of vaccines prepared from micro-organisms recovered from those septic areas, then to prove completely the causation of arthritis by oral sepsis, these organisms should cause arthritis when inoculated into animals by oral routes. That is the proposition, the validity of which I set out to examine. No one has yet successfully defined those various changes taking place in and around joints which, for lack of a better word, I have called, collectively, arthritis. The nearest approach to a successful classification appears to be that of Rose and Carless [17], which I have abstracted as follows:—

(1) *Rheumatic Synovitis*.—Causation, pyococcal. (a) Occurring in the course of acute rheumatism, involving one joint after another, usually followed by resolution or by thickened ligaments and loss of mobility. If in one joint only, this may be disorganized without suppuration, being then acute rheumatic arthritis. (b) Occurring as a chronic disease characterized by swelling of joints, caused by effusion into them; thickened synovial membranes and thickened capsular and other ligaments. May end in fixation by adhesions; no lipping of cartilage or bone changes, usually accompanied by chorea, erythema, or Osler's nodes.

(2) *Osteo-arthritis* (having various synonyms such as chronic rheumatoid arthritis, arthritis deformans, &c.).—Cause, auto-intoxication. Pathological changes are thickened vascular synovial membranes, proliferation of villi; cartilages showing fibrillation, multiplication of their cells, softening and wear. Later there is hyperplasia of cartilage into which ossification takes place producing lipping, the osteophytes (which may cause locking) occurring in the terminal stage. The exposed bone is sclerosed and eburnated, the below surface being more cancellous than normal. The disease is divided into three groups: (a) Chronic mono-articular; (b) chronic polyarticular, in which the characteristic finger symptoms are seen; (c) acute polyarticular, commencing with febrile attacks, steadily progressing unfavourably, the small joints being affected symmetrically.

Osler [14] deals mainly with arthritis deformans, does not attempt a real classification, leaves the ætiology doubtful but probably of a chronic infective nature. He states that the changes are as above.

Adami [6] divides arthritis into the following groups: (1) Serous arthritis,

(2) sero-fibrinous arthritis, (3) suppurative arthritis, (4) osteo-arthritis; involving mainly the synovial membranes and ending in ankylosis. He states the causation to be increasingly severe infections, saying that acute rheumatism is of streptococcal origin.

Poynton and Paine [1] were the first to show experimentally the infectivity of acute rheumatism.

Beattie [2, 3], with Yates [4], &c., has published much work, showing plainly and in a straightforward fashion that streptococci cultured from cases of acute human rheumatism caused arthritis in rabbits. They also demonstrated the phenomena of relapsing experimental arthritis, and between 1906 and 1914 published reports of some hundreds of cases, which conclusively proved their thesis.

About 1909, Dr. William Hunter [5a] pointed out from his clinical experience, that low grade infections derived from septic foci in the mouth caused anæmia.

In 1914, Rosenow [8, 9] reported that he had produced experimentally, various lesions, among others, arthritis from streptococci of oral origin. In 1915 [11] he stated that streptococci from various diseases often had a most striking affinity for the organ or tissues from which they had been isolated, and in 1916 [12] published more work on elective localization, especially showing that streptococci isolated from an upper left molar had an elective affinity for teeth, dental nerves and muscles of animals.

In the *Lancet* of 1919, H. Warren Crowe [15] pointed out the importance of guarding against the fallacy that "because the vaccine of a given organism has a pronounced effect on a patient, that, therefore, this organism must be the causal germ of the disease," and quoted Sir Almroth Wright's suggestion that vaccines were probably not nearly so specific as had hitherto been thought.

Kerr Pringle [16], in 1920, quoted Nathan [16a], who laid down the very important axiom that once the micro-organism has entered the blood, its connexion with the portal of entry ceases; and that for this reason removal of the affected teeth (if they are really the site of the original focus which is not always certain) *will prevent re-infection or recurrences*; that such processes have absolutely no influence on the joint condition as it already exists, for the joint condition itself is a focal infection.

Willecox [19], in 1921, stated that in the large group of cases included under the term rheumatoid arthritis, arthritis deformans, &c., the cause is probably infection of *non-specific* origin.

Lastly, in the *Dental Cosmos* for last month (Feb., 1922), Nodine quotes a letter from Rosenow in which Rosenow states that he has produced heart and kidney lesions by infecting the teeth of dogs; no other details are given.

Dogs were chosen as suitable subjects for my work, for their habits and diet are very human, and their large canine teeth are readily accessible and of finished growth. Rodent incisors are obviously unsuitable; the pulp enlarges towards the apex; the tooth is of continuous growth and never of a decently workable size, although the rabbit has a very definite low resistance to streptococci generally. Owing to the expense it has not been possible to use as many dogs as one would like for this series of experiments recorded, as follows:—

The first dog, a small black and tan terrier, was anæsthetized. I have found that the best anæsthetic is chloroform until the animal is under, after this the anæsthetic is continued by intratracheal methods, using a Junker's bottle. I give one part of ether

to two parts of chloroform. The terrier had attached to its right upper canine a piece of Angle's thick retention wire. In order to get this wire into place it was found necessary to cut into the periodontal membrane below the cingulum, which is not well-marked in the canine tooth of a dog. In the pocket so formed, strain No. 12 (Warren Crowe's fully hæmolytic) was inoculated, the whole culture on one nasgar slope being used and emulsified in one drop of broth. It was found that the dog managed to remove the wire sometime between the third and fourth days, but as a quite definite pocket had been formed on the buccal alveolar surface between the bone and periodontal membrane where the wire-ends had been twisted together, the wire was not replaced. The whole culture, No. 12, on one nasgar slope was again inoculated with the aid of a barb broach into this pocket on the eighteenth and nineteenth days. The dog got quite used to my examination of its mouth, and did not mind the two little movements involved in inoculation. On the thirty-second day after the first inoculation the dog was in as good health, if not better than, when we received it. This was confirmed by Mr. Trevor Matthews of the Veterinary Department of the University of Liverpool, who one day examined all the dogs.

No. 1 Airedale was inoculated as follows: With the assistance of a dental student, Mr. Findlay, who worked a foot engine, under full anæsthesia the top of the crown of the left lower canine was removed by grinding, the pulp extirpated and a fine plain broach passed right through the apex, the operation being conducted with rigid asepsis. Into the pulp chamber, strains No. 7, 8 and 9 of the streptococci were inoculated in a concentrated emulsion, in plain broth. The pulp canal was closed with gutta-percha. On the eleventh day, the gutta-percha was removed, the pulp cavity being found quite dry. A plain broach armed with cotton-wool which had been inoculated with the same strains was passed right down to the apex. The cotton-wool was left there and packed in with a blunt broach; and the pulp chamber then sealed as before. Next day, the twelfth since the first operation, on the gutta-percha being removed, a small quantity of almost white pus escaped. An attempt was made to obtain cultures from this, but as the dog swept his tongue over the surface of the tooth immediately before the material was picked up we (Professor Glynn and myself) could not be sure that the material would be uncontaminated, although we attempted to sterilize the surface by dry heat. The culture proved to be grossly contaminated with *Bacillus proteus*; we could not be certain that streptococci were present. On the thirteenth day this dog developed slight lameness in both hip-joints (the night of the twelfth to thirteenth day had been a cold one); on the twenty-first day it again was slightly lame in both hindquarters. This proved on both occasions to be a transient condition and may have been due to the weather. As the lameness was fairly pronounced on the twenty-first day the left lower jaw was X-rayed. In spite of the movement of the dog during the short exposure (three seconds), I ventured to make a diagnosis from the film that there was certainly no bone destruction of a gross nature around the lower left canine.

No. 2 Airedale was operated upon in the same way as No. 1, but in this case the pulp was opened on the tenth day, when an abundant and free flow of whitish pus became apparent. Again the teeth were dressed with cotton-wool inoculated with streptococci; on the eleventh day there was only a small amount of pus, more sticky in consistence. On this day an attempt was made to cultivate from the pus and from the dressing remaining from the tenth day, but again all cultures were contaminated, probably in removing them through the last eighth of an inch or so of the pulp chamber, which could not be guaranteed sterile. This dog was noticed to be slightly lame on the twentieth day and was declared to be so by Mr. Matthews. No. 2 Airedale was X-rayed together with the first Airedale, and although a most restless dog a radiograph was obtained. It apparently shows a patch of rarefying osteitis behind and below the left canine; this together with a very free flow of pus leads me to think that a septic area if not actually an abscess has been established. It was largely a matter of opinion as to whether the dog was lame or not.

The fourth dog, was inoculated as above with a concentrated emulsion of *all the streptococci obtained*. The dog had been observed to cough once or twice before

the operation. Immediately after the operation I noticed it was coughing badly; the cough disappeared entirely about the fourteenth day. On the seventh day the dog was distinctly off colour, being lame in its near hind leg. This was confirmed by Mr. Matthews. It was reinoculated; no pus was observed. On the eighth day the dog was lame in both hind and its off fore leg, and was still coughing.

The dogs have not been killed as I want to give them every opportunity either of recovering or of becoming seriously ill.

It was perhaps unnecessary for me to confirm the fact that arthritis could be produced in rabbits by inoculation with streptococci of certain definite strains. However, I did this in order to be assured that repeated sub-cultivation had not lowered the virulence of the strains which I obtained. The following are details of the positive results:—

Rabbit No. 8 was inoculated with a full growth of Professor Beattie's *Micrococcus rheumaticus* (intraperitoneal route). Twenty-eight days later the rabbit had a definite and established arthritis; it moved slowly in its cage, objected either to the caretaker or myself handling it, and when placed on the floor used its fore-legs rather than its back-legs, just the reverse of what a normal rabbit does. It was therefore killed and a post-mortem performed, the results of which I will give later.

Rabbit No. 9, inoculated with Professor Beattie's *Micrococcus rheumaticus* C—a fatal human type. Several days afterwards I thought it had arthritis; on placing it on the floor it was noticed that it progressed vigorously but was not attempting to use its near hind leg which was kept in the air with a most peculiar jerking movement. This animal was therefore killed on the twenty-eighth day.

Rabbit No. 10 was inoculated with Professor Beattie's strain No. S7. This rabbit had been noticed to be becoming sluggish in its movements, and on the twenty-eighth day was very obviously ill and was killed with the other two. It had the most peculiar gait I have yet seen in a rabbit. It did not hop with its back-legs as a rabbit should do, but walked very stiffly and very slowly.

The other rabbits, into which were inoculated strains 5, 6, 7 and 11—15, have maintained fairly good health.

As I write this rabbit No. 11 is however apparently going under. (It had recovered by the fifty-fifth day.—A. L.) It is evident therefore that the strains I am using will cause arthritis. The post-mortem examination of rabbit No. 8 showed thick yellowish pus in the knee-joint of the near hind-leg; this was cultivated together with a scraping of the endothelium, and streptococci were recovered. The heart blood proved sterile; all other organs were normal. From rabbit No. 9 no streptococci could be recovered from the joints of the back-legs or heart blood, whether on account of faulty technique or because they were absent is unknown. From rabbit No. 10 streptococci were recovered from both knee-joints, but not from the heart blood, the cultures being, so far as one could see, identical with those inoculated. There was a suggestion of recent kidney infarct. I had neither time nor opportunity to identify by "sugar" reactions these streptococci recovered from the knees.

All the cultures obtained from Professor Beattie and Dr. Warren Crowe were inoculated into mice. On the third day the mice inoculated with strains 4, 5, 7 and 6, 8, had a distinct disinclination to use their fore-feet; this passed off and although massive doses of the streptococci 7 and 8 were then given to those respective mice no effect has been observed from them. Strain No. 12 caused lameness of all limbs in one mouse about the eighth day after inoculation. In all cases the effects were transitory. In the other mice there was apparently no departure from the normal—none of the streptococci were fatal to mice; and Miss L. Digby, of this Department, informs me that her experience of streptococci, which I know to be very extensive, leads her to the conclusion that only 25 per cent. of the partially hæmolytic streptococci are pathogenic to mice.

I give a brief description of the streptococci used:—

Strain 4: Hæmolytic on Warren Crowe's medium; slightly hæmolytic on laboratory blood nasgar; obtained from case of oral sepsis-arthritis.

Strain 5: As above.

Strain 6: As above, but non-hæmolytic.

Strain 7: Non-hæmolytic, from the throat of a case of acute rheumatism, Professor Beattie's number S135.

Strain 8: Non-hæmolytic, Professor Beattie's *Micrococcus rheumaticus* from a fatal case of rheumatic fever.

Strain 9: Non-hæmolytic, Professor Beattie's *Micrococcus rheumaticus* C from a fatal case of rheumatic fever.

Strain 10: Non-hæmolytic, Professor Beattie's No. S7 from a case of acute rheumatism with chorea and septic throat.

Strain 11: Non-hæmolytic, Professor Beattie's No. S1 from the hip-joint of a child.

Strain 12: From Warren Crowe: very hæmolytic on his medium, moderately hæmolytic on laboratory blood nasgar (of oral origin).

Strain 13: As above, but apparently true viridans type (of oral origin).

Strain 14: As above, but non-hæmolytic (of oral origin).

Strain 15: As above, but fully hæmolytic (from sputum).

It is necessary to examine the results of these experiments before arriving at a conclusion. I am informed that muscular rheumatism is very common in dogs, whilst arthritis, of the chronic rheumatic synovitis type, or osteo-arthritis proper, is rare. Thus given that everything was in favour of the development of the disease, such as lowered resistance and unsuitable environment, the dog would not react like a human being to equal infection under similar conditions, for it appears to have a higher natural immunity. This view is supported by the fact that the inoculated material was exploded, as it were, at the apex of the dog's teeth, and in human beings experience leads to the view that the methods adopted would have caused grave local disease. This experimental inoculation has not occurred in those human cases in which arthritis exists: the infection if by way of the pulp, is probably very much smaller in amount and certainly far less virulent. We know that the peri-apical tissues, given fair chances, put up a good defence, by means of fibrous tissue barriers; and that the micro-organisms reside mainly on the inner or tooth side of the abscess wall; are very scanty, if present at all on the outside of the abscess, and usually are completely absent from the pus if that is collected with due precautions so as to prevent disturbance of the micro-organisms.

Contrast with this my attempted introduction of very large amounts of powerful streptococci into the peri-apical spaces of the dog's teeth. There must have been an immediate reaction locally sufficient to wall off, digest, or otherwise dispose of the organisms, even if few in number, and the reaction was apparently permanent, and scarcely susceptible to forcible later attempts at breaking it down—unless artificial immunity was established—which I consider very doubtful. At all events I did not confirm immunity reactions of any sort by blood counts or agglutination, &c.

The clinical results were very small in my opinion, and I might add, still smaller in the opinion of the animal caretaker, although the veterinary specialist considered that *certainly* in the fourth dog there was an unusual "action" as he called it. The contrast was very marked when a comparison was made with the "action" of the rabbits, especially on the day on which the bad cases were killed, which was very cold. It was not necessary to study these animals for any length of time, the attempt at anything more than one step was

obviously painful to them, whilst it was necessary in the case of all the dogs to watch them walking, running, and at rest, before an opinion was ventured—except in that of No. 4, who was rather obviously “off colour” for two days.

I did not feel myself justified in attempting to do all the various things that really were necessary. To take temperatures, weigh the animals, renew the inoculations daily, obtain blood for cell-counts and blood-cultures, would have been rather too much to inflict upon them, even in the interests of science: this is only possible if one establishes a proper animal hospital with nurses and so on.

The experiments may be assessed in the following way:—

Animals.—Not very susceptible species used.

Organisms.—From human sources, definitely arthritic: causing arthritis in rabbits.

Dose.—Very large in pulp canal.

Number of Organisms at Peri-apex immediately after Operation.—Certainly a few.

Method of Infection.—The repetition of the inoculation produced a slow and continuous infection with exacerbations—as often occurs in human beings.

Clinical Results.—Very small.

Radiographic Results locally.—Doubtful.

Bacteriological Results.—Almost negative.

These results may be applied to human conditions in this sense, that undue stress has been laid on “focal infection” as a potential or actual causal agent of arthritis. For it seems rational to believe that this disease, which is not excessively common, may arise from focal areas if the resistance of the whole organism is very greatly lowered, or if the focal areas are excessively large and numerous. Further, it seems probable that an established infection may be avirulent, but that infection with a second strain may cause a general flare-up out of all proportion to the amount of circulating organisms. But in the large majority of cases, an acute infection of the human peri-apex, leading to a chronic abscess, does not terminate as an arthritis. But should one or two small areas of osteitis around the teeth or in the jaws be demonstrated radiographically, such as Stanley Colyer has recently described [22], then in cases of arthritis the physician or surgeon must not omit a thorough and complete bacteriological examination.

SUMMARY.

(1) Certain strains of “rheumatic” streptococci when inoculated intra-peritoneally produced arthritis in rabbits, but only slight arthritis in mice.

(2) These strains inoculated by oral routes into dogs produced only *very* slight arthritis.

(3) Too great stress is laid on the production of arthritis in human beings by “focal infection in the mouth.”

I wish to acknowledge the kind assistance of Professor Ernest Glynn, Messrs. W. Findlay and F. Willis, and the laboratory staff, and to thank Professor Beattie and Dr. E. Warren Crowe for providing me with the cultures used.

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Section of Odontology.

President—Mr. MONTAGU F. HOPSON, L.D.S.E.

On the Significance of the Extra Cusp commonly found on the Antero-internal Aspect of the Maxillary First Permanent Molar in Man.

By EVELYN SPRAWSON, M.C., L.R.C.P., M.R.C.S., L.D.S.

ON March 22, 1917, Professor Sabouraud of the St. Louis Hospital, Paris, published a paper in the *Presse Médicale* entitled "On a Dental Sign of Hereditary Syphilis." In this paper, he advanced the hypothesis that the extra cusp often found on the antero-internal aspect of the maxillary first permanent molar was diagnostic of congenital syphilis. In this country we have no special name for this cusp, but on the Continent it is apparently known by the name of "Carabelli's tubercle," so named after Dr. Georg Carabelli Edlen von Lunkaszprie, who was Professor of Dental Surgery to the Academy of Petrograd and dentist to the Court in the first half of the last century, and who described this cusp under the name of "tuberculus anomalus." Since the paper by Professor Sabouraud was published, his conclusions as to the significance of this cusp have been transcribed in various text-books, both British and foreign, especially dealing with dermatological and venereal diseases. On this account, some eighteen months ago, at the suggestion of Dr. Sequeira of the Dermatological Department at the London Hospital, I made a short investigation into this subject. The proposal was that several hundreds of *children* should be examined, and that in the case of all those presenting this extra cusp, a Wassermann blood test should be carried out; the examination of children was indicated, as it is often said that after the second decade of life the Wassermann reaction is frequently negative in its results in congenital syphilitics.

At that time a number of boys were available to me for examination. The results of the examination of the first hundred, as stated below, combined with some further information which came to hand, were, however, so conclusive that it was not considered worth while to proceed with further examinations or with the Wassermann reactions.

In its incidence this cusp was found to be one of the commonest of the many variations from the normal dentition which occur in man. In size it varied considerably, from a slight prominence to a very well defined cusp; it was not always symmetrical on the two sides in the same individual, it was sometimes well marked on one side and entirely absent on the other, and occasionally associated with extra cusps elsewhere in the mouth, especially in the corresponding mandibular molars.

The boys examined lived in a residential home and varied in age from 6 to 15; and the first 100 who had otherwise normal dentitions and no obvious signs—dental or extra-dental—of congenital syphilis or other physical defect, were tabulated in four groups as follows: (1) Those having no supplementary cusp; (2) those having the supplementary cusp slightly marked; (3) Those having the supplementary cusp well marked; (4) Those having the supplementary cusp exceptionally well marked.

In order to get my first 100 *apparently normal* boys it was found necessary to examine 116 boys: 16 were discarded for the following reasons: 8 had some form of dental hypoplasia; 5 had doubtful signs (1 dental and 4 extra-dental) of congenital syphilis; 1 had had the test teeth removed; 1 was mentally deficient; 1 had cleft palate and hare lip.

In the 100 boys tabulated the results were as follows: Cusp absent, 35; cusp slightly marked, 39; cusp well marked, 15; cusp exceptionally well marked, 11.

It was found that in the great majority of cases with the extra cusp on the first permanent molar it was also present on the second deciduous maxillary molar, where this tooth was surviving for comparison, and conversely. Five boys were found to have it well marked on one side but absent on the other, and several others had slight variations between the two sides. In two cases in which the cusp was "exceptionally well marked" there was also a sixth cusp on the mandibular first permanent molars, and I have noticed this corresponding mandibular variation several times since.

One other point which is of some interest is that of the five boys who were discarded on account of doubtful dental or extra-dental signs of congenital syphilis, and so do not come into the above tabulation, two of them had the cusp "well marked" and three "exceptionally well marked."

The cusp, then, was present in one of the three degrees indicated in 65 per cent. cases, and absent only in 35 per cent.

Congenital syphilis I am informed by the Dermatological and V.D. departments of the London Hospital is a relatively rare disease, and of course does not occur in anything like this frequency. Dr. L. M. Pautrier, Professor of the Clinic of Cutaneous and Syphilitic Maladies at Strasbourg, has written an article entitled "Tubercle de Carabelli," which was published in the *Annales des maladies vénériennes*, in September, 1921. In this he records the occurrence of this cusp in some twenty-seven cases of primary syphilis in which infection did not occur till the young adult age, in people apparently otherwise healthy, i.e., not congenital syphilitics. In the *British Journal for Children's Diseases* for April, 1918, Dr. Mantoux, who examined seven tuberculous soldiers presenting this extra cusp, found the Wassermann reaction negative in five; and he quotes Dr. Mozer and Dr. Cheret as finding the Wassermann reaction negative in nineteen children, in all of whom this extra cusp was present.

Sir Arthur Keith, of whom we made inquiry, has informed us that the occurrence of this extra cusp is not uncommon among the anthropoid apes.

It would seem therefore that this cusp has probably no connexion with congenital syphilis at all, but is most likely to be a visible stage in the present-day dental evolution of man; and as far as one can gather from comparison with older skulls both of existing civilized, and primitive races— from which, however, no exact figures have been worked out—it would seem that the occurrence of this cusp is becoming commoner. Its existence at the present day is a disadvantage rather than otherwise to the individual, as the fissure

between the cusp and the main mass of the tooth is not infrequently the site of dental caries.

It may here be remarked that among the Order Primates there are apparently more signs of active dental evolution to be seen at the present day than among any other group of mammals. Sir Frank Colyer has recently drawn attention to variations in Rhesus monkeys (*Macacus rhesus*); they are known to be more frequent among anthropoids, and still more so in man: though of course one must remember that man has been subject to a somewhat intensive dental inspection. It may also be noted that these are all Old World forms of the Order, and that it is in the Old World that man is believed to have evolved.

Amongst the lower animals, however, the patterns of teeth are so constant that a slight difference in size of a cusp may be a deciding factor—in the dried skull—in giving to an animal its correct name, as in the case of lions and tigers; and an extra cusp may even be taken to indicate that a new species is being examined—for instance the common short-tailed field vole (*Microtus agrestis*), except for the fact that its second maxillary molar has five cusps, is almost identical with the Continental short-tailed field vole (*Microtus arvalis*), which has but four cusps to this tooth, but is ranked by zoologists as a separate species. And I am informed by Mr. Oldfield Thomas, F.R.S., of the British Museum, that the difference between these two teeth is the usual and handiest means by which zoologists distinguish between the two species.

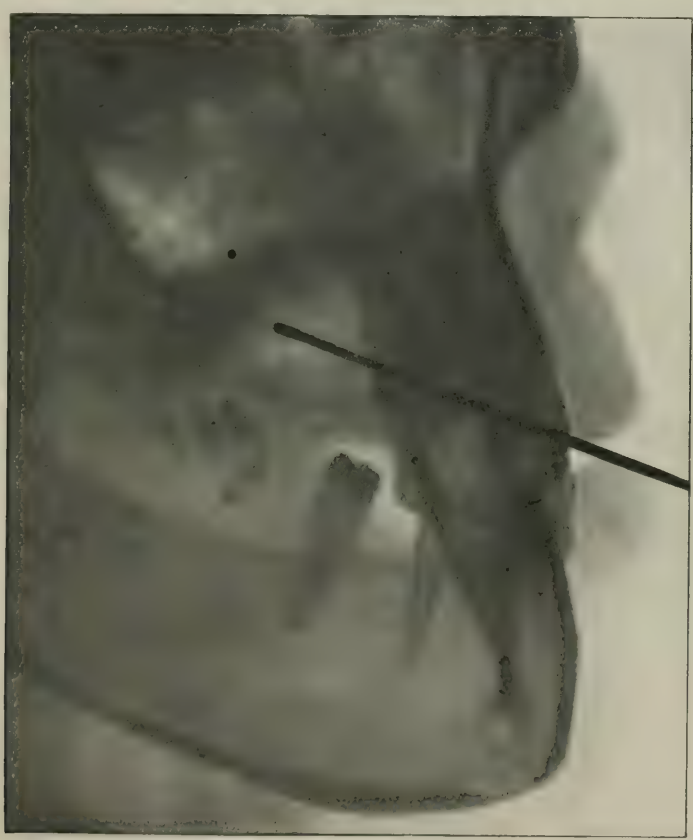
Section of Odontology.

President—Mr. MONTAGU F. HOPSON, L.D.S.E.

Suppurating Dental Cyst involving Floor of Nose and causing Necrosis of Palate.

By FRANK COLEMAN, M.C., M.R.C.S., L.D.S.Eng.

PATIENT, a woman, aged about 40, admitted to the dental department, St. Bartholomew's Hospital, early in January, 1922.



Showing cavity of cyst with probe *in situ*.

History.—Patient states that the mucous membrane of the palate had been tender and swollen for about six years.

O—OD 1 *

June 26, 1922.

Condition on Admission.—Swelling and thickening of mucous membrane of palate apparently connected with a carious right upper lateral tooth and simulating a chronic palatal abscess.

Treatment.—January, 1922, nitrous oxide administered. Right upper lateral tooth extracted (followed by a discharge of pus) and mucous membrane of palate incised. On exploration of the palate with a probe bare bone was detected and at one place an absence of the bony palate, the probe apparently passing into the nose.

A few days later an X-ray was taken with a probe *in situ* and this revealed a cavity (about the size of a walnut) projecting into the nasal fossa (see figure, p. 75). The remaining carious teeth were subsequently removed.

May 12: The cavity was opened into from the palate, explored, lightly scraped and plugged.

A few weeks later, to allow better drainage, the cavity was opened up a little more freely (under nitrous oxide) and a counter-opening made through the alveolus above the lateral incisor tooth-socket.

After-treatment.—Plugging discontinued after a few days and syringing after meals substituted.

Remarks.—This at first appeared to be a case of a chronic palatal abscess with necrosis arising from a carious upper lateral incisor tooth; no bulging of the bony walls of the palate or maxilla could be detected.

The diagnosis of dental cyst was only suggested after an X-ray had been taken with a probe *in situ*.

Types of Difficult Extraction and their Treatment.

By FRANK COLEMAN, M.C., M.R.C.S., L.D.S.Eng.

A SATISFACTORY definition of what constitutes a difficult extraction is not easy and the term has only a relative value, for as Mr. William Guy says: "Cases regarded as difficult will be in inverse ratio to the skill and experience of the operator."

Difficulties, however, from time to time present themselves even to the skilful and experienced. Experience enables one to foresee what is likely to be a difficult extraction, but apart from this every extraction should be undertaken on the assumption that it may be a difficult one until it has proved otherwise.

Some extractions only become difficult as the result of futile attempts at extraction, each attempt having the effect of nibbling away portions of the root and rendering more difficult the adaptation of an instrument to the remaining portion.

The difficulties of an extraction are due to anatomical and pathological conditions of the teeth and jaws, such as misplaced and mis-shapen teeth, divergent and exostosed roots, impaction of teeth, trismus of the jaw. None of these conditions is insuperable and when recognized can usually be dealt with by appropriate methods. The difficulty lies chiefly in the want of adaptability of the instruments at our disposal for accomplishing the task.

Ten years ago I read a paper at a British Dental Association¹ meeting on "Some Types of Difficult Extraction"; the types then explained still remain the types that give rise to difficulties.

¹ *Brit. Dent. Journ.*, 1912, xxxiii, p. 545.

The anatomical and pathological conditions of the teeth and jaws remain fairly constant factors, but our technique and methods of overcoming these difficulties might often be modified advantageously or altered to cope with such difficulties.

An extraction may be difficult at the outset or may have only become so after an unsuccessful operation. As prophylactic are better than remedial measures every precaution must be taken to avoid fracturing a tooth and should this unfortunately occur the difficulties will to a large extent depend upon the initial attempt at extraction.

A good attempt at an extraction, even if failing in its ultimate object of removing the tooth, plays an important part in the subsequent difficulty of the operation. For a good attempt usually means that the tooth has been slightly dislocated in its socket, the tooth socket has been slightly dilated and the operator has gained valuable information as to the cause of resistance in the tooth. This is a very different state of affairs from that resulting from a "snap" fracture which leaves the bone and root in firm contact. The snap fracture usually occurs within a few seconds of applying the forceps to the tooth—in fact frequently before even this part of the operation has been correctly carried out. The operator by means of this abortive attempt has gained no information as to the cause of resistance of the tooth, nor has he even dilated the tooth-socket, which might have been of great value in any subsequent attempt at extraction. There is some sound teaching and reasoning in a statement I once heard made by a person of great experience, namely, that "a tooth should never be fractured within ten seconds of the commencement of the operation": and it embraces far more than may be apparent at first sight. The time limit may, however, be arbitrary and subject to modification according to prevailing conditions.

I have always endeavoured to warn students against regarding the operation of extraction as a brief and rapid procedure.

An operator should be able to train his sense of touch to such an extent that the beaks of the forceps become for all practical purposes an extension of his own fingers, as it is by this means alone that he can respond in the most advantageous manner to the sense of resistance offered.

Another aphorism applicable to the operation of extraction can be stated as follows: "A grating sound is an indication of metal working upon metal and is a sure sign that the blades of the forceps are not closely fitting the root of the tooth."

Some of the difficulties of extraction that are not infrequently encountered are the following:

(I) MANDIBULAR PREMOLAR TEETH FRACTURED AT OR BELOW THE MIDDLE OF THE ROOT IN DENSE UNYIELDING BONE.

The ordinary forms of root forceps employed become locked in the socket before the blades reach the root, as the socket prevents the blades being opened sufficiently wide to enclose the root (fig. 1A).

The following methods are available:—

(a) The use of a specially designed instrument, the blades of which are long, slender and parallel and meet at their ends. This instrument allows the blade to pass deeply into the socket and can sometimes be opened sufficiently wide to grasp a root that has been previously separated (i.e., dislocated) in its socket.

(b) Opening the blades of forceps sufficiently wide to embrace the alveolus as well as the root of the tooth (fig. 1B). This method will usually succeed when the bone has been sufficiently damaged to allow the forceps to obtain a grasp on the fragment.

(c) Packing the socket firmly with gauze, so that the remains of the tooth can be detected on a subsequent visit when it may also have become slightly loosened. The objections to this method are that the wound is likely to become septic and painful, and that even if a view of the root be obtained, it may still be necessary to resort to one of the above methods for its removal.

(d) Removal of a small osteo-plastic flap over the outer surface of the root, similar to that for excision of the apex of a root. This method would, I believe, result in a clean surgical wound with a minimum of damage to the surrounding hard and soft tissues and allow of better drainage and consequently be associated with less post-operative pain.

Fortunately, in most cases when the root is surrounded by dense bone the tooth is in a healthy condition and is often best left alone.

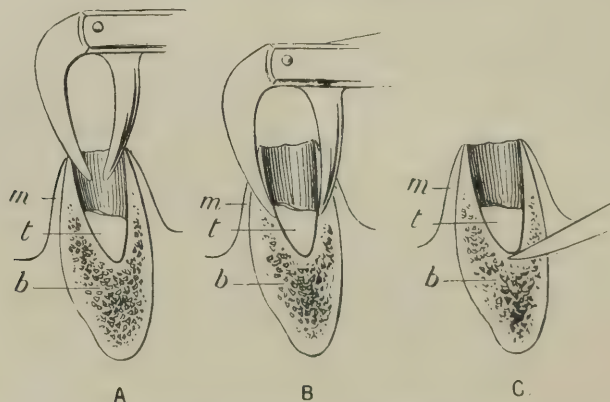


FIG. 1.—(A) Showing how a dense socket may prevent the admission of the blades of the forceps. (B) Showing the forceps including portions of the alveolus in their grasp in order to reach the root. (C) Showing the blade of a straight elevator passed through the outer alveolar wall below the apex of the root. *t*, Tooth; *m*, mucous membrane; *b*, bone.

The difficulties of this fracture do not arise, as a rule, when the bone is softened by disease and in these cases forceps will usually be serviceable, or a straight-elevator may be forced through the outer alveolus below the apex of root (guided by a sinus when present) and employed to force the root towards the orifice of the socket (fig. 1C).

(II) MANDIBULAR THIRD MOLAR TEETH, CARIOUS OR FRACTURED BELOW THE LEVEL OF THE ALVEOLUS IN A DENSE JAW.

The difficulties here are somewhat similar to those of fractured mandibular premolars with the additional difficulty of inaccessibility. The bony tooth socket offers such firm resistance that sufficient opening of the blades of the forceps to enclose the root is rendered impossible.

¹ For the loan of this and the following block the author is indebted to Messrs. H. K. Lewis and Co., Ltd.

The straight elevator is unavailable under these circumstances as the root is generally too low down in its socket to afford a grasp. A right-angled elevator, which I devised, has sometimes proved useful; the blade is wedge-shaped and set almost at a right angle to the shaft of the instrument. This instrument can be applied to the mesial or distal surface of the tooth, and forced as low down on the root as possible, and the root loosened by rotatory movements, i.e., supination and pronation of wrist. The effect of the instrument is rather to displace the tooth backwards or forwards, according to the lie of the blade, than directly to raise it from its socket; but the result of this is to dilate the socket and to render the subsequent extraction less difficult.

It is usually by means of a combination of instruments and methods and, thereby, detecting the line of least resistance, that the tooth is eventually loosened and removed.

If this condition could always be recognized beforehand, a set operation of removing a portion of the outer alveolus around the root would give better access, cause less damage to the jaw, leave a cleaner and better drained wound, and be more certain of success. The patient would need to be prepared as for a surgical operation.

(III) HOLLOW MAXILLARY MOLARS WITH BULBOUS CROWNS, AND STOUTLY IMPLANTED IN THE JAW (USUALLY FIRST MOLARS).

The difficulty encountered in the removal of these teeth lies chiefly in the want of a suitably designed instrument. Upper root forceps (Read's) often fail in these cases because their handles must be so far separated for the blades to enclose the remains of the tooth-crown, that control over the instrument in carrying out the extractive movements is greatly hindered. The blades of this instrument are also too slender for obtaining a firm grasp on the root of the tooth, and frequently too springy for maintaining a firm grasp throughout the extractive movements. Upper bayonet forceps allow of greater separation of the beaks, with correspondingly less divergence of the handles, but the increased distance from the tip of the beaks to the joint introduces the factors of loss of power and impairment of the sense of touch.

Some years ago I had an instrument made to overcome the wide separation of the handles that is necessary in using the present forms of root forceps on a broad root. The instrument was devised on a stouter plan than the usual forms of root forceps, and the relative gain in wider separation of the blades with less extensive excursion of the handles was brought about by two factors: (*a*) the beaks of the instrument were set less closely together and (*b*) the handles lay in contact, thus allowing greater separation of the blades with correspondingly less divergence of the handles. My original idea was to go further than this, and allow the opening of the blades to commence even before the handles were at zero (i.e., together), by having one blade fenestrated and capable of allowing the other solid blade to pass through it in its excursion. This, however, entailed difficulty and expense in manufacture, so I intended to try first the simpler modification.

(IV) HOLLOW MANDIBULAR MOLARS.

These teeth when firmly set in a dense mandible present difficulties similar to the corresponding teeth in the upper jaw. The neck of the tooth is insufficiently strong, or there is not enough of it, to permit the use of full

molar forceps and the blades of root forceps are not sufficiently capacious to enclose the bulbous crown of the tooth and simultaneously embrace its roots.

An instrument was devised many years ago to overcome this difficulty, I believe by Mr. Leonard Matheson: the blades were hollowed out to enclose the crown of the tooth, and the ends of the blades were left parallel or even slightly everted to obtain a grasp on the root of the tooth. The hollowing out of the blades served the purpose of preventing the bulbous crown of the tooth from being crushed, but the parallel or everted tips of the blades obtained but a poor grasp on the root of the tooth, either diverging from the root or securing only an edge contact at the neck of the tooth.

An instrument I designed for the same purpose is of heavier build than the ordinary root forceps; the beaks are hollowed out to avoid encroaching on the crown of the tooth and the tips of the beaks do not quite approximate. There is no need for the close approximation of the blades in this type of tooth as the difficulty here is not that of fracture of the tooth, but of obtaining a firm hold on strong molar roots in a dense jaw and avoiding a bulbous and perhaps frail crown.

I have found this instrument most serviceable for mandibular molars with hollow crowns and firmly implanted roots, where ordinary root forceps would encroach on the tooth crown and be prevented from grasping the root of the tooth, or even if they grasped the root would probably bend under the necessary strain put upon them.

This instrument is very useful also for artificially "crowned" lower teeth and for impacted third mandibular molar teeth.

(V) HOLLOW MAXILLARY INCISORS.

In order to remove a tooth from its socket, the socket must usually be dilated to a variable extent. This dilatation is generally accomplished by movements applied to the tooth, the root of which becomes in fact an extension of the forceps. The type of tooth under discussion is, however, too frail to be used as a dilating force, consequently some other method must be employed for dilating or opening up the tooth socket.

The fact that this type of tooth has given rise to difficulty is evidenced by the many devices employed for its removal, e.g., temporarily filling the root, or the use of an instrument known as the screw.

Temporarily filling a root produces only a false solidity, and a root sufficiently strong to withstand the internal strain of the screw instrument would with no greater risk withstand the external strain of root forceps if intelligently employed.

The following methods may be employed for this condition:—

(a) The bone around the root may be trephined or trephined by pressing the blades of fine root forceps up to the apex of the root, avoiding any pressure on the root by having the blades opened sufficiently wide to clear its margins. After a gutter or trench has been prepared around the root, the root may be lightly and cautiously grasped and allowed to fall out almost by its own weight (fig. 2A).

(b) If the root breaks up even under these precautions, the method of raising its apex, often the only solid part, by an elevator passed through the alveolus may be tried.

(VI) PALATINE ROOT OF MAXILLARY FIRST MOLARS.

This is usually the most difficult of the molar roots to localize when carious or fractured below the gum. An empty buccal socket forms a guide to its outer margin, but owing to its obliquity the inner margin of the root cannot easily be detected. Curved root forceps such as Read's are apt to slip off the palatine margin of the root towards the centre of the socket (fig. 2B).

A pair of upper narrow incisor forceps is often the best instrument for the removal of this root, the axis of the root is more easily obtained with this form of instrument and there is not the same tendency for the blades to slip towards the middle of the socket.

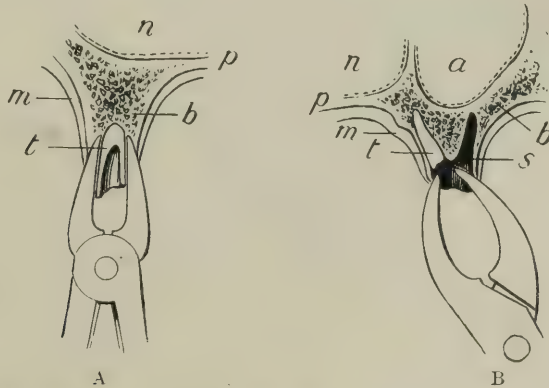


FIG. 2.—(A) Showing the forceps applied to a hollow maxillary incisor root. (B) Showing how curve-bladed root forceps may fail to grasp a palatine molar root, unless the palatine blade be opened to include the alveolus. *t*, Tooth; *m*, mucous membrane; *b*, bone; *p*, palate; *n*, nasal fossa; *a*, antrum; *s*, socket of a buccal root.

(VII) IMPACTED MANDIBULAR THIRD MOLARS.

These can usually be removed with forceps provided the covering bone is thin and there is some indication of the position of the tooth. The blades of the forceps are widely opened and driven down to the hilt in the opened position and only cautiously closed when they are felt to embrace the crown of the tooth. Slight lateral and rotatory movements are then applied, the distinctly backward curve that the roots of these teeth often possess being borne in mind.

Full molar forceps can be employed if the crown of the tooth is discernible through the gum, otherwise strong root forceps will obtain a deeper reach. It may be necessary to embed the entire beak of the instrument before a firm grip of the tooth can be obtained, and the forceps must be firmly retained in this position throughout the loosening of the tooth.

Most mandibular third molars that can be removed with an elevator can be removed equally well with forceps. If the tooth be rotated or otherwise lie in such a position that a grasp with forceps is unattainable, the overlying bone must be removed before the tooth can be raised.

When the tooth is deeply embedded in the jaw, it will be necessary to gouge away some of the overlying bone to expose a portion of its surface, it may then be dislodged by an elevator if forceps are unadaptable. This

procedure is, however, rarely necessary, as teeth deeply embedded in the jaw seldom give rise to discomfort.

The extraction of a sound mandibular second molar, as a preliminary to the removal of an impacted third molar in the quiescent stage, can rarely be necessary or justifiable; although it may undoubtedly simplify the operation. But its extraction for severe symptoms arising from an impacted third molar may sometimes be justifiable on the following grounds:—

(1) The removal of a badly impacted third molar is an operation requiring a certain amount of preparation of the patient beforehand and of arrangement as to the carrying out of the operation.

(2) The relief of pain and mitigation of sepsis afforded by the extraction of the second molar will conserve the patient's strength, permit the operation to be undertaken under more favourable conditions and assist in the subsequent drainage.

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SECTION OF OPHTHALMOLOGY.

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Section of Ophthalmology.

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THE following cases were shown:—

- (1) "Injury, Implantation Cyst (?)." By N. B. B. FLEMING, M.B.
 - (2) "Tumour of Ciliary Body (?)." By J. S. BOOKLESS, F.R.C.S. The case was discussed by Sir Wm. Lister, Mr. M. S. Mayou, and Mr. Treacher Collins.
 - (3) "Case of Benedikt's Syndrome." By W. G. WYLLIE, M.D.
- Six Reconstruction Models illustrating Stages in the Development of the Human Eye were shown by Miss IDA C. MANN.

Milestones on Refraction Work.

By ERNEST CLARKE, M.D., F.R.C.S.

(ABSTRACT.)

THIS is a review of the progress that has taken place in the theory and treatment of "refraction" during the last forty years. It coincides with my own experience as I was appointed Clinical Assistant at old "Moorfields" in May 1881.

That summer I spent at Donders' Clinic at Utrecht where I found this great man's influence had revolutionized refraction work. The metric system was in full use whereas in England many were still prescribing glasses with the old notation, the inch unit. Another great figure at this clinic was Snellen, and it was his demonstrating to me the means of detecting monocular blindness and malingerers by coloured glasses that led me, with Mr. Adams Frost, to devise the "Friend" test.

At this time refraction work was very primitive at "Moorfields," the test cases of glasses were very small, and the grading very large. Low errors of astigmatism were very rarely corrected. Two years later, on my appointment as Assistant Surgeon to the Central London Ophthalmic Hospital, I found attention was being paid to eyestrain, but it was then still called "asthenopia," and ocular headache was practically the only symptom recognized. Some years later I published my book on eyestrain and I tried to impress on the profession here, as Gould was doing in America, the extreme importance of its recognition.

We now know that there is scarcely any, if any, functional nerve trouble that may not be due to eyestrain, wholly or in part. We now know that it is the *small* errors that count, low degrees of astigmatism and anisometropia, and that by correcting them we stop a leakage of nervous energy. We also recognize now that eyestrain may be the "third partner" in any disease and that by removing it we improve the individual's fighting power against disease.

Glasses prescribed for eyestrain should be worn always, not necessarily at games out of doors, but always in the house. If this is done by young patients low errors tend to decrease and disappear, with the result that in time the wearing of the glasses may be discontinued. Small degrees of hypermetropia, if the same in both eyes and unaccompanied by strabismus, do no harm and need not be corrected unless associated with astigmatism.

Myopia.—The former practice of allowing myopes of a small amount to do near work without glasses has been proved to be bad and a cause, if not the chief cause, of progressive myopia. The full correction should be worn always, and only weaker glasses allowed for near work when the presbyopic period approaches. It is excessive convergence, or the excess of convergence over accommodation, that does the harm. Observations, over a period of fourteen years, of 750 myopes treated by me with full correction showed that only 270 increased in myopia and of these only 129 increased up to 0.75 D. or more and only sixteen of these increased over 2 D. These statements do not refer to high myopia with fundus changes. Very special care should be taken with the children of myopes, as heredity plays a very large part.

Presbyopia.—Great progress has taken place in its treatment; the static refraction and the accommodation power of every presbyope must be recorded and all errors corrected. If eyestrain is present bifocals combining distance and near glasses should be prescribed. The common fault is to give too strong a reading addition; 2.5 D. added to the static refraction is generally the strongest addition required for near work at any age, in bifocals. A stronger glass may be given separately as a reading glass only.

Cycloplegics.—The excessive use of atropine in the olden days is now changed in the practice of some by only using a cycloplegic very occasionally. From my experience I believe that cycloplegics are necessary in all under 40 or 45 because in many we can only thus arrive at the *static refraction*. Atropine should be used up to the age of 20, and afterwards homatropine. In some special cases homatropine may have to be used up to 50 or 53. Once a record is obtained of the static refraction the cycloplegic need not be repeated for some years, except in special cases.

IMPROVEMENT IN APPARATUS USED IN REFRACTION WORK.

The Ophthalmometer.—I have seen this instrument go through all its developments from Kagenaar's model which I brought from Holland, to the present perfect instrument in Meyrowitz's latest model. It only gives the corneal astigmatism, but I maintain that in most cases the lenticular astigmatism is dynamic and corrective, and therefore the knowledge of the corneal astigmatism is all that is wanted. A most reliable instrument and a great time saver.

Type.—Snellen's type is still the best, it should be at least 6 metres from the patient in a dark part of the room and evenly and artificially illuminated.

Trial Frames.—The only place for the old rigid trial frame is the lumber room. The frame should be light and adjustable in all directions, and the "cells" for the sphere and cylinder should be such that the glasses almost, if not actually, touch when placed in the frame.

(Reference was made to the great improvement of late years in the work of the opticians themselves in measuring and fitting the patient.)

As the refraction in some eyes is constantly altering it is the duty of every oculist to avoid making any adverse comment on the former prescription of a colleague.

DISCUSSION.

Mr. HARRISON BUTLER said that at one time he was fond of using mydriatics; now he seldom did so, and was getting, he thought, better results. He was converted to his present view by his late friend, Mr. Devereux Marshall, and other eminent men, who pointed out that he was wasting time and putting patients to inconvenience by using mydriatics. For two or three years he made careful observations with and without mydriatics, and he had arrived at a different conclusion from Mr. Clarke's. The statement of Mr. Clarke that the eyes were constantly altering was a very important one, and was worthy of careful investigation. Many years ago his own impression was that the eye was an optical instrument which, once set, in most cases remained the same throughout life. He was himself wearing glasses which he had when aged 13, and they were still right. For many years he omitted to tell patients to see him again, because to take a fee for such further visit savoured to him of charlatanism. He now wished he had made the request more frequently. For the last ten years he had had under his charge more than one large school clinic, and he had taken much trouble with the cases, for he had found that the more care one took with the retinoscopy, the less the subsequent trouble caused. After four or five years these children began to come back again, and instead of getting out the old card to see what was the refraction, he ascertained it afresh, then compared the result with that previously registered, and he had been astounded at the difference in the result obtained. There seemed to be only two or three alternatives to explain these discrepancies. One was that his work was very bad and irregular. The second was that there might be a want of evenness in the mydriatic used; in some, the mydriatics had been in too long, in others not long enough. Another possible reason was that perhaps one was doing retinoscopy sometimes on the macula, sometimes not on the macula. In many cases he was satisfied that the refraction of children was altering, in some to a considerable degree; the astigmatism might have altered a diopter, one way or another. Perhaps some of the younger men might take that subject up and ascertain whether the statement that eyesight was always changing was true. What Mr. Clarke said about criticizing one's colleagues should be borne in mind; he believed there were cases in which changes in refraction took place from time to time, and with a certain type of cornea it was possible that slight alterations in shape might be due to variations in intra-ocular tension.

Mr. ERNEST CLARKE (in reply to Mr. Harrison Butler) said his experience had been that in very many cases the refraction did alter from time to time, and also the axes of the cylinders. He had known 0.25 D. cylinder alter from horizontal to the vertical in two years. It would be interesting to try to find out the cause of these alterations. The occupation of the patient might be an important factor. He would like to reconvert Mr. Harrison Butler to the use of cycloplegics. He thought it very important after using a cycloplegic to see the patient in normal conditions before ordering the glasses, because unfortunately no one had yet discovered a cycloplegic that was not a mydriatic, and the alteration in the size of the pupil sometimes made a great difference in the glasses selected. This post-cycloplegic examination was particularly necessary in cases of high hypermetropia, as patients varied enormously in the amount of hypermetropia they would allow to be corrected. It was no use forcing a child to wear glasses which rendered the vision worse than when without glasses.

“The Fourth Cranial Nerve.” By J. HERBERT PARSONS, F.R.C.S., F.R.S.
Published in the *British Journal of Ophthalmology*, December, 1921,
pp. 529-543 (with illustrations and bibliography).

Section of Ophthalmology.

President — Dr. JAMES TAYLOR, C.B.E.

Tuberculosis of Conjunctiva of Right Eye.

By HUMPHREY NEAME, F.R.C.S.

PATIENT, a male, aged 19, now with one eye, went for a holiday to Southend in August, 1920. He cycled there and felt rather exhausted at the end of the journey. After he had returned to town, while stooping at work, he felt something give way in the left eye, and from that date the sight of that eye has been gradually failing, so that after six weeks he could see with it nothing more than a hand held closely in front of it. In March of this year (1921) a cough developed and he complained of watering of the right—the other—eye. The cough has persisted until the present day. Swellings in the neck were noted last September twelve months (1920). He attended the Metropolitan Hospital for a time, and last May he went to the Victoria Park Hospital where the diagnosis made was Hodgkin's disease. The Registrar of the latter hospital informed me that there were no definite signs of tubercle in the chest, and that no tubercle bacilli were found in the sputum. During the last year he has lost about a stone in weight, his weight now being 8 st. 5 lb.; during the last three months, however, his weight has remained stationary. He has not had night sweats.

Last June he was admitted to Moorfields, under Mr. Treacher Collins, with irido-cyclitis in the left eye, a hazy cornea, and an anterior chamber full of a whitish substance. The eye was excised, and a section of it is now exhibited. The anterior chamber is seen to be full of cellular and fibrous material; the iris and ciliary body are practically destroyed. Last month some of the conjunctiva of his right eye was excised, sections of which are also exhibited; they seem to show a fairly typical tubercle formation; there are two or three fairly definite tubercles, some with giant cells, and with round cells, epithelioid cells, and much fibrous tissue. Two small glands behind the ear were removed. One of these was crushed up and the material inoculated into a guinea-pig, but it caused no illness in the animal, except a local reaction. The conjunctiva of the excised eye also proved negative with regard to the presence of tubercle. But I think we can diagnose tubercle on the condition of the conjunctiva of the right eye alone. The gland shows marked fibrous tissue formation. It must have been a very chronic condition; the lymphatic tissue of the gland is almost entirely replaced by fibrous tissue.

Flattened Papular Condition of Conjunctiva of Right Upper Lid.

By HUMPHREY NEAME, F.R.C.S.

PATIENT, a male, aged 16, History: Patient says he noticed nothing wrong until the beginning of October, and then there was marked swelling of both upper and lower lids, some swelling in the pre-auricular region, and a

[November 11, 1921.]

tender enlarged gland there. The conjunctival secretion showed no eosinophils, and the not very satisfactory section shows only a zone of round-celled infiltration, not enough to account for the papular formation. The nature must remain unsettled until the pathological investigation has been made.

DISCUSSION.

Mr. M. S. MAYOU thought that before the case was relegated to the category of spring catarrh, eosinophils ought to be found in the discharge; he had never seen a case of spring catarrh which did not show eosinophils either in the discharge or in microscopic sections, or else an increase of these cells in the blood.

Mr. HARRISON BUTLER said he disagreed with Mr. Mayou, and thought that this case was likely to be spring catarrh. He saw many examples of this disease while he was in Jerusalem. Recently he saw a boy who came from Canada, with both eyes in a similar condition, but worse. He had published cases in the *Lancet*, with illustrations, showing enormous nodules on the conjunctiva and limbus. He felt so strongly that the present case was one of spring catarrh that if eosinophil cells were reported to be absent from the conjunctiva, he would have a further examination made. He had not seen or heard of a case in which eosinophils were not found. The blood should be examined for excess of eosinophil cells.

Cyst of Lower Retrotarsal Fold.

By HUMPHREY NEAME, F.R.C.S.

PATIENT, a female, aged 31, with fifteen years' history of translucent cyst left lower fornix, movable with lid. I have brought this case of cyst chiefly from the point of view of diagnosis. I think it is a cyst of Krause's gland, although those glands are not so common in the lower fornix as in the upper.

DISCUSSION.

Mr. TREACHER COLLINS thought cases of cyst of Krause's gland were very rare; he did not remember having seen a primary cyst of that gland before he inspected this case; those he had seen previously were secondary to cicatrization from trachoma, in which the orifice of the duct of one of these glands had been occluded by scar tissue. The proof of the real nature of the cyst would probably be shown when sections were cut of its lining membrane.

Dr. WALLACE HENRY said that last week he saw a girl who had an apparently similar condition six years ago, but she objected to having anything done. He thought it was a dilatation of a lymphatic, and cutting it across he liberated the contained fluid. After that she had no further trouble until recently, when the cyst recurred, and was now of the same size as that in Mr. Neame's case. In her case there had been no injury.

Mr. AFFLECK GREEVES suggested that the case was one of implantation cyst. Two years ago he saw another case very much like it, in which the cyst was lined by stratified epithelium. Apparently it arose after a bad injury.

Case of Right-sided Ptosis treated by Poulard's Operation.

By R. LINDSAY REA, F.R.C.S.

PATIENT, a male, aged 4. History: Right side ptosis since birth, eye closed for three months after severe confinement. Treatment by Poulard's operation.

In a case of ptosis it is a problem which operation shall be done. In Paris we were taught there were only two operations worth doing for ptosis; one was Motais', the other was one devised, I believe, by Terrien. In the latter a single strip of skin is used, whereas two are employed in Poulard's operation. By pulling on each strip you can judge most accurately the width of the palpebral aperture which is to result from your operation. It is now four weeks since the operation was done, and already this boy, though only 4 years of age, is learning to use his occipito-frontalis in order to elevate his eyelid. In many cases a Motais' operation is preferable provided it can be done well enough, but in such a case as this Poulard's operation has proved a decided success.

(Blackboard description of the operation.)

Angioma of Retina of Right Eye.

By R. LINDSAY REA, M.D., F.R.C.S.

PATIENT, a female, noticed, in July last, that she could not see with right eye. In 1915 when she went to the East to act as a V.A.D., both eyes were normal. A -10 D. lens is required to see retina of right eye. The vision of the upper part of the retina above the macula is somewhat better than that below the macula, while the area corresponding to the growth is quite blind.

When I first saw this case I was puzzled by it, but Mr. Treacher Collins remembered he had examined a similar case which was illustrated in the *Transactions of the Ophthalmological Society* for 1892 and 1894.¹ The growth in the present case is lower down; much of it can be traced by perimetric methods. The patient can tell you when she sees finger tips alone, &c. There is a large white mass over the macular area which, at first, I thought was of inflammatory origin. Now one knows it to be the result of hæmorrhage. There are many such smaller areas scattered over the retina. If you trace the large vein running down from the disc you come to an area which shows an anastomosing mass of vessels together with whitish areas resembling detached retina. In fact it is the retina being pushed forward by the growth. The coloured illustration in the *Transactions* for 1892 (Plate IV, facing p. 143) represents it exactly except that the growth is much farther from the disc.

The cases referred to in the *Transactions* were those of a brother and sister. Mr. Treacher Collins examined one eye from each. The structure in each case was the same, viz., a mass composed of small capillary vessels and cells, presenting a honeycomb-like formation.

As the history of the present case dates only from 1915 when, according to the patient's oculist, this eye was perfect, the question arises as to what will eventually take place. Most probably glaucoma or staphylomatous bulging will occur; both of these were present in the cases quoted from the *Transactions*.

The patient is being kept under observation.

Mr. TREACHER COLLINS said he did not think angiomas of the retina were so very rare. The picture thrown on the screen by Mr. Rea was that of the well known case exhibited by Dr. D. J. Wood, of Cape Town, when he was house surgeon at Moorfields.

¹ *Trans. Ophth. Soc. U.K.*, 1892, xii, p. 143; 1894, xiv, p. 141.

He (the speaker) had the opportunity of examining the fellow eye microscopically, it having been removed for secondary glaucoma, and found the angiomatous conditions of the retinal vessels. Since that date he had seen several similar cases. Three years ago he showed before the Ophthalmological Society an excellent example in which there were several separate angiomata in the retina in the same eye. He had lost sight of the cases he had seen, and he did not know what was the final outcome of the affection. With regard to the case of ptosis, he preferred Motais' operation to that described by Mr. Rea; in Mr. Rea's case, though a good result had been obtained, he thought an even better might have been produced. In three recent cases in which he had done Motais' operation there had been no scar forming externally, such as was seen in the present case, and the patients could completely close their eyes in sleep, which was probably not possible in the case of the patient shown that evening. Motais' operation was only suitable in cases of ptosis with good upward movement of the eyes. He had done it in patients with unilateral ptosis in which the action of the superior rectus was generally unimpaired.

Congenital Malformation of Iris.

By P. G. DOYNE, F.R.C.S.

PATIENT, a male, aged 8. History: No history of inflammation, with the exception of mild conjunctivitis on two occasions. Very fretful during the first three months of life; not backward in development in other respects.

Present condition: Holes in iris—pigment layer exposed—pigment layer absent in places with red reflex showing, suspensory ligament visible in hole in left eye. Defective development of angle of anterior chamber—strands running from posterior surface of sclero-corneal margin to anterior surface of iris.

DISCUSSION.

Mr. M. S. MAYOU thought the explanation of the formation of colobomata with bridge was, that before the anterior end of the optic vesicle grew forward to form the iris, the capsular pupillary membrane was present and was stripped up by the iris, but persisted over the coloboma to form a bridge. In such a case as the present one, he believed there was some mesoblast in the stroma of the iris, which would account for a sphincter being present. The fetal condition of the angle was due to imperfect formation and to some of the strands of the capsulo-pupillary membrane being attached to the back of the cornea. He had sections showing the blood-vessels in such strands.

Mr. TREACHER COLLINS said that the mesoblastic portion of the iris was developed from two sources: it arose partly from the anterior layer of the fibro-vascular sheath covering the anterior surface of the lens, and partly from the blood-vessels which grew forwards from the periphery of the anterior chamber from the anterior ciliary arteries, beneath the pupillary membrane or anterior fibro-vascular sheath. He agreed with Mr. Mayou that there was a thin layer of membrane over the gaps in the stroma of the iris in the case shown. These gaps in the stroma had resulted, he suggested, from loops of the anterior ciliary arteries having failed to grow in between the pigment epithelium and the pupillary membrane. If in some places those loops of arteries grew forwards and not in others, those which budded forwards might meet at the pupillary border and form a complete circle of anastomosis there, from which normal iris stroma would be developed. The prolongations forwards of iris at the angle of the chamber were similar to those met with in the fetal human eye. In the present case they were a persistence of the fetal condition of the angle of the chamber.

Two Cases of Thrombosis of the Retinal Vein, one showing a Hole, the other a Star, at the Macula.

By F. ARNOLD WILLIAMSON, F.R.C.S.

(ABSTRACT.)

[This Paper will be published in full in the *British Journal of Ophthalmology*, February, 1922.]

THE late Mr. George Coats [1] in his paper on "The Pathology of Macular Holes," supported the theory of œdema being the underlying cause of their formation in traumatic cases. He stated that a hole had never been observed sooner than sixty hours after an injury, while œdema of the retina and subsequent formation of a hole had been observed in cases where there had been no injury. Kipp and Alt [2], however, reported a traumatic case in which a macular hole was observed ophthalmoscopically on the day following the injury; this observation was confirmed by pathological examination when the eye was enucleated four days later.

Sections were shown from the case of a female, aged 62, sight failing for fourteen years, severe pain from August, 1920, till enucleation for absolute glaucoma in July, 1921. Vision in unaffected eye $\frac{6}{9}$ with correction. No history of trauma, except that thirty years previously she had had a fall which blacked both eyes, but did not affect sight.

On macroscopic examination a small hole was seen with the loupe at the macula. Microscopical examination showed evidences of old irido-cyclitis, and in the region of the disc an old thrombosed vein was visible. Some serial celloidin sections through the macula were shown on the epidiascope. In the section passing through the fovea the following characteristics were noted: (1) A slight separation of the retinal pigment layer from the inner layer of the choroid; (2) an accumulation of œdematous exudate between the retinal pigment layer and the layer of rods and cones in the region immediately surrounding the macula; (3) at the fovea, the presence of a small cyst in this layer of exudate. Its anterior wall in the peripheral part was formed only by the membrana limitans interna, the layer of rods being absent, while in the centre the membrana limitans interna was absent, thus giving rise to the formation of a hole; (4) that with the exception of slight œdema in the outer molecular layer, the retina surrounding the fovea appeared to be unaffected.

Attention was called to the formation of the cyst, not in the retina, but in the œdematous exudate.

The patient in the second case had been blind for five years in the right eye, which fourteen days before admission became red and painful, and was excised for secondary glaucoma. The urine was normal. There were evidences of old irido-cyclitis and of a fairly recent thrombosis of central retinal vein. Star figure noted at macula. Sections through this showed cystic degeneration of retina in macular and peri-macular regions, and large collection of coagulated œdematous exudate between choroid and retina, the retinal pigment layer being absent.

The first series of slides therefore showed a hole at the macula occurring in an area separated from the choroid by a layer of subretinal œdema, and the second a process of degeneration affecting an area similarly separated. The problem then, is first to determine the source of the œdema, and secondly, to explain its localization to the submacular region.

Points in favour of the œdema arising from retinal vessels are: (1) A source of venous congestion in the presence of the thrombosed retinal vein; (2) the retinal degeneration in the second series begins in the outer molecular layer, which is supplied by the terminal branches of the retinal vessels; (3) hæmorrhages in the early stages of massive exudative retinitis occur in the outer molecular layer and later break through into the subretinal space. If blood takes this path, presumably œdematous exudate can do so.

Points in favour of a choroidal origin are: (1) The macular portion is poorer in retinal capillaries than any other part of the retina, the fovea being dependent on the chorio-capillaris for its blood supply; (2) the calibre of the choroidal capillaries is greatest at the macula (Leber); (3) Mr. Hepburn [3] has suggested grouping of the posterior ciliary vessels into three separate divisions, one being destined solely for the supply for the macular portion of the choroid; (4) the macular fan of papillœdema due to obstruction to retinal venous and lymphatic return is produced by superficial vesicles of œdematous exudate, whereas in the two cases shown the œdema is in the deeper layers of the retina; (5) the external molecular layer is farther away from the choroid than any of the other retinal layers, which depend on this structure for their nutrition.

The suggested explanation is the presence of a toxic influence, causing irido-cyclitis, thrombosis of the central retinal vein and degeneration of the delicate macular choroidal capillaries. This brings about œdema of choroidal origin, which separates the macula from its source of nutrition, so that it degenerates and forms a hole. In traumatic cases a similar origin of the œdema is possible, the macular choroidal capillaries being injured by *contre coup*.

REFERENCES.

[1] COATS, *Roy. Lond. Ophth. Hosp. Repts.*, 1907-8, xvii, pt. i, p. 69. [2] KIPP and ALT, *Amer. Journ. Ophth.*, 1908, xxv, pp. 225. [3] HEPBURN, *Roy. Lond. Ophth. Hosp. Repts.*, 1910-12, xviii, p. 92.

Mr. M. S. MAYOU said that some years ago he had had a typical case of a star figure at the macula in albuminuric retinitis, sections of which he now showed on the screen. It could be seen that the exudation was in two places: intraretinal exudation and subretinal exudation, just as in Mr. Williamson's specimen. With regard to the origin of the exudation, he thought there was very little doubt that the albuminous fluid in the layers of the retina was derived from the retinal vessels; but the fluid which was subretinal, i.e., between the retinal pigment layer and the retina itself, probably came from the choroid. It was albumin of a different consistence to that of intraretinal fluid, and stained darker, and on the inner surface some pigment could be seen which seemed to be carried forward by the passage of the fluid through Bruch's membrane. It would be seen that there was a considerable vascular sclerosis in the choroid, and no doubt there was some blocking of some of the vessels. The elastic tissue stain which he used was a very ready means of staining the exudates. He would next show the specimen which came from the other eye of the patient with albuminuric retinitis. There was also a star figure by it, but of that he had no sections. It had, in the retina, the thrombosed vein shown, but there were not the typical clinical signs of thrombosis of the central retinal vein present. One did find in the retina of these cases thrombosed veins, without there being the typical appearance of thrombosis present, such as was present in the last case of Mr. Williamson's, which had a star figure apparently without the other typical signs of thrombosis.

The New Psychology in its Relation to Problems of Vision.

By CHARLES F. HARFORD, M.D.

(ABSTRACT.)

[This paper is published in full in the *Medical Press and Circular*, November 30, 1921, p. 445, December 7, p. 468.]

THE problem of vision is without doubt fundamental to the ophthalmologist, but hitherto attention has been mainly directed to the anatomical and physiological aspects of the subject. Parsons, in his text-book on "Diseases of the Eye," after tracing the processes of vision from the external object to the cortex cerebri, states: "Here the nervous impulse is transformed into a psychic impulse which is not, and probably never can be, understood." It was claimed that psychology could offer its important contribution to supplement physiology, and that its recent developments, helped largely by the results of psychoanalysis introduced by Freud, threw new light upon the whole subject of human thought.

Unfortunately, many were devoting attention to psycho-pathology without first studying normal psychology, and it was the object of the present paper to formulate a scheme of the working of the psyche, which might help to interpret many of the problems of vision. This was done mainly by the use of a chart of the action of the psyche, which included the following parts:

(I) A circle X representing, as if it were a gramophone, the mechanism of the psyche divided into three sections of a *cognition*, by which an image or impression, visual or otherwise, is recognized; an *affection*, which stands for the emotional result of cognition, which in its turn leads to a *conation* or action.

(II) A second disc Y, as if it were a gramophone record, stood for the field of consciousness which would present to the psyche the visual and other images received from external objects or from the memory, or from both combined.

(III) Six compartments referred to as the store chambers of the psyche were described as (a) results of careful observation, (b) results of casual observation, (c) amnesia of common life, (d) pathological repression, (e) infantile impressions, (f) instinctive and hereditary factors.

These would be usually referred to as the unconscious, and the circles within them were spoken of as psychograms. The relation of certain common terms to this scheme was then explained:

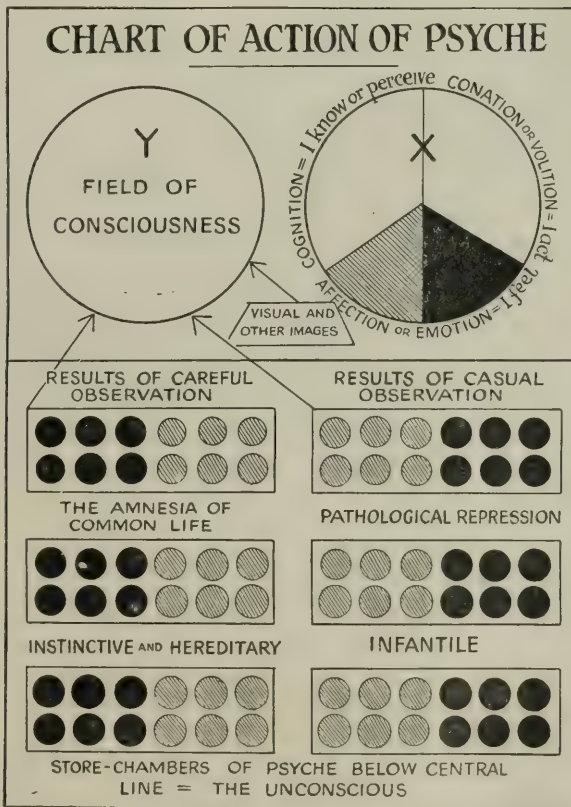
(i) *Consciousness* was represented by fields of consciousness, a succession of which would be spoken of as the stream of consciousness.

(ii) *Memory*, commonly used for the threefold process of the reception of a mental concept or psychogram, the storing of this by the psyche, and the recovery of this in the field of consciousness. Each step of this process is represented in the chart. Emphasis was laid upon the emotional part of the psyche which activates each mental concept, and this was connected with "the urge" of the British psychologists, the "élan vital" or "énergie spirituelle" of Bergson, or the "libido" of Freud and Jung. This great force was represented on the chart by the two colours green and red (marked by shading and black as reproduced) signifying hope or fear or similar antagonistic emotions, and the psychograms consigned to the unconscious were regarded as charged with these qualities and coloured in the same way.

(iii) *Repression* was regarded as the means by which each of the psychograms was relegated to the unconscious, and though this term is usually spoken of in connexion with pathological repression, yet it was contended that a similar process is at work in normal cases, though with very different results.

(iv) *Association* was defined as the means by which the ideas or psychograms were brought back into the field of consciousness, perhaps by virtue of some special attractive force attached to them.

(v) *Dissociation* was explained with reference to the circle Y. It was suggested that the psyche possesses alternating methods of dealing with the



content of the unconscious, such as the dream life and the ordinary waking life, though there might also be day dreams, and that this alternation might account for perverted visual and other impressions.

(vi) *Apperception* as applied specially to vision signified the accommodation between new visual images and existing visual memories, which was constantly taking place in the field of consciousness, and this process would be illustrated by a number of instances, and also accounted for the different impressions of a number of observers looking at the same object.

(1) *The vision of an infant* was only gradually evolved, not because of any defect of the organic structures concerned in vision but, as it would be termed,

owing to a gradually awaking intelligence. There would be certain instinctive and hereditary predispositions, which would develop a visual memory which by education led to more or less highly developed visual powers, even in the absence of any organic defects.

(2) *A person blind from birth*, one particularly intelligent witness being cited, had of course no idea of colour, and could only very imperfectly apprehend problems of space, size and distance. Much might be learnt from similar cases.

(3) *Strange objects* seen for the first time by normal adults could not be recognized until they could be related to some previous visual memory with which an association could be formed.

(4) *The use of optical apparatus* such as an ophthalmoscope was largely a matter of training and experience, only to be acquired by the storing up of a series of careful visual memories. Reference was made to psychological difficulties in the use of spectacles.

(5) *Pictures* of all kinds, even including caricatures, conveyed impressions which were dependent upon previous associations, and were meaningless to the primitive savage. The artistic eye with its recognition of perspective could only be developed by careful practice, though hereditary and instinctive influences would play their part.

(6) *Recognizing faces* is a matter of ordered visual memory.

(7) *The athletic eye* is generally spoken of as largely a matter of judgment, and this is a striking instance of the influence of memories in the process of vision.

The author believed that this view of the problem of vision opened up great possibilities of practical help in the practice of ophthalmology.

Section of Ophthalmology.

President—Dr. JAMES TAYLOR, C.B.E.

Hyaline Bodies at the Disc, associated with Night Blindness.

By M. L. HEPBURN, F.R.C.S.

PATIENT, a male, aged 16.

Discussed by the PRESIDENT.

Lobulated White Mass at Macula.

By C. J. LONGWORTH BLAIR.

PATIENT, a male, aged 11. Vision in right eye defective for about the last two and a half years. He is stated to have had laryngitis, whooping-cough and croup, during or about the years 1914-15; also an attack about 1914-15 of (?) chorea. The point of special interest consists in the appearance of the lobulated white mass at the macula.

Discussed by Mr. BATTEN, Mr. FISHER, Dr. HARFORD and Mr. MAYOU.

Case of Cysts of Iris.

By R. R. CRUISE, F.R.C.S.

PATIENT, a male, aged 37, clerk, first attended Royal Westminster Ophthalmic Hospital on July 13, 1921, with history of "noticing marks in right eye for several years—at first very small." The following note made in out-patient department: "Apparently two cysts in iris with partition, and base consisting of pigmented floor of iris tissue." Pigmented area in iris up and out. One small point of reflex. No history of trauma, but faint nebula present below—? on deep aspect of cornea. Vision, $\frac{6}{24}$. No change since patient was first seen.

DISCUSSION.

Mr. CRUISE asked for expressions of opinion as to whether an operation should be undertaken or not.

Mr. MCMULLEN described a similar case in which no injury had been noticed in early life. He admitted the possibility of an injury without any knowledge of its occurrence. He thought it wiser not to operate.

Mr. MAYOU cited pathological evidence showing that when these cysts were operated on they were liable to recur, and in some cases operative interference had resulted in iridocyclitis, with in one case the loss of the eye. He agreed with Mr. McMullen that operation was not advisable.

Mr. HINE mentioned a case of implantation cyst. He also agreed that the cyst in this case should not be interfered with.

Mr. J. HERBERT FISHER thought that the dangers of operation were much exaggerated. He had one case in which a free iridectomy was followed by no recurrence; and he advised that in the present case the cyst should be punctured.

Coloboma of Optic Nerve (? Traumatic).

By J. F. CUNNINGHAM, F.R.C.S.

PATIENT, a female. No history of injury. Right vision : No perception of light (a highly myopic eye, refraction -18.0). Left vision : $\begin{matrix} -6.5 \text{ sph.} \\ -3.0 \text{ cyl.} \end{matrix}$ axis $30 = \frac{6}{9}$. Seen at the Central London Ophthalmic Hospital; the right has been defective as long as she can remember.

Discussed by Mr. HEPBURN, Mr. WALKER and Mr. MAYOU.

Inflammatory Nodule above Right Optic Disc.

By R. LINDSAY REA, M.D., F.R.C.S.

PATIENT, a female, aged 75. In May of this year (1921) a whitish nodule was noticed above right optic disc. It required a $+6$ D. to focus surface. Now it can be seen with $+4$ D. Degenerative retinitis in right and left eyes. The condition is most probably tubercular.

Case of Sclerosing Keratitis of Right Eye.

By R. LINDSAY REA, M.D., F.R.C.S.

PATIENT, a female, aged 30. The condition was first noticed in September last. No previous scleritis. All causes can be excluded except the factor of septic teeth.

Discussed by Mr. CRUISE.

Gunshot Wound of Left Eye and Lower Lid; Operation for the Formation of New Lower Lid.

By M. W. B. OLIVER, M.B.

PATIENT, wounded May 3, 1917. Prisoner from the time he was wounded until 1919.

Condition on admission to Queen's Hospital, Sidcup: Upper lid intact; lower lid had practically disappeared, exposing the conjunctiva of the lower fornix.

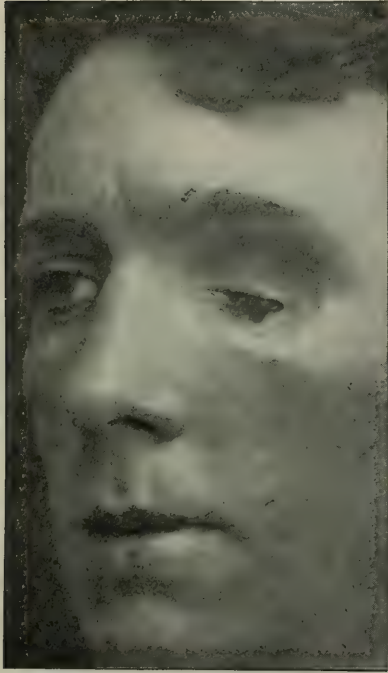


FIG. 1.



FIG. 2.

April 1, 1921: Complete new lower lid made by bringing down a temporal flap. The lining of the new lid was made by turning in a skin flap from the malar region.

Discussed by the PRESIDENT and Mr. GOULDEN.



FIG. 3.

Double Congenital Ptosis; Motais' Operation on Right Eye.

By R. AFFLECK GREEVES, F.R.C.S.

PATIENT, a male infant, aged 12 months. Complete congenital ptosis of both eyes. Motais' operation performed on the right eye, October 19, 1921. The left eye has not been dealt with.

Section of Ophthalmology.

President — Dr. JAMES TAYLOR, C.B.E.

Growth on the Conjunctiva in an Infant.

By P. G. DOYNE, F.R.C.S.

PATIENT, a female infant, aged 4 months. Swellings to outer side of limbus in each eye noticed since birth. ? Dermoid. ? Subconjunctival lipoma. Other congenital defects: Bifid tongue; three phalanges on right thumb.]

The case was discussed by the PRESIDENT and Mr. FOSTER MOORE, and the general opinion was that the growth on the conjunctiva was a dermoid.

Case of Filamentary Keratitis.

By R. LINDSAY REA, M.D., F.R.C.S.

PATIENT, a female, aged 53. This woman has suffered from this condition for over two years. I have had the patient under observation for nine months, and have treated the condition by removing the filaments and carbolizing the small denuded areas which remain. The treatment, so far, has been useless. The patient now returns after three or four weeks with a fresh crop of filaments. At this stage the eyes are exceedingly painful. The tension of each eye is normal, and the filaments are not preceded by bullæ. The patient suffers from osteoarthritis and rhinitis sicca.

Case of ? Branching Remnant of Hyaloid Artery.

By HUMPHREY NEAME, F.R.C.S.

PATIENT, a female, aged 66. History: The patient has noticed mistiness in the left eye, and "black spots" several years. Right vision: $\frac{1}{80}$ with + 1.5 = $\frac{6}{36}$. Left vision: $\frac{6}{24}$ (2) with + 1 = $\frac{6}{9}$. Right fundus shows some floating strands in the vitreous, arising, as far as can be ascertained, by a single stalk from the disc. In front there are five main strands, close together, in the position of the hyaloid canal. They float in the vitreous much as certain seaweeds float under water. There is no attachment of these strands to the lens, and no visible opacity at the back of the lens.

DISCUSSION.

Miss MANN showed slides and drawings of embryos, one at five weeks and the other at three months, indicating that a branching hyaloid artery was the rule rather than the exception, and that there were five main branches which broke up again into smaller branches eventually surrounding the lens.

18 Mayou: *Osteomyelitis of the Upper Jaw in Infants*

Mr. MAYOU said he had specimens which supported Miss Mann's observations regarding the branching of the hyaloid artery. He also regarded the fibrous tissue plaque on the back of the lens, generally supposed to be the remains of the hyaloid artery, as really atypically developed fibrous tissue formed originally in the cone made up by the branching vessels.

Mr. LESLIE PATON inquired whether the possibility of the case being one of retinitis proliferans had been considered. It might not be one of persistent hyaloid at all. He also suggested that the fibrous tissue development might be due to an old hæmorrhage along the canal of Cloquet.

Mr. NEAME (in reply) thought Mr. Paton's suggestion was a possible one, but that the fact of the branches emanating so distinctly from the posterior part rather negatived this view.

Osteomyelitis of the Upper Jaw in Infants.

By M. S. MAYOU, F.R.C.S.

M. T., aged 4 months. Admitted to the Bolingbroke Hospital on July 28, 1921, with a temperature of 100·8° F., pulse 102, respiration 28; with a sinus and swelling below and to the outer side of the right eye. Two weeks before admission the swelling, which was attributed to an insect bite, appeared below the right eye. An incision had been made into it by the family practitioner parallel to the lower margin of the orbit and a quantity of pus evacuated. Two and a half weeks after admission the sinus was opened up and scraped. The patient remained in the hospital for two months and was finally discharged with the sinus unhealed. A piece of bone was extruded from the sinus to-day.

Remarks.—A second patient which I hoped to show to-night was unfortunately unable to be brought. It was also the case of an infant about the same age, admitted to the same hospital with a swelling below the right eye, the lids of which were red, cedematous and tender. The eyeball was proptosed. The child was ill, temperature 104° F., and pulse 140. Several days before admission an abscess in the hard palate had been incised by the general practitioner, but this had healed completely on admission. An incision was made parallel to the lower border of the orbit and carried down to the bone; this liberated a quantity of pus, and bare bone in the floor of the orbit could be felt. An immediate improvement took place in the child's condition, the temperature falling to normal in about a week. A sinus persisted until the child was discharged from the hospital six weeks later, but has now healed.

The first case of this kind which came under my care was seen in 1901, in an infant of much the same age, when practically the whole of the upper jaw came away piecemeal as a sequestrum. The source of infection is difficult to understand as these children have no teeth. As a rule the nose could also be excluded, as there are practically no sinuses and I think in all probability the infection is metastatic, as in the case of the long bones. The Wassermann reaction was negative in the first case, and in the second case the general practitioner said there was no suspicion of syphilis either in the parents or in the child.

Discussed by Mr. PATON, who mentioned a similar case under his care on which he had operated, also by Mr. STACK; and Mr. MAYOU replied.

An Operating Lamp.

Shown by **BASIL LANG, F.R.C.S.**

THIS lamp consists of a gas-filled "focus type" electric bulb, a condenser and a corrected objective, contained in an aluminium body fitted with a hinged detachable handle. In the operating theatre it is held by means of this handle. In the consulting room the handle is removed, and the lamp fits into the ophthalmoscopic bracket in place of the usual bulb. In this way it can be raised or lowered, rotated or tilted into any convenient position. A filter can be added, with the aid of which fluoresceine staining may readily be seen. This enables one to use focussed light in the dark room to examine the cornea or minute staining conditions.

The lamp throws a circle of light of uniform illumination, free from filament image, 2 in. in diameter at 7 in., 6 in. in diameter at 22 in.



Experience gained from 140 Trephine Operations for Glaucoma.

By **MALCOLM L. HEPBURN, F.R.C.S.**

(ABSTRACT.)

Mr. HEPBURN related his experience gained from 140 trephine operations, twenty-nine of which had been in private practice, and the rest in hospital; and he expressed the opinion that, if all the experience of other surgeons who were in the habit of frequently performing this operation were collected and added to his, a very large amount of evidence could be accumulated in favour of this treatment for, at any rate, chronic glaucoma.

He described the technique which he usually employed and laid special stress on obtaining the thickest possible layer of conjunctiva for covering the hole, and

also on taking every precaution to avoid button-holing the flap at any stage of the operation where this accident seemed likely to occur. While actually trephining, he held the handle of the trephine forwards so as to cut through the anterior part of the sclero-corneal tissue before the posterior, thus ensuring a hinge posteriorly as far away from the conjunctival flap as possible when the hole was complete. He always used a pair of straight one-toothed forceps for the button-hole iridectomy.

He mentioned various complications which had occurred to him in his experience of the operation, in common with that of others: button-holing the flap, loss of the disc, unintentional complete iridectomies, loss of vitreous, delay in the re-formation of the anterior chamber, opacity of and pushing forward of the lens, and detachment of the choroid; and discussed the effect of such accidents on the results of the operation.

These complications had only occurred in his practice on comparatively few occasions, and in most cases the effective drainage through the trephine hole had not been impaired. Opacities of the lens had only happened in three or four instances, and he found this difficult to explain; and he thought that the choroid after being detached at an early stage generally became replaced again.

With regard to late infection, he thought the term could only be justified in those cases in which the conjunctival flap had for some reason become an insufficient covering over the trephine hole, and thus encouraged the entrance of organisms.

On looking through his records nothing struck him so much as the fact that the greatest number of successes had been secured in those cases which were operated on in the early stages, though many with very poor vision had been immensely improved in every way by the operation.

He had had about thirteen failures, including those cases in which acuity of vision had sunk to hand movements or only perception of light, which consisted mostly of cases of secondary glaucoma, acute primary glaucoma, and buphthalmos; and therefore he never performed the operation in either of the two first conditions, but always did an iridectomy.

He had to record one case of suppuration due to a weak conjunctival flap. He had never had a case of sympathetic inflammation following trephining.

He recommended the operation as ideal in chronic glaucoma, and the earlier it was undertaken the better.

DISCUSSION.

Mr. CRUISE stated that he had ceased to perform the trephining operation owing to the fact that he had had unfortunate experience in most of his cases from loss of vitreous, infection, and in one case undue prominence of the scar which interfered with the proper closure of the lid. He had often noticed a transparent bubble of thin conjunctival tissue covering the hole, which looked very dangerous. He was in the habit of doing a modified flap sclerotomy in cases of glaucoma.

Mr. MAYOU agreed with Mr. Cruise with regard to the bubble-like swelling over the trephine hole, and thought this happened fairly frequently. He also objected to the splitting of the cornea. He himself had recently been doing Holt's new operation of sclerotomy, a description of which had appeared in the *British Journal of Ophthalmology* for December, 1921. He described Professor Harden's contact-illuminator method of detecting detachment of the choroid.

Mr. BASIL LANG considered that the danger of infection from an open wound in the conjunctival flap was much exaggerated, and that infection did not necessarily occur under such conditions. He enumerated some dangers associated with trephining, such as damage to the lens, detachment of the choroid, cyclitis, and the bubble-like appear-

ance over the hole due to narrowing of the drainage area. Other operations for the relief of glaucoma which he recommended were the old iridectomy and Lagrange's operation.

Mr. HEPBURN (in reply) considered Mr. Cruise's experience had been most unfortunate, and that Mr. Mayou attached too much importance to conjunctival blebs and splitting of the cornea. The former was not so common as he supposed while the conjunctiva often became thicker again after a time, and the latter made no difference and served to strengthen the flap. He thought the Lagrange operation was similar to the trephine operation in technique and method of filtration. That eyes escaped infection when the hole into the eye was uncovered by conjunctiva did not justify carelessness in dealing with the conjunctival flap.

Epibulbar Sarcoma with Penetration of the Globe.

By HUMPHREY NEAME, F.R.C.S.

(ABSTRACT.)

IN May, 1919, a patient, F. B., male, aged 53, had noticed a swelling under the upper lid of the left eye. There was a history of syphilis, recent ulceration of palate, deformity of nasal bones, and positive Wassermann reaction. For a considerable time the swelling was considered to be a gummatous infiltration, but as it failed to react to antisymphilitic treatment, and as the eye was practically blind, eventually, Sir John Parsons, who had charge of the case at the Royal London Ophthalmic Hospital, decided to enucleate the eye. Pathological examination revealed an extensive epibulbar growth with extension within the lobe. The growth surrounded the cornea and as a thin sheet spread around the eye to the posterior pole. The iris, ciliary body, and choroid were completely infiltrated with the same type of growth. Microscopically, the growth was a round-celled sarcoma with a slightly alveolar formation. Within three months of the date of enucleation there was a recurrence within the orbit and exenteration of the orbit was carried out by Sir John Parsons. There was a mass of round-celled recurrent growth not definitely delimited from the orbital tissues. The stump of the optic nerve, in transverse section at the posterior limit of the growth, was free from growth cells.

The outstanding feature of this case was that there had apparently been an extra-bulbar sarcoma with extension within the globe—an extremely rare condition. Less rarely sarcoma started in the choroid and extended out through the coats of the eye, usually by the perivascular lymphatics of the various perforating vessels. There seemed to be more evidence in favour of an extra-bulbar than of an intra-bulbar origin of the growth in this case. The main points in this evidence were: (1) The earliest symptoms of which the patient complained were referred to the epibulbar region above the cornea. (2) The vision, as tested within two months of the first symptoms, was found at the Royal London Ophthalmic Hospital to be $\frac{6}{6}$ with the appropriate glass in the left eye. The patient did not notice any failure of vision in the left eye until seven months after the onset. An extensive choroidal growth usually, though not always, had some effect upon the vision. (3) Pathological examination revealed that the epibulbar growth above the cornea—i.e., at the site at which it was first noticed—was much more massive than at any part of the choroid. (4) The gross appearance of the choroidal growth resembled a secondary or metastatic growth, in its wide and even distribution, more than a primary growth in that situation.

(5) Flat sarcoma of the choroid was usually densely pigmented. This was a non-pigmented growth apart from a few streaks of pigment in the choroid—surely the normal choroid pigment. (6) Flat sarcoma was almost always relatively avascular. Throughout this growth there were many delicate capillaries. (7) Flat sarcoma often invaded the deeper layers of the sclerotic, whereas this growth had done this only to the slightest degree, and, on the other hand, had invaded to a definite extent the superficial layers of this structure in various parts.

DISCUSSION.

Sir JOHN PARSONS, F.R.S., said the case was at first thought to be gummatous infiltration and other inflammatory developments pointed to this view. Epibulbar growths very rarely extended inwards from without, though there was nearly always the possibility of some small focus being found within the eye which had escaped notice in the early stages.

The PRESIDENT asked whether the fracture of the tibia was due to metastasis.

Mr. MAYOU was not at all convinced that it was an epibulbar growth extending into the eye. He instanced a case which showed that intra-ocular growths could remain undetected if situated in certain places for a long time, and he advocated the formation of a Pathological Committee to make a report on this case which was practically unique.

Mr. TREACHER COLLINS remarked that there might be some doubt in the case of a sarcoma as to where the growth originally started, but not in cases of an epithelioma. He had himself described cases of a similar nature to this one in a paper read before the Ophthalmological Society. The method of extension in this case was suggestive of growth from without inwards, viz., backwards into Tenon's capsule. Had a piece of the growth been removed and examined in the early stages it might have been possible to decide as to its nature, and avoid removal of the eye by treatment with the galvanocautery and radium.

Mr. PATON described a similar case of a man under his care in 1917, where he had treated the growth locally by dissecting it away as far as possible, followed by the application of radium. There had been no recurrence in the orbit but two and a half years later a swelling had appeared in the soft palate which was first regarded suspiciously, but after treatment with radium had completely disappeared. He also described a case he had seen at Queen Square Hospital where a post-mortem on a man who during life had suffered from a rather obscure nerve condition, had revealed a flat sarcoma in one eye with metastases in the whole of the spinal cord and base of the brain.

Mr. NEAME (in reply) said there was no connexion between the tibial fracture and the present growth. He welcomed the formation of a Pathological Committee to decide on the findings in his case.

The PRESIDENT nominated Mr. TREACHER COLLINS, Sir WILLIAM LISTER, and Mr. MAYOU to confer with Sir JOHN PARSONS and Mr. NEAME.

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Section of Ophthalmology.

President — Dr. JAMES TAYLOR, C.B.E.

Case of Symmetrical Macular Disease.

By F. A. JULER, F.R.C.S.

PATIENT, a male, aged 17.

The case was discussed, and opinions expressed as to whether it was a familial type of case or inflammatory.

Gunshot Wound of Right Eye: Plastic Operation on the Lid— an Intermediate Stage.

By M. W. B. OLIVER, M.B., F.R.C.S.

PATIENT was wounded in 1916. A good many operations had been performed on him when he came to me two months ago. The whole of the skin of the upper lid had disappeared, and the lid was pulled up and was adherent to the upper orbital margin. The cornea was exposed. He has had severe corneal ulceration, which has left a nebula. I first excised all the scar tissue (in all plastic operations it is most important to replace tissue in its normal situation). The lid could then be replaced in its normal position with the conjunctiva, which was intact, and the skin margin, with some of the lashes. That left a large bare surface which was covered by a temporal flap, the pedicle of which is now shown (slide exhibited). In making a temporal flap it is essential to include the superficial temporal artery. Owing to the length of the flap and its narrow base, it would be lost if the superficial temporal artery were not included. All that remains is to remove the whole pedicle. If the pedicle were very wide, you would have to open up the part you wanted to use, and replace it in the wound; but in this case, where one can bring the edges together, there is no need to do that. This is Major Gillies' operation, and it is a most useful one.

Case of Blepharo-chalasis.

By R. AFFLECK GREEVES, F.R.C.S.

PATIENT, a female, aged 28.

Superficial Deposit of Cholesterin in Corneal Scar.

By R. AFFLECK GREEVES, F.R.C.S.

PATIENT, a male, aged 30.

On the Morphology of certain Developmental Structures associated with the Upper End of the Choroidal Fissure.

By IDA C. MANN, M.B., B.S.

(ABSTRACT.)

TWO points in the development of the human eye were considered, namely, the origin and morphological value of certain cells found on the vitreous surface of the optic disc in the human embryo, and also the meaning of a

small mass of non-pigmented cells found on the back of the optic cup in the pigment layer immediately below the insertion of the optic stalk.

Slides were shown illustrating the development of the posterior part of the eye in various vertebrates (man, pig, mouse, dogfish, terrapin, lizard, chick). It was shown that in these animals there is at first a more rapid growth of the inner layer of the optic cup than of the outer, and that this leads to an eversion of the inner layer in the upper part of the choroidal fissure and, in some cases, to a heaping up of the inner layer along the margins of the fissure inside the eye (*crista intraocularis*). When closure of the fissure takes place fusion occurs between two areas derived from the inner layer of the optic cup and two ridges are formed, one on the outside and one on the inside of the eye, both derived from the inner layer. In birds the *crista intraocularis* is cut off from the rest of the inner layer by the issuing nerve fibres, and later becomes vascularized to form the pecten (which is thus primarily of ectodermal origin). In man and some other mammals it forms a nidus for the developing retinal vessels. The everted portion of the inner layer in birds is invaded by outgrowing nerve fibres and so forms the "cauda" of the nerve, while in man it is not so invaded, and is only present for a short time as the unpigmented mass of cells mentioned above.

CONCLUSIONS.

(1) The wedge of cells seen on the optic disc in human embryos is epiblastic in origin and represents the *crista intraocularis* which in birds and reptiles becomes the pecten.

(2) The unpigmented mass of cells seen below the optic stalk on the back of the eye in human embryos of about 15 mm. consists of the remains of the everted portion of the inner layer of the optic cup and represents an abortive potential "cauda" of the optic nerve.

Three Cases of Choroidal Sarcoma, with Notes on the Microscopic Appearances.

By H. J. MAY, M.B., and F. A. WILLIAMSON-NOBLE, F.R.C.S.

(I) CLINICAL REPORT, BY H. J. MAY, M.B.

(ABSTRACT.)

THREE cases seen within nineteen days; all men over 50 years of age. In all cases the right eye was affected.

Case I (May 12, 1921).—W. B. C., aged 60. Symptoms of presbyopia. "Distant vision perfect." Excellent physical condition. Examination, right eye: Tension slightly higher than in left eye; anterior chamber natural; pupil round, rather larger than in left eye; sluggish to light stimulus; media clear. Fundus: Below and to outer side of optic disc is a kidney-shaped swelling, roughly $1\frac{1}{2}$ discs long by 1 disc broad; on the concave border, which involved lower margin of macula, are two small round retinal hemorrhages; two folds of retina with vessels curving over as if pushed forward by a mass from behind. Vision: Hand movements at 18 in. and that but dimly. Field of vision (roughly): Scotoma on inner side of right field much larger than fundus appearances suggested. No evidence of antecedent disease, except "getting thinner for four and a half months." Complete ignorance of blindness until examination. Diagnosis: Malignant disease of choroid. Advised further consultation and enucleation; both refused; warned of results; to come again in six weeks' time. Seven months later, having been quite free from any symptoms until two days previously, came with acute secondary glaucoma:

tension + 3; cornea dull, but not intensely so; no view of fundus; quite blind. Enucleation three days later (December 16, 1921). Mushroom-shaped growth occupying whole of macula; complete detachment of retina; no naked-eye evidence of extension outside globe. Hospital pathologist's report: "melanotic sarcoma; section of nerve shows no sign of growth."

Case II (May 18, 1921).—J. J. F., aged 52. Sent to hospital as a case of iritis. History of having attended at an eye hospital three and a quarter years previously for failing vision and pain in right eye of three months' duration. Big healthy looking man with painful injected right eye; no trace of iritis; cornea quite bright and clear; tension at least + 2; anterior chamber apparently obliterated; iris quite bright, appearing to be in contact with Descemet's membrane; pupil dilated, pear-shaped and inactive, filled by iridescent-looking lens tilted forwards and outwards; no perception of light. Enucleation (May 19, 1921): Growth in macula, half-sphere in shape, about the size of an average marrow-fat pea, the innermost margin attached to or growing from outer edge of optic disc; retina completely detached. One week later pathologist at hospital reported, "Optic nerve involved." Exenteration of orbit performed (May 26, 1921).

Case III (June 1, 1921).—W. B., aged 67. Tall, thin, sallow-faced; complained of loss of right vision for six months, and some pain in left eye for two months; history of having consulted an ophthalmic surgeon three and a half years previously for pain and blindness of right eye. Examination: Both eyes quite quiet; tension normal; media clear; in right fundus external to disc, a large pigmented mass with folds of retina lying over or intermingled with it. Vision: Hand movements at 18 in. Homatropine: Edge of right optic disc blurred and indistinct, and to outer side extending far into periphery a large area appearing to consist of new tissue development in and among folds of retina, the retina itself appearing to be obscured by strands of connective tissue; the whole mass is intensely dark in colour, making differentiation of detail impossible. Diagnosis: Probably malignant disease of choroid; to come again for examination, under no circumstances later than three months hence, and warned of probable course of events. Came four and a half months later. "Now right eye is totally blind." Vision: Not even perception of light; pupil widely dilated; cortical opacities in lens; in fundus a large mass pushing contents forward. Advised enucleation. Six weeks later consulted Mr. Leslie Paton, who agreed as to nature and treatment. Enucleation, November 29, 1921. Hospital pathologist's report: "Melanotic sarcoma; no evidence of extension of growth along optic nerve."

POINTS FOR CONSIDERATION.

(A) What are the earliest signs of the real nature of these conditions? In Cases II and III three and a quarter years and three and a half years previously there had been noted choroidal changes in the macula with or without pigmentary deposit, together with failing vision.

(1) If we have been able to exclude the possibility of specific disease (tubercle, syphilis, &c.) and toxic causes, are we justified in regarding these cases of a definite swelling in one eye only as of malignant nature at the outset, or is it only that degenerated conditions of the macula are especially prone to become the seat of malignant disease?

(2) Are there any means yet available for making an earlier certain diagnosis?

(3) How can we impress on patients in the early stages the importance of submitting to enucleation and even exenteration without delay?

(4) Is the glaucoma onset (which does seem to serve this useful purpose) to be the only determining factor?

(B) As to the glaucoma in these cases. Why does it occur at all? It does not always (Case III). The size of the tumour seems to have little if indeed anything to do with it. Why is the onset so sudden, for the disease has existed for a long time? Is it due to obliteration of the angle of the anterior chamber? In point of fact, is the angle really obliterated?

In these cases of acute secondary glaucoma with an intra-ocular tumour, I seem to have noticed that the œdema of the cornea is never anything like so intense as in acute congestive glaucoma; often indeed there is none, as in Case II. Is this absence of corneal œdema with brightness of iris a clinical sign of any importance in such cases, and, if so, what is the pathological interpretation? Can it be that the increased tension is due to a blocking somewhere post-equatorially, and the consequent steady increase in tension the cause of the obliteration of the anterior chamber, so that the closure of the anterior angle in these cases is a consequence of, rather than the cause of, the glaucoma?

(C) Again, even with a small tumour at the posterior pole of the eye, why in these cases is the retina entirely detached and folded up? What has become of the vitreous? How and why has it been so completely absorbed?

Addendum.—Case III: Patient died February 11, 1922, from "cancer of the liver and stomach." The other two patients (Cases I and II) remain in good health and at work and quite free from any symptoms of dissemination.—H. J. M. (May 5, 1922).

(II) NOTES ON THE MICROSCOPIC APPEARANCES OF THE CASES,
BY F. A. WILLIAMSON-NOBLE, F.R.C.S.
(ABSTRACT.)

Case I.—Large growth projecting forwards from back of eye. Choroidal sarcoma. Composed of closely packed round cells, with little pigment and few blood spaces. Infiltration of walls of a vortex vein, invasion of one-third thickness of sclera over a distance of 3 mm. from front to back. No involvement of optic nerve.

Case II.—Spindle-celled choroidal sarcoma. Small brownish mass in centre of optic nerve resembling extension of growth but in reality due to extravasation of blood.

Case III.—Choroidal sarcoma, composed of closely packed round cells, and many blood spaces. The pigment is autochthonous, gives no iron-reaction and is situated for the most part in cells of the growth which are farthest away from the blood spaces. Serial sections of the optic nerve showed infiltration of the growth along the walls of the central artery for a distance of 0.9 mm. Close to the globe a quadrant of the nerve is invaded and direct continuity can be traced between this and the growth itself. The marked vascularity is due to the advanced nature of the growth.

DISCUSSION.

Mr. LESLIE PATON said he saw one of the patients referred to, and there was no doubt that the eye was full of growth; no transillumination could be obtained. He considered that the glands in that patient were definitely enlarged, though not greatly so. He did not connect this enlargement with the ocular growth.

Mr. AFFLECK (GREEVES) said the tracking of the growth along the optic nerve was an interesting feature in one of the cases. That was uncommon, and perhaps it was found in this case because it was in such an advanced stage. Dissemination occurred usually through the venous system.

Section of Ophthalmology.

President — Dr. JAMES TAYLOR, C.B.E.

Circinate Retinitis to Nasal Side of Optic Disc, with Excavation of the Optic Disc.

By RAYNER BATTEN, M.D.

PATIENT, a female, aged 54, has a large white circular patch on the nasal side of the disc, resembling in some ways retinitis circinata, except that it is more raised and there is more definite exudation. In association with it there is a very deep, almost glaucomatous excavation of the disc, which has occurred whilst she has been under observation. She came to me as a normal patient in 1916; vision in the right eye was $\frac{6}{6}$, and the tension, pupil and optic disc were noted as normal. She came again in April, 1920, when she had had some pain for six days. The fundus could not then be seen, because of vitreous opacities. In July, 1921, the vitreous had cleared, and then this present condition of fundus was seen. Also, the optic disc was then noted as having a definitely large cup, extending almost to the margin. There was no evidence of tension, either when she came at first or when the cupping of the disc was noted. Therefore I claim that this case gives definite evidence of the occurrence of cupping being due to something other than increased tension.

Mr. HARRISON BUTLER thought that this was one of those rare cases of chronic glaucoma without tension, which were originally described by von Graefe. He thought that they were due to the cavernous atrophy of the optic nerve described by Schnabel. There was an excellent specimen of this condition in the Museum of the Birmingham Eye Hospital. He found very few examples of it now that he made a routine use of the tonometer. The circinate retinitis was an interesting feature. It was unusual to find it on the nasal side of the disc for it was always described as a macular condition. He did not think that it had anything to do with the glaucoma but was merely a coincidence.

Case of Bilateral Enlargement of the Lacrymal Glands.

By REGINALD THORPE.

PATIENT, a girl, aged 14. The condition has lasted five weeks, and there has been no previous trouble in the eyes.

DISCUSSION.

Mr. LESLIE PATON drew attention to the report of a case of double enlargement of the lacrymal glands in a man, which appeared in the February issue of the *American Journal of Ophthalmology*.¹ In that case the origin was supposed to be syphilitic, and it cleared up after one dose of salvarsan! About fifteen years ago he (Mr. Paton) had had a case of a woman aged about 40, in whom the swelling of the glands came up in

¹ H. W. Cooper, "Symmetric Cystic Enlargement of the Lacrimal Glands due to Syphilis" (with photograph), *Amer. Journ. Ophth.*, February, 1922, p. 125.

the same sudden way, but the swelling of the glands persisted after the acute inflammation had subsided. Ultimately he had to remove both lacrymal glands, and the appearances were those of simple inflammation; there were no signs of tubercle, and no evidences of syphilitic infection nor of growth. Possibly the glands would have subsided if he had left them long enough, but the patient begged to have something done, as she was unable to open her eyes properly. No other glands were enlarged, and there was no reason to suggest it was Mikulicz's disease; the only case of Mikulicz's disease he had seen he came across within three months of seeing the case he had just described.

Dr. JAMES TAYLOR, C.B.E. (President), said he had seen an extraordinary enlargement of the lacrymal glands in a case in which iodide of potassium was being taken.

Syphilitic Degeneration of the Retina in a Boy aged 8.

By R. LINDSAY REA, M.D., F.R.C.S.

I INTEND giving this boy full antisyphilitic treatment in the hope of doing him some good.

Case of ? Retinitis or Choroiditis.

By R. LINDSAY REA, M.D., F.R.C.S.

PATIENT, a female, aged 20, has suffered from this condition for the past six months. She first noticed the vision in the right eye was worse than that of the left; and with correction the vision could not be raised beyond $\frac{6}{12}$. On ophthalmoscopic examination at and around the macula of the right eye a somewhat coarse watered-silk appearance can be seen. If this appearance is exudate then it is running in fine streaks. Choroidal vessels cannot be seen and there is no pigmentation. During the past three months the affected area seems to have spread slightly. The Wassermann reaction is negative, the blood count is normal, the patient's general health is good but there were ten septic teeth all of which have been removed.

DISCUSSION.

Mr. MALCOLM HEPBURN remarked that Mr. Lindsay Rea said nothing about the large vitreous opacity which was noticeable in this case; there was a long thin linear opacity. He (Mr. Hepburn) thought this might be the remains of a vitreous hæmorrhage, which had caused some fibrous degeneration over the retina. He felt sure this was a retinal condition.

Mr. HARRISON BUTLER thought that one of the streaks was superficial. He suggested therefore that the exudate was chiefly in the layer of nerve fibres.

Mr. LINDSAY REA (in reply) agreed as to the importance of the vitreous opacity mentioned by Mr. Hepburn. On the patient's sheet he had omitted to lay stress upon it.

Unusual Condition of Retinal Arteries.

By H. GRIMSDALE, M.B.

PATIENT, a male, aged 34. This is a case of a well-known but not very common group; it is depicted in Frost's "Atlas." Mr. Frost always maintained that these arteries had nothing to do with the hyaloid artery, but I think he was mistaken in that, because the hyaloid artery always comes off

apparently from the lower part of the arteria centralis retinae. The upper branch of that comes off first, then the lower branch comes off, lower down, from the hyaloid artery, just at the point where these loops of vessels appear. I think that in development the hyaloid artery pulls this loop forwards into the vitreous, and it disappears in a loop behind. I saw a case a good many years ago at the Westminster Ophthalmic Hospital, in a male, who turns out to be the patient's younger brother; he suffers from the same condition, but it is rather more marked. It is the sort of variation one would expect to find in more than one member of a family, but I do not think it has previously been recorded in two brothers.

Coloured Vision.

By P. G. DOYNE, F.R.C.S.

(ABSTRACT.)

COLOURED vision is associated with a variety of conditions, both physical and pathological. Perhaps the red vision, which is induced by prolonged exposure to snow, or which may appear to persons who have had their cataractous lenses removed, is the most generally familiar form of this condition.

In the *Transactions of the Ophthalmological Society*, 1899, xix, p. 281, the late Mr. Work Dodd described a state of green vision which occurred in a man, the subject of tabes; and in the succeeding volume of the *Transactions* he collected thirteen cases of this condition. Of these thirteen cases, three were suffering from lead poisoning, three were myopic, one had a detachment of the retina, one had tabes, in one there was active choroiditis, two were the subjects of migraine, one had a wound of cornea with prolapse of the iris, and in one case no details were obtainable.

Mr. Collins has shown me some notes of his on coloured vision. He states that coloured vision may be associated with the following conditions: (1) Changes in the normal media of the eye; (2) after-images; (3) position in which the light impinges upon the eye; (4) pathological, retinal, and nerve irritation; (5) cortical disturbance; (6) functional.

It appears, therefore, that though this condition is not of very common occurrence, yet a very varied assortment of circumstances may be instrumental in producing it. During the last few months I have collected eleven cases of various forms of coloured vision.¹ Of these eleven cases, two of them had marked colloid body formation at the macula, three had optic atrophy and yielded a positive Wassermann reaction, two were myopic, one was aphakic, one had gross macular change probably hæmorrhagic in origin, and in one no obvious change was apparent.

The two cases in which there was the colloid body formation at the macula interested me most—the ophthalmoscopic appearance was so definite and the area of change so clearly defined. Yet this type of degeneration is not uncommon, and I am not aware that red vision is usually associated with it. In the first case the phenomenon was noticed when the patient awoke in the morning and at twilight—that is at times when a change in the stimulation of the retina was taking place. On the assumption that the colloid body formation interferes to some extent with the nutrition of the retina, is it possible to suppose that the retinal elements which are stimulated by red rays have a lower threshold?

¹ These cases will be reported in the *British Journal of Ophthalmology*.

DISCUSSION.

Mr. MAYOU commented on the fact that some of Mr. Doyne's cases were patients with only one eye. He himself had seen one such case, that of a taxicab driver who dare not take his cab out at night, since every lamp looked like a red ball, and therefore he was unable to distinguish between front and rear lights. Nothing abnormal was found in the eye, and after three or four months he had so far improved that he was able to resume work.

Mr. LESLIE PATON pointed out that the commonest type of coloured vision in tabetic patients was green, and thus Mr. Doyne's case of red vision was unusual. Some tabetics suffered from purple vision or a green meshwork in a purple field. Other cases, in which coloured vision with various formations was complained of, were those of temporary amaurosis associated with cerebral tumour; also cases of migraine.

Mr. FRANK JULER said that he had recently had under his care a medical man, upon whom he operated for extraction of cataract. There was good vision afterwards, but erythropsia came on five months later, which he described as purple rather than red vision.

Dr. JAMES TAYLOR, C.B.E. (President), said that in some cases of disseminated sclerosis with pallor of discs the patient said that in the twilight a pale blue mist came over the eyes. He had not been able to explain how that occurred.

Mr. R. AFFLECK GREEVES said that he had recently had under his care a lady with central cataracts, and he had given her weak atropine drops to use. She complained that when she went out of doors everything looked green, and when she returned home everything in the house looked red. She was so worried by this that she preferred not to use the drops.

Mr. LESLIE PATON, in some further remarks, said the suggestion offered in explanation of tabetic cases was, that there was a disturbance in the descending fibres, which originated in the cells of the external geniculate body, and which were supposed to be concerned with adaptation. When these were disturbed there was a very definite alteration in the length of time in which after-images persisted, and there was a distinct disturbance in adaptation for light and darkness, resulting from atrophy of these descending fibres, rather than of the ascending fibres.

Mr. HOLMES SPICER referred to cases of blue vision after cataract extraction, especially after a yellow cataract had been operated upon; also to erythropsia following retinal hæmorrhages and exposure to light reflected from a snow field.

Sir RICHARD R. CRUISE suggested that in patients of advancing age some arterio-sclerotic change which interfered with the function of the macula might be a responsible factor.

Mr. DOYNE (in reply) said that the patient in his tabetic case was very definite about the brilliancy of the red she saw. He gathered from the literature on the subject that there were two main groups; one in which the optic nerve was affected, such as the optic atrophy cases, and the other in which there seemed to be some local alteration in the retina itself, such as in the instances in which there were colloid bodies. He had been much struck by the descriptions of the patterns seen. His patient called it a blackberry. Mr. Work Dodd's patient described it as a flower pattern, and Mr. Paton's patient spoke of a mesh-work. As to the causation of erythropsia, what appealed to him was the explanation of Dr. Rivers, of Cambridge, who said that the erythropsia was due to hyperæmia of the retina. After exposure to strong light, an acute conjunctivitis came on after a latent period, and the same condition might occur in the retina itself.

Microscopical Section of a Series of Sympathizing Eyes examined microscopically.

By R. AFFLECK GREEVES, F.R.C.S.

(ABSTRACT.)

Case I.—Male, aged 60. Sympathizing eye began to be inflamed five weeks after operation; excised three months after first appearance of inflammation.

Case II.—Female, aged 49. Three months after operation of extraction, second eye affected; excision done five months after inflammation first appeared.

Case III.—Inflammation appeared in second eye two months after operation; excision five months later for secondary glaucoma.

Case IV.—Female, aged 54. Sympathizing inflammation in second eye three months; excision ten months later for secondary glaucoma.

Case V.—Female, aged 59. Inflammation in second eye five months after operation; excision eleven months later.

Case VI.—Male, aged 70. Second eye affected year after operation on first eye; excision a year later for secondary glaucoma.

Case VII.—Female, aged 64. Five months after operation inflammation in second eye which was excised three months later for secondary glaucoma.

Case VIII.—Female, aged 50. Inflammation appeared in second eye four years after operation; excised five months later.

Case IX.—Sympathizing eye excised eighteen months after original operation.

Case X.—Eye needled in childhood; sympathetic trouble had set in; twenty years later sympathizing eye excised.

In summarizing the appearances in this series of cases, I shall leave out the last case, as the period between onset and excision was out of all proportion to that in the other cases.

It is obvious that the particular character and extent of the inflammatory changes do not depend on duration; nor do they depend on the length of the period between injury to the exciting eye and the onset of trouble in the sympathizing eye. In four of the cases the infiltrating cells were purely lymphocytes and plasma cells; in five, epithelioid cells were present, and in three of the five a few giant cells. The only constant factor in all cases is an irido-cyclitis, which varies much in intensity and character. The choroid was normal posteriorly in four cases, and infiltrated in five, and in these five cases the infiltrating cells were lymphocytes and plasma cells; in one case only were there a few epithelioid cells. No giant cells were found in the choroid in any of the cases. The inflammation occurred in patches, which showed a tendency to be situated in the outer layers of the choroid, the capillary layer being free. This is the usual distribution in exciting eyes. The posterior part of the choroid was never more densely infiltrated than the anterior. In each case the vitreous showed some involvement. The optic nerve was normal in three cases, œdematous in five, in which the sheaths of the central vessels showed inflammatory infiltration. The nerve sheath showed infiltration in one case only, and in this case there was some infiltration round the perforating ciliary vessels.

An interesting point is that in connexion with the rupture of the lens

capsule, which occurred in three cases, in none of which, however, had there been any operative interference. In two of these cases the iris and the swelling lens matter together formed a necrotic mass, and in those cases the tension of the eye was low. In the third case there was the usual secondary glaucoma. Organized plastic exudation was present in four cases, and its undoubted presence in these cases does not support Fuchs' view that plastic exudation in sympathetic disease—invariably present in exciting eyes—is due to a secondary infection, and is not a part of the actual process.

If any conclusion can be drawn from a study of these cases, I think it must be that there is no special characteristic of the sympathetic process, as far as microscopical appearances are concerned, which is of constant occurrence in sympathizing eyes.

DISCUSSION.

Mr. M. S. MAYOU said in some cases, although Mr. Greeves' cases did not appear to show it, there was a well-marked deposit of fibrous tissue beneath the epithelium and Bowman's membrane containing new blood-vessels. The lantern slides which were shown on the screen were made from sections of a sympathizing eye in which both eyes had been removed for secondary glaucoma. Clinically the corneæ were opaque, and he thought that the origin of the fibrous tissue was probably organized fibrin due to bullous keratitis. Mr. Mayou also showed slides showing the condition of the choroid containing typical nodules consisting of giant cells, lymphocytes and plasma cells. He thought possibly that in Mr. Greeves' cases, which apparently were of long standing, the typical appearance had probably disappeared. In the cases described above the patient's eyelashes went white after the development of the sympathetic trouble.

Mr. AFFLECK GREEVES (in reply) said he was much interested in the section showing giant cells as well as epithelioid infiltration, as none of his own cases showed it, and in his cases he did not think there was any fibrin under the epithelium.

Section of Ophthalmology.

President — Dr. JAMES TAYLOR, C.B.E.

Mass partly obscuring the Optic Disc.

Exhibited by J. F. CUNNINGHAM, F.R.C.S.

Mrs. S. came to the Central London Ophthalmic Hospital about two months ago because she thought she had an occasional squint.

Right vision, $\frac{6}{36}$ not improved; left vision, $\frac{6}{6}$ Hm. + 0.75 S.

In the right fundus there is a greyish raised mass about half the diameter of the optic disc, which obscures the lower part of the disc; from it some brighter bands are seen passing upwards and curving in towards the disc. There are no vitreous opacities. The condition has not altered since it has been under observation.

Even under favourable conditions the mass at the lower part of the disc is very indefinite, and the conditions for seeing it to-night are much less favourable than usual. It is probably of congenital origin; the diagnosis appears to rest between that and a condition due to trauma.

Sir JOHN PARSONS (Chairman) said that the condition was more likely to be a congenital film, consisting of an excess of fibrous tissue on the disc, than ordinary retinitis proliferans.

Symmetrical Swellings in neighbourhood of Upper Lids near the Outer Canthus of each Eye.

By P. G. DOYNE, F.R.C.S.

PATIENT, a male, aged 66, is an inmate of Springfield Mental Hospital, and I am indebted to Dr. Reginald Worth for allowing me to show the case. There are symmetrical swellings towards the outer margin of the upper lid in both eyes. The man says they date from birth, though I do not know how far one can accept his statement. The history is that there has been no marked change in size since boyhood. At first I thought the swellings were dermoids, but afterwards I felt somewhat sceptical about that, because on pressure being exerted one gets a sensation of definite lobulation, which is rather like what pressure upon a lipoma would produce. Therefore I thought I would like the opinions of others.

The case was discussed by Mr. LAWFORD, Sir JOHN PARSONS (Chairman), and Mr. HEPBURN.

Mr. M. W. B. OLIVER, M.B., and Mr. T. JACKSON exhibited models to show the Method of making Prostheses.

Sir JOHN PARSONS (Chairman) said these models were extremely interesting; they had been brought to the pitch of a very fine art.

Retinal Detachment at Macula.

By H. ROWE JEREMY, F.R.C.S.

PATIENT, a female, aged 38, seen by me at the Western Ophthalmic Hospital on May 22, complaining that she had been unable to see well with the left eye for the past two years. When 8 years old she was struck in the left eye by a stone. The eye is painful at times. On examination the peripheral vision of the left eye was fingers at a foot. There was no central vision, V. R. $\frac{6}{6}$ J 1.

The left eye appears quiet: tension normal. There is no conjunctivitis or circum-corneal congestion. Cornea clear, left pupil larger than right; reacts sluggishly to light and accommodation; media clear. Fundus: disc normal.

There is a detachment of the retina at the macula, ovoid and regular in shape, 4 to 5 discs' breadth in width, surface smooth and greyish-white in colour. The edge is regular, and there is some choroidal pigmentation at the edge below. The surface of the swelling is best seen with + 8 to + 10 D. The vessels on the surface are not dilated. There is a faint shadow on transillumination.

The Wassermann reaction of the blood is negative.

Since I first saw her the detachment has appeared larger, and I have advised her to have the eye removed; but I should like to have other opinions before undertaking removal.

Sir JOHN PARSONS (Chairman) said the detachment was now extensive, and if it had started in the macula, loss of vision for two years was a long history. He remembered a case, years ago, under the late Mr. Marcus Gunn, with a tiny tumour in the macular region, which had manifested itself by a disturbance of central vision at a very early stage in the history of the growth. On examination, there was found, in the lower part of the fundus, a large simple detachment of retina, separated from the very shallow detachment just over the growth. He wondered whether in the present case there had originally been a detachment due to the growth in the macula, and whether the larger detachment was separate.

**Report on the Specimen submitted by Mr. H. Neame,
F.R.C.S., to the Pathological Committee.¹**

THE Committee have examined the specimen submitted to them and are unanimously of opinion that it is impossible to arrive at a definite conclusion as to whether the growth was primarily intra- or extra-ocular.

The majority of the Committee are inclined to the view that the growth was primarily intra-ocular. The sight remaining good for some time after the appearance of an epibulbar growth is not inconsistent with flat sarcoma of the choroid, of which cases have been reported of long duration without serious symptoms.

Flat sarcomata starting in and spreading along the lymph spaces cause almost uniform thickening of the uveal tract, as in this case. They are also specially prone to extend outside the globe along the perivascular spaces of such perforating vessels as they may encounter.

If in this case the growth had extended from without inwards one would

¹ See *Proceedings*, 1922, xv (Sect. Ophth.), p. 22.

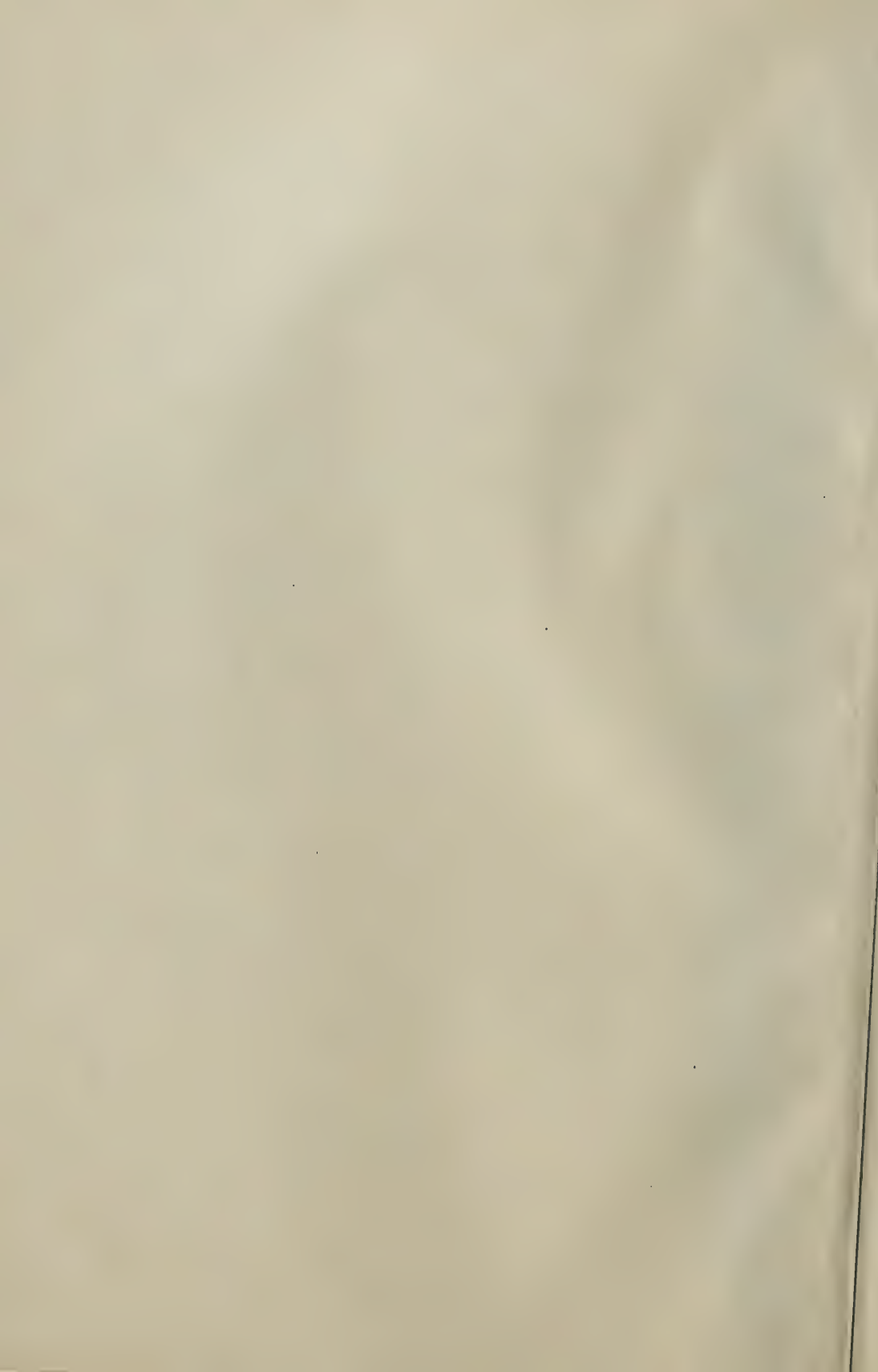
have expected localized enlargements at the sites of the perforating vessels within the globe.

The fact that the whole of the uveal tract is so extensively and evenly infiltrated they consider strong evidence that the growth was primarily intra-ocular.

One member of the Committee (E.T.C.), on the other hand, can find nothing to contra-indicate the view that the growth started in the lymphatic tissue of the conjunctiva at the limbus, and spread from that situation by centrifugal lymphatic permeation, in a similar way to that in which epibulbar epithelioma has been found to do. From the conjunctiva it may have permeated along the veins leading to the canal of Schlemm, and from it to the lymphatic spaces of the uveal tract, giving rise to a flat intra-ocular growth. Extra-ocularly it may have similarly extended into Tenon's capsule, and from it along the lymphatic channels of the perforating blood-vessels into the sclera.

Such a view seems to him more consistent with the clinical recorded observations, the growth having first attracted attention in the epibulbar region, and the failure of vision, and other intra-ocular symptoms, not having been observed until considerably later.

(Signed) E. TREACHER COLLINS.
W. T. LISTER.
M. S. MAYOU.
J. HERBERT PARSONS.



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Section of Otology.

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Section of Otology.

President—Dr. A. LOGAN TURNER.

PRESIDENT'S ADDRESS:

The Structural Type of the Mastoid Process, based upon the Skiagraphic Examination of 1,000 Crania of various Races of Mankind.

By A. LOGAN TURNER, M.D.

AFTER the publication of Arthur Cheatele's valuable anatomical work upon the pneumatic and infantile or acellular types of mastoid process, it might be regarded as superfluous for others to carry out an investigation which re-covered the ground already so thoroughly dealt with by him.

But the application of radiology to the study of the mastoid process upon an extensive scale has not hitherto received the attention of anatomists, though amongst otologists, Beck, of Chicago, has probably examined the largest number of mastoid processes in this way. Moreover, being provided with material which permitted of the examination of a number of races of men, it was possible to extend the research beyond anatomical boundaries and to wander into the realms of anthropology—always a fascinating study.

In conjunction with the late Major W. G. Porter, D.S.O., formerly a member of this Section, I commenced the investigation in 1913. Shortly before the outbreak of war, Dr. Porter had completed the radiography of 2,000 temporal bones in 1,000 crania. His enthusiasm and energy have made this paper possible. Unfortunately the war, the lamented death in action of Dr. Porter, and other reasons, prevented the completion of the research at an earlier date, and it was not until after the armistice that I had time to study the results which we had obtained and to formulate certain conclusions.

We were fortunate in having at our disposal the fine collection of crania in the Anatomical Museum of the University of Edinburgh, brought together by the anatomists—the Monros, Goodsir, Turner, and Cunningham,—furnishing examples of the skulls of all the races of mankind.

With this wealth of material we were able to investigate many points, some doubtless more of scientific than of clinical value, but all of interest because of the variety of the collection.

The *anatomical* points investigated were:—

(a) The determination of the relative frequency of the occurrence of the two types of mastoid process.

(b) The symmetry and asymmetry of the mastoid types.

(c) The types and their asymmetry in the two sexes.

[October 21, 1921.]

2 Turner: *The Structural Type of the Mastoid Process*

The points of *anthropological* interest were:—

(a) The relative frequency of the two types of mastoid process in dolichocephalic, mesaticephalic, and brachycephalic crania.

(b) The mastoid types as they occurred in the mixed or impure, and in the relatively pure, races of mankind.

And as one of us, several years before, had investigated the frontal air sinuses in many races, we attempted

(c) A comparison between the development of the mastoid and frontal air spaces in crania of a similar cephalic index and in the skulls of the same races of men.

It is to these points that I draw your attention to-day.

The conclusions which have been reached may briefly be summarized as follows:—

ANATOMICAL SUMMARY.

(1) It is possible to summarize the infantile and pneumatic types of mastoid process in the skiagram of the skull. A correct interpretation of what is seen in the living head is not always possible when non-stereoscopic pictures are employed.

(2) A mastoid process containing pneumatic spaces occurs, on an average, in 79 per cent. of European skulls, the persistent infantile or acellular process in the remaining 20 per cent.

(3) Anatomical asymmetry of the mastoid process in individual skulls occurs in 12 per cent. of a series of 1,000 crania. The asymmetry in the majority of instances is due to a pneumatic process on one side, and an infantile process on the other: in the remainder, both processes are pneumatic, but the cells are unequally distributed. Clinical asymmetry, therefore, is found in only 7 per cent. or 8 per cent. of the skulls.

(4) The relative frequency of the occurrence of the two types of mastoid process and the incidence of asymmetrical development is virtually the same in both male and female crania.

ANTHROPOLOGICAL SUMMARY.

(1) That the pneumatic and infantile mastoid processes occur generally in the skull types in a definite relationship: the percentage of pneumatic processes being lowest in the dolichocephalic crania, and highest in the brachycephalic, while they occupy an intermediate position in the mesaticephalic skulls. The reverse is true of the infantile processes in their relation to the long and short heads.

(2) That apparently there are certain racial exceptions to the above, possibly among the Esquimaux, and certainly in the Melanesian and Polynesian crania, in which the pneumatic processes greatly predominate, irrespective of the type of skull.

(3) That in the dolichocephalic and brachycephalic crania of the relatively pure races, there is a higher percentage of pneumatic mastoid processes than in the same types of skull in the mixed European peoples.

COMPARISON OF MASTOID PROCESS AND FRONTAL SINUS.

(1) That the "infantile type" or absent frontal sinus, like the infantile mastoid process, occurs more frequently in the dolichocephalic than in the brachycephalic crania, while it occupies an intermediate position in the

mesaticephalic skulls: or to put it conversely, the frontal sinus, like the pneumatic mastoid process, is found more often in brachycephalic than in dolichocephalic crania.

(2) That the frontal sinuses, both as regards their average height and breadth measurements and the frequency of their occurrence, are better represented in the crania of the mixed European peoples than in the skulls of the relatively pure races; the pneumatic mastoid processes, on the other hand, occur in a higher percentage of the crania of the relatively pure races.

(3) That there is no evidence, either in individual skulls or in groups of skulls, that the frontal sinuses and the pneumatic mastoid spaces attain a similar degree of development.

(4) That while the frontal sinuses attain a remarkable size in certain individual crania of the brachycephalic races, in no group of skulls can their development be regarded as assuming a racial characteristic, such as the pneumatic mastoid process appears to acquire in the dolichocephalic Melanesian and the brachycephalic Polynesian crania.

Posterior Ethmoidal Cell Exploration.

By P. WATSON-WILLIAMS, M.D., and E. WATSON-WILLIAMS,
M.C., M.B.

THIS paper is published *in extenso*, with illustrations, in the *Journal of Laryngology and Otology*, October, 1921, p. 464.

Case of Vertigo, due to Cholesteatoma of Attic, cured by Ossiculectomy.

By Sir JAMES DUNDAS-GRANT, K.B.E., M.D.

MRS. D., aged 40, schoolmistress, first seen April 27, 1910, complained of attacks of vertigo with loss of consciousness and occasional double vision, two or three times a month for several months. At 4 o'clock the previous morning she had an attack in which her arms became stiff for about a minute; this was followed by very loud snoring, and she was unconscious for about fifteen minutes; at noon she had a very light dinner followed by vomiting, and during the whole day suffered severely from headache. She occasionally lost the vision of the right eye for about two minutes, and had attacks of double vision two or three times a week. The ear had discharged with pus on and off since childhood.

On examination the left ear was found full of cerumen, and the hearing for the whisper was reduced to 3 in.; after this was removed she could hear at 2 ft., but there was found to be a large perforation in the attic through which desquamative material protruded; it was obvious that the cholesteatomatous material was being blocked up by the presence of the ossicles. Ossiculectomy was performed next day, and from that time she has been free from pain and any vertigo beyond an occasional slight "swimminess."

The PRESIDENT said it was very satisfactory that this patient should have been kept free from her attacks for so long. In this case the ossiculectomy had been justified.

A Pigeon Sixteen Months after a Single Application of Alcohol to the Membranous Labyrinth (left side).

By SYDNEY SCOTT, M.S.

WHEN the pigeon is placed on a perch it rotates the head in an antero-posterior axis with the vertex of the head to the left and downwards, so that the right eye looks to the left side and the left eye looks to the right. It will remain in this position for several minutes at a time, and then assumes the normal posture unless excited or disturbed.

DISCUSSION.

Dr. A. A. GRAY asked whether there was any loss of muscular tone in this pigeon. A theory had been propounded by Ewald that the labyrinth was the organ of muscular tone; but, as far as could be judged from the human subject, a lesion of the semi-circular canals did not produce loss of muscular tone.

Mr. J. F. O'MALLEY asked if Mr. Scott would indicate the method he employed in the experiment, and whether there were striking disturbances in equilibration immediately after the operation.

Mr. SYDNEY SCOTT (in reply to Mr. O'Malley) said the behaviour of the bird was now similar to that observed shortly after the injection of alcohol, and resembled that described by Flourens after his epoch-making discoveries ninety-seven years ago. In answer to Dr. Gray's question, he was unable to say whether there was any loss of muscle tone or not; he had not observed loss of tone in human beings in recent unilateral labyrinth lesions comparable to that which occurred in unilateral cerebellar disease, although some patients certainly had said they were conscious of a feeling of weakness in the extremities on the side of the labyrinthine lesion. He would be prepared to indicate method employed at the next meeting.

A Large Exostosis removed from the External Auditory Meatus of an Adult Male.

By H. LAWSON WHALE, F.R.C.S.

THE specimen is exhibited merely on account of its unusually large size. It measures $\frac{3}{8}$ in. in length and $\frac{3}{8}$ in. in each of the other diameters.

Case of Ossification of the Membrane around a Perforation.

By T. H. JUST, M.B.

THE patient, a woman aged 60, complains of deafness and tinnitus, especially in the left ear, which has been increasing for the last three years. There is no history of otorrhœa. In the left ear the membrane is scarred and shows a large, dry posterior perforation, around which is well-marked ossification. The right membrane shows a similar condition, but less advanced.

Tumour of External Auditory Meatus.

By T. H. JUST, M.B.

THE patient, aged 60, has noticed the swelling in the ear for six years, during which time it has steadily increased in size. The tumour causes deafness. The membrane is normal.

DISCUSSION.

Mr. ARCHER RYLAND said that on examining this case he had found it impossible to obtain any view of the tympanic membrane, on account of the situation of the tumour. It was obvious, however, that the meatus contained a quantity of moist yellow secretion. Was it not possible, therefore, that this case might be one of chronic suppurative otitis media, and that the tragal condition might be an irritative lesion?

Mr. JUST replied that there was a good deal of secretion. He had not seen the patient for a fortnight, but when she was last seen and when the secretion was syringed out the membrane was normal, and the secretion was apparently banked up behind the tumour.

Result of Double Schwartze Operation.

By LIONEL COLLEDGE, F.R.C.S.

PATIENT, a girl, aged 6. December 1, 1920: Schwartze operation on left mastoid for acute suppuration. December 4, 1920: Similar operation on right ear.

The perforation in the tympanic membrane has healed in both ears, and all suppuration has ceased. The wound in the right mastoid has filled up and healed in the normal way, but a large dry cavity persists in the left mastoid. Both sides were treated in the same manner.

DISCUSSION.

Dr. P. MACDONALD said he had had a very similar case, operation upon which he had almost completed; one side healed up, but on the other a fistula remained through which one could pass a comparatively large probe, and he had thought of doing a plastic operation if it persisted. The wound was first packed with gauze for a few days, and later the gauze was omitted, the cavity being mopped out twice a day. The posterior wall of the osseous meatus was extensively removed. The lining of the cavity was not skin, but the ordinary mastoid cell lining.

Mr. SYDNEY SCOTT raised the question as to the method of drainage after the operation. He had never seen a permanent opening, and he wondered whether the operator had employed the gauze pack. He always employed tube drainage with partial closure by suture and had discarded the gauze pack for ordinary Schwartze operations years ago.

Sir JAMES DUNDAS-GRANT called attention to a paper by Panse on "Artificial Cholesteatoma,"¹ which was likely to take place in such a case as Dr. Macdonald mentioned, in which epidermization extended from without inwards and left a fistula. Desquamation then took place in the interior. Heath's operation left an opening in the form of a fistula in the meatus, instead of on the outside. Removal of the posterior osseous wall of the meatus without perforating the membranous part would allow of collapse of the soft parts into the mastoid cavity.

¹ *Archiv. f. Ohrenheilk.* 1892-93, xxxiv, p. 251.

Mr. G. J. JENKINS said that in this case the cavity was partly lined with mucous membrane, almost to the surface; and it seemed to him that the mucous membrane had possibly not been completely removed from the antrum at the time of the operation. If there happened to be a large cavity, the mucous membrane might grow rapidly and the granulations might not be sufficient at the outer part of the cavity to come together before the mucous membrane and skin met.

Extra-dural Abscess with Extensive Sloughing of Dura.

By H. BUCKLAND JONES, M.B.

MALE, aged 25. History of polypus removed from left ear.

August 5, 1920: Slight discharge from left ear with deafness.

April 19, 1921: Severe headaches, tenderness over mastoid, looks very ill; temperature 99.2° F. Operation advised but refused.

May 11, 1921: Admitted collapsed. Mastoid operation, antral tegmen necrosed; large quantity of stinking fluid and pus. Middle fossa exposed; extensive sloughing of dura. Lumbar puncture, cerebro-spinal fluid clear and sterile. Much improvement after operation; for few days temperature normal.

May 18, 1921: Slight rise of temperature; headaches. Opening extended anteriorly, temporal muscle divided: slough 4 in. by 1½ in. exposed. Vaccine of *Bacillus coli communis* and streptococcus. Wound kept open seven weeks; slough entirely separated in five to six weeks.

October 7, 1921: Small granulations, slight discharge present. Hearing improving. Slight headaches, general condition good.

Mr. SYDNEY SCOTT said that Mr. Buckland Jones's case of sloughing dura reminded him of one which had occurred in his own practice. A gentleman of 40 years of age had had acute suppurative otitis media with abundant discharge from the ear for four weeks. He stated that he did not feel ill and wished to see to his business affairs, but he had nevertheless had mastoid tenderness and had looked ill. The mastoid had been opened without delay and found in a crumbling state, and full of pus. The sigmoid sinus was bathed in pus, and the dura mater on the mesial side was in such a sloughing condition that it had actually given way beneath the tip of the finger. What astonished the speaker was that clear cerebro-spinal fluid simply poured out like water from a tap the moment the finger was removed; probably 5 oz. poured out (not oozed or dribbled). On inspecting the opening in the sloughing dura, more clear cerebro-spinal fluid could be seen separating the cerebellum from the dura mater. His former experience had always been in cases of incision or perforation of the dura, that the brain cortex immediately occluded the opening. In this case the brain was fully ¾ in. or about 2 cm. away from the sigmoid sinus and adjacent dura. Cerebro-spinal fluid continued to ooze so rapidly after the operation that towels were saturated and the patient died in sixteen hours without regaining consciousness. The only parallel case he could find recorded, was one mentioned by Sir William Macewen, although in that case the lesion was in the parietal region.

Epithelioma of Auricle.

By LIONEL COLLEDGE, F.R.C.S.

PREVIOUS history: Six weeks before admission patient noticed a small pimple on the right auricle. He scratched this and an ulcer formed, which has steadily increased in size. He was treated in out-patient department for seven days previous to admission, with hot fomentations locally and haust.

hydrarg. pot. iodide internally. On admission: There was an ulcer about the size of a sixpenny piece on right auricle; floor granulating and covered with purulent discharge; edges surrounding skin hyperæmic and sharply cut; whole auricle was swollen and tender. There were small hard glands over the mastoid process and in the posterior triangle. Wassermann reaction negative.

Under a general anæsthetic the ulcer was scraped: the cartilage was necrosed in the floor of the ulcer; the edges were undermined and friable. Piece removed for examination. Treated with dilute solution of formalin.

Pathological Report.—Epithelioma with typical cell-nests and small rounded-celled infiltration.

DISCUSSION.

Mr. NORMAN PATTERSON spoke of a case of epithelioma of the auricle which he had recently shown, and which was well nine years after the operation. In this case there was a large mass of glands high up in the neck. If he had the present case to deal with he would adopt the same course as he had adopted in his own, namely—remove the auricle, expose the lateral sinus, and occlude the sinus by a gauze plug inserted between the sinus wall and the skull. If this were done there would be no fear of serious hæmorrhage from the jugular vein should it be accidentally injured.

Mr. WOODMAN spoke of the extraordinary way in which epitheliomata of the ear seemed to traverse the meatus. Ten days ago he had had a case of epithelioma of the ear which had been unsuspected even at the operation. Apparently it commenced in the skin of the external meatus; it was an ordinary epithelioma with cell-nests. The woman came with facial paralysis and outpouring of pus; the drum had gone, and pus was welling up all the time. On opening the mastoid he found the mastoid bone was necrosed, and the whole bone was picked out with forceps. The lateral sinus was patent, the walls thick and yellow, and the dura mater also. As the patient's condition was very bad under the anæsthetic he desisted, and the woman died suddenly two days later. The post-mortem examination showed that an ordinary squamous-celled epithelioma had grown into the temporo-sphenoidal lobe, to which the dura mater was adherent, and it must have come from the skin of the meatus, or from the auricle. He had had other cases in which epithelioma of the auricle had tended to grow upwards and had almost escaped notice. With regard to the operative treatment of the present case, the whole auricle must be ablated. He would first split the neck from end to end, dissect all the glands from the neck, tie the jugular in the neck and the external carotid, remove all glands, ablate the auricle and a large part of the temporal bone, as he thought recurrence would be likely in the deepest part of the skin attachment of the auricle.

Case of Unilateral Deafness with Anæsthesia of the same Side of the Face.

By Sir JAMES DUNDAS-GRANT, K.B.E., M.D., and
C. C. WORSTER-DROUGHT, M.D.

A WOMAN, aged 37, complained of dullness of hearing of the right ear and numbness of the right side of the face, of gradual onset and of about three months' duration. This was preceded by headaches which are not now present. The tests for hearing showed almost complete deafness on the right side (checked by the noise machine and other tests); there was, however, slight preservation of hearing for hissing sounds. There was total absence of nystagmus or vertigo by the cold labyrinth test on the right side, both for the vertical and horizontal canals; on the left side, however, they were

8 Dundas-Grant and Worster-Drought : *Unilateral Deafness*

actively induced. The galvanic reaction was also absent on the right side. These tests point to a lesion involving the trunk of the auditory nerve internal to the labyrinth.

The numbness of the right side of the face was mainly observable in the area of the second and third divisions of the fifth nerve. Sensation appeared to be diminished on the outer wall of the right nasal cavity, but was normal in the mouth, while taste was not impaired. The movements of the palate and vocal cords were normal; there was, therefore, no involvement of the vagus. The Wassermann reaction was negative and there was no optic neuritis. The evidence was, therefore, that of neuritis of the eighth nerve and of part of the fifth.

The inactivity of the horizontal canal on one side and of the vertical on the other, which would have suggested cerebello-pontine angle tumour, is not present in this case.

An X-ray examination will be made in due course and the further progress will be reported.

Section of Otology.

President—Dr. A. LOGAN TURNER.

Some Observations on the Early Diagnosis and Drainage of Otitic Meningitis, illustrated by Fourteen Cases and Specimens.

By E. D. D. DAVIS, F.R.C.S.

THESE observations are limited to the diagnosis and drainage of fourteen cases of otitic meningitis of the posterior fossa of the skull of which I have full notes. All the cases were fatal, and post-mortem examinations were made in twelve. Purulent meningitis of the posterior fossa of the skull was found in every post-mortem with large collections of thick gelatinous pus between the tentorium and the cerebellum, also in the cisterna interpeduncularis surrounding the optic chiasma and pituitary fossa. In advanced cases the meningitis was beginning to spread along the Sylvian fissures to the cerebral cortex. The cisterna magna in some cases was free from pus, and a collection of pus in this region was not a marked feature in any case.

The path of infection in eight cases was traced through the fenestra ovalis to the labyrinth and to the internal auditory meatus, then to the under surface of the pons or to the interval between the tentorium and cerebellum. In one case a collection of pus was found at the aqueduct of the vestibule and appeared to reach this position along the saccus endolymphaticus. Collections of pus were more commonly seen at the internal auditory meatus. Fistulae of the external semicircular canal or of the promontory were not discovered. This post-mortem evidence, showing that the path of infection was through the labyrinth, indicated that symptoms of suppurative labyrinthitis should precede meningitis and in six of the cases there were definite symptoms of labyrinthine inflammation which preceded the characteristic signs of an established meningitis. The post-mortem evidence of a collection of pus at the internal auditory meatus and at the aqueduct of the vestibule also demonstrated that the most direct route of drainage was through the labyrinth and internal auditory meatus, or through the inner wall of the mastoid cavity in front of the lateral sinus to drain the inner surface of the petrous bone.

In no disease is an early diagnosis of greater value than in a case of threatened meningitis. It has long been recognized that an established case of otitic meningitis with its severe headache, vomiting, temperature, and pus cells in the cerebro-spinal fluid is hopeless and operation useless, but even in such a case, drainage of the skull and lumbar puncture should be done as it is the only method of relieving the agonizing headache and distressful symptoms of the patient, who is often quite conscious to within a few hours of death.

The labyrinthine symptoms which preceded meningitis were those due to irritation of the labyrinth followed by loss of function and destruction. The irritative symptoms were vertigo and nystagmus and, in one case, severe tinnitus.

The patient looks ill, with a strained expression indicating headache and pain. The temperature is raised to 99° F., or may be normal.

The *cerebro-spinal* fluid at this stage *may be normal*, but more usually there is a polymorphonuclear leucocytosis, which however is nearly always sterile.

When severe headache, vomiting, neck rigidity—the symptoms of manifest meningitis—appear, then the *cerebro-spinal* fluid is turbid and contains pus cells, but it is now too late to save the patient. In two cases the normal *cerebro-spinal* fluid produced a false sense of security, but any excess of leucocytes in the fluid should be a grave warning. A leucocyte count should be done immediately and no time wasted in looking for organisms in the *cerebro-spinal* fluid, which is frequently sterile. The tests by litmus paper for acidity and for the reduction of Fehling's solution were carried out and found to be unreliable. Fehling's solution was reduced by the purulent fluid of one case of pneumococcal meningitis arising from otitis media and in two other cases of probable mixed infection.

It is in the first stage of labyrinthine symptoms that labyrinthotomy is likely to be successful and several such cases have been recorded. If the labyrinth is defunct with complete loss of hearing, then there need be no hesitation in draining the labyrinth, but this decision is not so easily made during the irritation stage of the labyrinth because it is known that such symptoms may disappear with the performance of the mastoid operation.

In three of the above cases a fulminating meningitis followed—four days, eleven days, and three weeks respectively—after a simple and satisfactory mastoid operation for acute suppuration, and it is in this type of case that it is difficult to decide on labyrinthotomy before the onset of meningitis.

Labyrinthotomy is not a severe or difficult operation if the middle ear is well exposed by reducing as much as possible the facial spur, particularly its anterior edge, and by detaching the auricle and the cartilaginous meatus downwards and forwards. When the promontory is thus clearly exposed, one tap of a small chisel on the bridge of bone between the fenestra ovalis and fenestra rotunda removes the promontory and opens the labyrinth.

If signs of threatened meningitis have developed, drainage through the internal auditory meatus is obtained by firmly thrusting the narrow end of a Ballance's mastoid curette inwards and slightly forwards through the fenestra ovalis. The concavity of the curette should be directed forwards, and the fenestra must be seen, otherwise it is quite easy to pierce the carotid canal, which lies immediately in front of the promontory and is covered by a thin plate of bone. Occasionally the bone is so firm that it is difficult to force a curette through into the internal auditory meatus, and the specimens of temporal bone exhibited clearly show these points.

If there is not a free flow of *cerebro-spinal* fluid, a bad prognostic sign if the internal meatus has been opened, further drainage of the skull can be carried out through Trautmann's triangle, i.e., through the inner plate of the mastoid process in front of the lateral sinus and immediately behind the labyrinth. There are some small details of technique in this procedure which experience has shown must be observed, i.e., the lateral sinus should be exposed, then the bone between it and the facial spur and *below* the mastoid antrum removed. If the opening is made in the mastoid antrum or too high up, the superior petrosal sinus is injured, and this has been done in one of the temporal bones exhibited. The disadvantage of this method of drainage is the small area of operation at considerable depth, and the pressure of the inserted drainage tube on the lateral sinus may produce thrombosis. In the first specimen it can be

seen that the drainage tube, by its position, is directed to the under surface of the cerebellar hemisphere on a level with the flocculus and away from the cisterna magna.

Drainage through the internal auditory meatus and in front of the lateral sinus has proved more satisfactory in the above cases where the diagnosis is clearly established than the more formidable and dangerous craniectomy of the posterior fossa, because the original focus of suppuration in the mastoid and internal ear can be dealt with, and it gives more direct access to the collection of pus at the internal auditory meatus and to the area between the tentorium and cerebellum.

In conclusion, the following procedure is suggested:—

(1) Given a case of acute and virulent streptococcal suppuration of the middle ear and mastoid, with symptoms of labyrinthine irritation, a lumbar puncture should be performed first, and at the same sitting the drum incised and a simple mastoid operation done.

(2) If the labyrinthine symptoms do not subside in forty-eight hours, and the cerebro-spinal fluid shows a leucocytosis, then the radical mastoid operation, a simple labyrinthotomy through the promontory, and a second lumbar puncture should be carried out.

(3) Failing improvement, and if signs of meningeal infection develop, then the posterior fossa is to be drained through the internal auditory meatus and in front of the lateral sinus.

SPECIMENS EXHIBITED.

No. 1.—Temporal Bone from a Case of Meningitis occurring Eleven Days after a Simple Mastoid Operation: (a) Mastoid curette passed through the fenestra ovalis to internal auditory meatus; (b) drainage tube passed through opening made in front of lateral sinus; (c) this tube impinged on the cerebellar hemisphere on a level with the flocculus and its direction is away from the cisterna magna.

No. 2.—Temporal Bone from a Case of Fulminating Meningitis resulting from Acute Otitis Media of Three Days' Duration: (a) Promontory removed to open labyrinth; (b) pin passed into internal auditory meatus: a mastoid curette could not be forced through to the internal auditory meatus; (c) drainage tube inserted through inner wall of mastoid antrum to show injury to superior petrosal sinus: the antrum is nearly always too high for such drainage; (d) middle fossa dura extensively opened, but no meningitis of the middle fossa was found.

No. 3.—Temporal Bone to show Relation of the Promontory to the Carotid Canal.

DISCUSSION.

Mr. LAWSON WHALE said he had had three similar cases, all fatal. Every one found difficulty in draining through the tube. He had used a spiral silver wire drain instead of rubber. He now used two tubes, the total calibre of which equalled the diameter required, so that if one should become blocked the other would probably still act. They were sewn together side to side, so that on section they looked like the figure 8. With regard to the use of a scoop for the internal meatus, an instrument which was less liable to do unnecessary injury was the Alexander Eustachian-tube rasp, which was square on section.

Mr. G. J. JENKINS said the operative procedure in these cases had been carefully described by Mr. West and Mr. Scott, who were the originators of the operation. There was much to say about the recognition of early meningitis, but he wished to express his

disapproval of repeated lumbar punctures or the taking away of much fluid in cases in which the diagnosis of meningitis of labyrinthine origin had been made; he thought it dangerous, as a means of diffusing infection through the posterior fossa. He did not agree that if the fluid appeared to be clear the case should be regarded as one which could be treated by simple mastoid operation. There were other certain clinical signs of importance. The fluid should be examined at once and a report given before the operative procedures were carried further. If polymorphs were found, it indicated meningitis.

Remarks on the Comparative Effects, immediate and remote, of introducing Absolute Alcohol into the Labyrinth of Birds and Human Subjects (Cinematograph Demonstration).

By SYDNEY SCOTT, M.S.

(SYNOPSIS.)

MR. SCOTT described some investigations on the labyrinth of pigeons which he had carried out a little more than two years ago. In connexion with aviation during the War the importance of the labyrinth came into prominence. It was claimed that the functions of the semicircular canals should be tested in all applicants for commissions in the Air Force. Different persons reacted in different degree to stimulation by rotation, &c., but the clinical method of hyper-stimulation was employed to detect the presence of a functioning labyrinth. Only by the total absence of reactions could it be inferred that the labyrinths had been destroyed, or were functionless. He had destroyed the labyrinth by means of absolute alcohol, five years ago, in a patient who had had an ordinary fistula of the external semicircular canal, caused by otitis media. A radical mastoid operation had already been performed two years before he first saw the patient—a young woman—who was then subject to persistent recurrent vertigo. At that time the labyrinthine reactions were normal, but when pressure to a spot in the region of the external semicircular canal was lightly applied with a probe, it produced the usual phenomena associated with a bony fistula or sinus of the canal; suction applied to the stapes also gave rise to the same phenomena.

After the injection of absolute alcohol through the "fistula" into the semicircular canal the patient had immediately presented the signs associated with recent destruction of the labyrinth, from the symptoms of which she had now long since recovered. She still showed the typical signs of defunct labyrinth but only in response to the caloric and rotatory tests. At first there had been severe facial paralysis, perhaps because too much alcohol had been introduced. In other cases when alcohol had been applied to render the labyrinth functionless, the effects had been the same, without the production of facial paralysis, possibly because steps had been taken to avoid any excess of the reagent remaining in the cavity.

The same method had been adopted in order to destroy the labyrinth of the pigeons, the alcohol being injected through the tympanic membrane and base of the columella into the vestibule. This injection had been painlessly carried out under general anæsthesia. The evidence of defunction was the immediately altered behaviour of the birds. The cinematograph records showed this altered behaviour in birds in which only one labyrinth had been destroyed and in one which had survived destruction of both labyrinths for nine months.

Whilst human subjects recovered in a few months so that they could run or turn, jump or dance, &c., birds exhibited very indifferent powers of adaptation to unilateral and bilateral labyrinthine loss. In evidence he (Mr. Scott) recalled Mr. Richard Lake's case reported in the early *Proceedings*¹ of the Section, and, of course, others of bilateral labyrinthotomy had been observed since, from time to time. As regards the birds, the altered attitude of the head and body was like that which Flourens had first observed in 1828.

PIGEONS.

(1) *Attitude assumed.*

Left labyrinth defunct.

Stands with head slightly or distinctly flexed to the left. When disturbed the head rotates suddenly with the vertex down and to the left until completely inverted. At the same time the head is held near the ground, and the bird generally moves backwards in short quick steps, circus-wise to the left.

Both labyrinths defunct.

Stands with feet wide apart and head hyper-extended, with beak pointing directly upwards. Suddenly the head may be flexed forwards until the beak meets the ground, and then, as suddenly, the head is elevated and flexed backwards until the beak rests on the bird's back. These "see-saw" movements were more obvious at first than after some months, but they were observable at the ninth month, that was the full period of survival.

(2) *Behaviour in Cage.*

Never leaves floor of cage or attempts to hop on to perch which was 18 in. high, but after twelve months was generally found on a new perch which had been put only 6 in. from floor of cage.

Quite soon was able to fly on to perch 18 in. high, although it exhibited difficulty in balancing in antero-posterior plane.

(3) *Flight.*

Generally avoided, even when released, but on the few occasions the bird rose and swept to the left, descending quickly in a spiral, resembling a rapid spinning nose dive.

Never flew more than a few yards, and always overbalanced forwards on landing.

(4) *Feeding.*

For several months the birds were hand-fed, but had gradually become independent and fed themselves.

Left labyrinth defunct.

Tends to peck at grain sideways with beak horizontal, pecking towards the right.

Both labyrinths defunct.

Misses the grain by always pecking about half an inch in front of each grain of corn, when separately scattered. It was necessary to keep a supply in a vessel full of grain.

(5) *Effect of Obscuring Vision by covering One or Both Eyes with a Hood.*(a) *Both Eyes covered.*

Left labyrinth defunct.

Stands still, head low, left eye down: walks cautiously circus-wise, turning to the left when repeatedly touched.

Both labyrinths defunct.

Remains motionless in whatever position it is placed for an indefinite time. For instance, will remain on back, or on side with head doubled up beneath body. Will stand on feet with beak touching ground, and may then, on being touched, suddenly elevate beak and hyper-extend (backwards flexed) neck till vertex rests on the back and tail touches ground.

¹ *Proceedings*, 1907-8, i, p. 150.

(b) *Right Eye only covered.*

Left labyrinth defunct.

Walks slowly circus-wise to the left. When held off the ground in the hand, both wings being free to move, the right wing flaps strongly, making complete excursions of 180°, whereas the left wing moves feebly through only about 90°.

Both labyrinths defunct.

Stands with feet wide spread, beak pointing skywards. Rotates on vertical axis to the left.

(c) *Left Eye only covered.*

Stands with right eye turned upwards; feet wide apart, right leg abducted, and weight of body on left leg. Rotates faster and faster to the left.

Again stands with feet wide spread and beak pointing skywards. Rotates faster and faster when touched, as if pivoting on vertical axis of beak, turning to the right.

When the unilateral "defunct" bird, standing with both eyes covered, was gently pushed sideways with the hand, it resisted the pressure much more strongly when pushed to the right than when pushed to the left; this was well seen in the cinematograph records. The bilateral "defunct" bird showed no difference in resistance to lateral pressure, nor was it startled by explosive noises like the other birds were, whether the eyes were uncovered or not.

It was not intended to draw deductions too closely from these observations in relation to aviation problems. The demonstration merely showed that absolute alcohol produced certain definite effects which, it was inferred, were the result of destruction of the otic labyrinth as a whole, and that in the case of birds the power of adaptation to labyrinth loss was very inferior to that possessed by human beings.

DISCUSSION.

Dr. URBAN PRITCHARD said Dr. Gray had confirmed macroscopically what he (Dr. Pritchard) had shown microscopically, that birds had an enormous area of nerve cells, and the cilia had the curiously wavy contour which was seen particularly on the crista of the semicircular canal. Hence it could be understood that when this was destroyed birds were much more at a loss than were human beings who had a similar loss.

Mr. G. J. JENKINS said that in some cases the human being did not completely recover control if both labyrinths had been destroyed. He had shown that day a man—obviously of low intelligence—who had had both labyrinths destroyed ten years ago, and he had not yet re-established control. Lately he had operated upon a case of meningitis, and the patient seemed likely to recover. He had already operated on the same man for meningitis infected from the other ear last June. He had now lost the function of both labyrinths, and it would be interesting for members to see him a short time before re-education methods had begun and again six months afterwards.

The PRESIDENT said he had been interested in having seen Mr. Lake's case of double complete destruction of labyrinth, as the patient had since become a ward-maid in the Edinburgh Royal Infirmary, where opportunities were obtained of seeing her from time to time.

A Short Account (Demonstration) of the Research Work being conducted at Utrecht on the Saccular, Utricular, and Allied Reflexes.

By A. R. TWEEDIE, F.R.C.S.

MR. TWEEDIE said that he had followed the recent literature on the subject—like other members of the Section—but as he had found difficulty in grasping the terminology and description of various experiments published, he had lately visited Utrecht for the purpose of endeavouring to understand these phenomena.

The experimental work was being carried out at the Pharmacological Institute by Professor Magnus and Dr. de Kleyn, and in the Aural Clinic of Professor Quix; it was further controlled in Professor Winkler's Neurological Clinic at Utrecht, and supported by investigations under Professor Kappers and Dr. Brouwer in the Institute for Brain Research, Amsterdam.

He drew attention to the fine work of Högyes of Budapest in 1881, work to which, via the Vienna school, we were chiefly indebted for the original research into the functions of the semicircular canals. Whilst this effort emphasized the kinetic factor in the auditory apparatus, it had remained for workers at Utrecht to point out that it also possessed a static function, due to the presidency of the otoliths over position in space.

A demonstration was first given of certain further reflexes of the semicircular canals, incidentally noted in connexion with this research ("the lift," "muscle-tremor," "toe spreading," and "springing" reflexes). It had been suggested that these reflexes were dependent on the influence of the otoliths, but by the ingenious method of Wittmaack, who discovered that after extreme centrifugization the maculæ of the otoliths were rendered functionless, whilst the ampullæ of the semicircular canals remained unaffected, these could no longer be considered dependent on otolithic influence. On the other hand, as these reflexes disappeared after total labyrinthectomy, it was fair to conclude that they were dependent on the normal function of the semicircular canals.

The method of centrifugization was then described. This was followed by an account and demonstration of the "neck" reflexes; these were dependent on the integrity of the first, second, and sometimes the third cervical nerves. All the "neck" reflexes (as, indeed, the otolithic reflexes also) were "tonic," i.e., they persisted so long as the stimulus causing them persisted. The effect on the limbs and movements of the eyes were then demonstrated in the rabbit. It was necessary to recognize these "neck" reflexes before considering the tonic reflexes of the otoliths, as the two were intimately associated, and it was more convenient to describe the "neck" reflexes first.

(The account of the otoliths and their reflexes was commenced, but owing to the pressure of time was postponed to the next meeting of the Section.)

The Course and Relations of Arnold's Nerve (Auricular Branch of the Vagus) in the Temporal Bone.

By ALBERT A. GRAY, M.D.

IN the *Journal of Anatomy and Physiology* (xlvii, p. 391) I published a short paper on the "Comparative Anatomy of the Middle Ear." Among

other subjects in that paper I demonstrated the existence of a large plexus of nerves on the posterior surface of the tympanic bulla in several mammals of different orders. On account of its anatomical position I named the structure the bullar plexus. This plexus was clearly composed of branches from at least two nerves, the facial and the vagus; and probably branches from the glosso-pharyngeal also took part in its formation. The existence of this structure in several mammals of different orders appeared to me to indicate that it was probably represented in man, but had hitherto escaped discovery on account of the difficulties of making satisfactory dissection of that region in the human subject. This conjecture has proved to be justified, and with the help of lantern slides I hope to be able to demonstrate the existence of this plexus in man, though it is a much smaller and more elusive object in him than in the other mammals referred to previously.

Before going on to demonstrate the plexus itself, it is important to refer briefly to the striking anatomical differences in this region between man and the anthropoid apes on the one hand, and all other mammals on the other hand.

In the first place, the mastoid process exists only in man and the anthropoid apes. The tympanic bulla on the other hand in these animals is reduced to an insignificant little cul-de-sac lying below and behind the tendon of the stapedius muscle, and termed in the human subject the tympanic sinus. In all other mammals the state of matters is, so to speak, reversed—that is to say, the mastoid process does not exist and the bulla is a large cavity: in many mammals indeed it is considerably larger than the rest of the middle-ear cavity.

It must further be pointed out that the course of the facial nerve in this region is external to the bulla, but internal to the mastoid process. Consequently the development of the mastoid process in the anthropoid apes and man, associated with the retrograde evolution of the bulla, has produced a considerable change in the anatomical relationships of the facial nerve and adjacent structures. Thus the facial nerve assumes a much more nearly vertical position before it escapes from the base of the skull. The stapedius muscle also assumes a lower position, being no longer found above the bend of the facial nerve, but now lying below that bend and partially under cover of it. Similarly the bullar plexus is found at a relatively lower level, and incidentally it may be noted that it has become smaller in size and less complex. The bullar plexus indeed in the human subject consists of the communications of Arnold's nerve (the auricular branch of the vagus) with the facial nerve, and in some cases at least with the chorda tympani.

As the result of investigations carried out by myself in regard to the mutual relationships of Arnold's nerve, the facial nerve and the chorda tympani, differ considerably from the descriptions given in standard anatomical works such as Quain, Spalteholz and Gray, I propose to give the results of these researches in the following short note. These results so far as they have gone are of the nature of a destructive criticism of the teaching at present accepted rather than of a full description of the actual relationships. The reason for this is that the course of Arnold's nerve varies considerably in different subjects; and, owing to the small number of specimens examined, it is not possible to say definitely what is to be considered the typical relationship, and what is to be considered a variation. The recent development of the mastoid process in mammalian evolution, which has just been referred to, is doubtless the reason why these variations are so common in this region.

The first specimen examined shows the following facts as regards the course of Arnold's nerve. The nerve enters the temporal bone on the external

surface of the jugular fossa as ordinarily described, and runs horizontally outwards and backwards until it reaches the facial nerve. At that point a small twig unites it with the facial nerve. Turning round the posterior aspect of the facial nerve it continues outward for a very short distance (about 1 mm.) and at the same time bends forwards and a little downwards, until it comes to be immediately behind the chorda tympani. At this point these two nerves are in close actual contact, and probably nerve-fibres pass from one to the other, though of course this could only be definitely settled by microscopic examination. After this junction Arnold's nerve turns more abruptly downwards until it is almost vertical. It continues downwards and leaves the bone through a small foramen a millimetre or two external to the stylo-mastoid foramen. It then passes outwards along the lower surface of the temporal bone for a distance of one or two millimetres until it reaches the cleft between the mastoid process and the bony rim of the external auditory meatus. Its course is then almost vertically upwards. In some specimens this vertical portion lies in the cleft just mentioned, but in others the nerve re-enters the temporal bone through a foramen which leads into a minute but long canal. This canal in its upper portion turns inwards and opens on the posterior wall of the meatus; and the nerve then terminates on the posterior wall of the meatus and on the tympanic membrane as described in anatomical text-books.

A second type in regard to the course of Arnold's nerve is found in which the nerve passes outwards along the lower surface of the temporal bone without apparently penetrating the bone. That is to say it runs outwards and crosses behind the facial nerve just as the latter leaves the stylo-mastoid foramen. At this point it gives off a branch to the facial nerve and also receives one from the latter; the second of these branches leaves the facial nerve at the same point as does the chorda tympani, and it may be that it actually does come from the chorda tympani. In this particular respect, therefore, the relationship of the chorda tympani and Arnold's nerve would nearly coincide with that found in the first type, the only difference being that the connexion between the chorda tympani and Arnold's nerve occurs lower down and quite close to the origin of the former.

Finally, in a third type, Arnold's nerve appears to arise direct from the chorda tympani by two twigs which unite immediately after leaving the latter to form one nerve which runs first outwards and then turns downwards to escape at the foramen mentioned when describing the first type. It may be, however, that in this third type another portion of Arnold's nerve may exist which may have been broken away in making the preparation from which the above description has been taken.

CONCLUSIONS.

The present investigation can only be looked upon as a preliminary survey of the region to be explored; but, as shown above, a sufficient number of facts have come to light which indicate clearly enough that the description of the course and relations of Arnold's nerve as given in the standard anatomical text-books is quite inadequate and probably quite incorrect. It is evident, further, that considerable variations occur in the course and relationships of this nerve. On account of the small number of cases examined it is not yet possible to say which is the most common type and which are to be regarded as variations from that type.

DISCUSSION.

Mr. G. J. JENKINS knew from experience how difficult it was to follow out small nerves in such structures as were concerned here. He had seen it stated that Arnold's nerve did come out by a separate foramen in some cases. But he wondered whether Dr. Gray had considered the application of his findings to the segmentation of the skull.

Mr. J. F. O'MALLEY asked as to the course of the auricular branch of the vagus; it seemed to take a downward and then an upward direction, and the chorda tympani the same route. Did Dr. Gray attribute that to developmental changes in the mastoid process?

Dr. GRAY (in reply) said he was not versed in the subject of segmentation in regard to the cranial nerves. In answer to Mr. O'Malley, he thought the change in position or course of the nerves described was due to the development of the mastoid process. In sheep and rabbits the stapedius muscle lay above the facial nerve.

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Section of Otology.

President—Dr. A. LOGAN TURNER.

The Research Work conducted in Utrecht on the Saccular, Utricular and Allied Reflexes.¹

By A. R. TWEEDIE, F.R.C.S.

[At the request of the Section the demonstration given at the previous meeting was first repeated and the account concluded.]

This communication was summarized as follows:—

(A) *Further Reflexes of the Semicircular Canals.*

THESE are known as the “progressive movement” reflexes since they are evidenced by certain passive movements of the animal. If, for instance, a guinea-pig be placed, in its normal position, on a board, and gently moved upwards, it will be seen that at the commencement, and throughout the movement, the limbs are flexed, whilst at the termination of the movement they are extended. The reverse occurs with a downward movement. This is the “lift-reflex.” Impetus may, of course, be suggested as a factor, but it can be seen with a very slow movement, and also can be shown to occur, if the animal is held vertically in one hand, with the other just supporting the feet, and the movement be made horizontally. At the same time, if one hand is gently resting on the back, definite tremor of the muscles can be felt, which constitutes the “muscle-tremor reflex.” Again, if the animal is held vertically with one hand grasping it about the axillæ, and if the hind limbs are gently stroked so as to secure a flaccid condition, any slight movement vertically either upwards or downwards will induce spreading of the lower limbs and toes—“the toe-spreading reflex.” Finally, if the animal be held horizontally with one hand around the neck, and the other around the loins, a forward movement will induce a forward movement of the limbs, and a backward movement the reverse, “the springing reflex.”

These reflexes are absent after labyrinthectomy. They are present if the head is held immobile to the body. They are unaffected if the muscle-sense is cut off by injection of novocaine, are unaffected by decerebration or the removal of the cerebellar hemispheres, and are unaffected by centrifugalization which (as is later shown) destroys the function of the otoliths. They must, therefore, be due to the semicircular canals. (They can be demonstrated in guinea-pigs, rabbits, cats and dogs.)

¹ For report of first account see *Proc. Roy. Soc. Med.*, 1922, xv (Sect. Otol.), p. 15.

It has been stated as an objection that the semicircular canals could not react except on some rotation movement, since they form a system comparable to a closed vessel with rigid walls. The walls, however, are not rigid, because there is the elastic fenestrum ovale and the fenestrum rotundum. A model has also been made showing that a movement of the fluid in the semicircular canals does occur in response to motion, downwards, upwards, from side to side, or from before backwards. There are, however, some in Utrecht who still maintain that these reflexes are due to otoliths, although after the otoliths have been destroyed the reflexes still persist.

It is characteristic of the next group of reflexes that they are "tonic," i.e., they persist as long as the stimulus producing them is maintained. They consist of the "neck" reflexes and the "otolith" reflexes. It is convenient to take the former first.

(B) *The Neck Reflexes.*

These appear (a) as certain extensions or flexions of the body musculature, and (b) as movements of the eyeball.

(a) If, for example, a rabbit is laid on its back, and the head extended, it will be found that the fore limbs become rigid, whilst the hind limbs are relaxed. If the head is flexed towards the abdomen, the fore limbs become flexed and the hind limbs extended. If the head is rotated towards, e.g., the right—the limbs on the right side become rigid, whilst the belly muscle of that side is relaxed, and that of the opposite rigid. (The reverse happens with rotation of the head to the left.)

These reflexes are present after ablation of the whole cranial contents down to the origin of the first, second, and third posterior cervical nerves.

(b) If, with the animal in its "normal" position of rest, the body is bent over the head, the eyeballs will be seen to rotate so that their upper poles move forward and downward. With the body flexed under the head the reverse action occurs. If the body is twisted around its long axis (whilst the head is held fixed) the upper poles of the eyes will move in the direction of the back (i.e., that on the "back" side towards its lower lid, and that on the "belly" side towards its upper lid). If the body is bent around its dorso-vertical axis, e.g., towards the right, the right eye will move into the nasal canthus, and the left eye towards its aural canthus (the reverse occurring with an opposite movement of the body).

These reflexes persist after ablation of the whole cranial contents so long as the motor oculi nuclei and corresponding nerves, their "downward" connexion with the spinal cord, and the upper three posterior cervical nerves are intact. There is a latent period for these reflexes of from one-third to six seconds.

THE OTOLITHIC REFLEXES.

(C) *Utricular.*

The otoliths of the utricles ("the lapuli") lie more or less in one plane which is horizontal in the "normal" resting position of the animal. Each is connected with both the homolateral and contralateral side of the body musculature, except as regards the neck muscles, with which it is only associated on the homolateral side. Their action is the result of alteration of their

“position” in space. They have a “minimal” and “maximal” position (the intervening positions affording varying degrees of action).

The “minimal” position is that in which the animal is in its “normal” attitude of rest, the “maximal” the reverse. For instance, in the rabbit the “minimal” position is that in which it normally sits, whilst the “maximal” position occurs when it is laid on its back, with its head at about an angle of 45° to its body. In this latter position the limbs become extended and rigid. (In order to eliminate the possibility of any “neck reflex” as shown above, the animal must be so held that no disturbance of the relation of the head to the body occurs.) If the animal is now turned back to its “normal” position the limbs become flexed.

From these and other data the majority of writers consider that the otoliths function only when they are “dependent” from the otolithic membrane.

(D) *Sacculus.*

The otoliths of the saccules (the “*sagittæ*”) do not lie in one and the same plane, but each lies somewhat below the utriculus of the same side, and in a plane at an angle of about 45° to that of the utricle on the same side. They control the “head position” reflexes and the “compensatory eye-movements.”

The “*head-position*” reflex can be shown by holding a guinea-pig, for instance, by the loins in the horizontal position with the back downwards; under these circumstances the animal attempts to bring its head into the “normal” position and so retain it. In other altered positions of the body the same effect as regards the head will be induced.

The “*compensatory-eye movements*” are demonstrated also with varying positions of the animal, the head and body being maintained in their “normal” relative positions, so as to avoid the introduction of the element of any “neck” reflex as above described. If, with this precaution, the animal be moved from its “normal” to the vertical position with nose upwards, the eyeballs will be seen to rotate, the upper pole moving towards the nose or, if a movement of the body in an opposite direction is made, the upper poles of the eyeballs will move towards the ears. If the body is rotated around its long axis, e.g., to the right, then the right eyeball will move towards the upper lid, and the left eyeball towards the lower lid; the reverse action of the eyeballs occurring with a reversed rotation of the body. (The sacculi are connected with both sides of the body.) The latent period for the otolith reflexes may be as much as twenty-three seconds.

These reflexes persist after decerebration and removal of the brain down to the level of the entrance of the eighth nerves—so long as, for the saccular effects, the motor oculi nuclei are preserved intact. They disappear after section of the eighth nerve—after labyrinthectomy or after “effective centrifugalization.” (For the convenient detection of the movements of the eyeball a “cross” should be gently branded on the cornea.) The reaction can be shown most easily, for example, in the rabbit and guinea-pig—in higher animals (such as the dog or cat) decerebration may be necessary. The reactions may persist as long as a year after labyrinthectomy and other operative interference.

(E) *Data.*

The data on which these findings are based may be summarized as follows:—

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(a) *Operative*.—(1) Removal of brain down to entrance of the eighth nerve.

(2) Removal of brain down to "origin" of the first posterior cervical nerves.

(3) Section of the upper three posterior cervical nerves.

(4) Section of one or both eighth nerves.

(5) Labyrinthectomy—either one or both sides.

(6) Centrifugalization (with the body vertical and the nose upwards) some one to two thousand times a minute for one to two minutes. The procedure is carried out "under ether," and provides a method of isolated destruction of the otoliths; the function of the semicircular canals being left undisturbed. (First suggested and described by Wittmaach in 1909.)

Deductions which may be made from above operative investigations are sufficiently obvious.

This experimental operative research is controlled by:—

(1) Detailed examination of the animals and their response to movements before operation—all of which are carefully noted.

(2) Detailed record of the operative interference.

(3) Re-testing the various reflexes subsequent to operation.

(4) Eventual post-mortem examination.

(5) Comparison of data thus obtained.

(6) Summarization of results and effects.

(General anæsthesia and local anæsthesia are employed in all cases.)

(b) *Accurate anatomical dissection and preparation of models* therefrom indicating the position of the otoliths and their relation to various movements of the body, as a preliminary procedure, serve to check the steps of the above operative investigation, and also provide a ready reference in the examination of the animal on—

(c) *The Examination Table*.—An apparatus on which the animal can be secured in any position and rotated or moved in various directions. With the assistance of a camera fixed on the table, and moving in exact conformity with the animal, an accurate record is obtained of the movements of the eyeball as indicated by a "cross" previously branded (under cocaine) on the cornea.

(d) *The Experiment of Kreidl on the Crustacean, Palæmon*.—Kreidl noted that when this animal "moults," the calcareous particles in the otocyst are expelled and fresh particles taken in. He therefore provided a medium containing only iron granules for an animal in this phase. Afterwards he was able to demonstrate by the application of a magnet definite movements corresponding to definite positions of the magnet in relation to its head.

(e) *The control histological work of Professor Winkler*, as set forth in his classic treatise on the eighth nerve (*see Bibliography*)—an account of special research which merits a separate discussion.

(F) *Clinical Application.*

Professor Magnus and Dr. de Kleijn, the chief investigators in this research, have collected eight cases (*see Bibliography*), all instances of lesions of the cerebrum, and corresponding more or less to the condition of "decerebration." They consist of cases of hydrocephalus—hæmorrhage into the ventricles, meningitis, gumma cerebri, and idiot children. All these showed the "neck reflexes," and four of them showed certain of the "otolithic" reflexes. Twenty-six normal children, under the age of $3\frac{1}{2}$ months, were examined—in none could

the "neck" reflexes be elicited—but in twenty-three certain of the "otolithic" reflexes could be demonstrated.

The dancing mice of Japan show phenomena which may quite possibly be due to pathological lesions of the otoliths, and incidentally it is thought by some writers that these lesions are analogous to those which constitute some of the forms of "hereditary deafness" in man. The study of the lesions in these mice may thus possibly afford some help in this direction.

Some writers consider that disturbance of the otoliths constitutes the main element in the production of sea-sickness.

The inclination of the head to the same side after labyrinthectomy, or in certain pathological lesions of the labyrinth, is also proved by these investigations to be an otolithic reaction, due to an effort to maintain the head in that position, wherein the remaining intact otolith is "at rest," and so the disturbance of balance is avoided as much as possible.

These researches, too, have led to the conclusion by Hoshino that probably the cerebellar hemispheres are not necessarily concerned with the static condition of the body but may be merely subsidiary central "stations" whose functions, after their ablation, can be easily assumed by the larger brain. On the other hand the central control of "vestibular" nystagmus and "by-pointing" is probably located in the cortex of the middle cerebellar lobe (see Bibliography).¹

(G) *Some Phylogenetic Theories.*

From the literature on the subject, and as the result of conversations with the authors of these investigations, fascinating reflections may be suggested. For instance, to such a primitive animal as a fish in a medium, such as water, the necessity of some mechanism whereby balance may be maintained by the assistance of the elemental force of gravity, is at once apparent. For this purpose the balancing apparatus must automatically control the musculature of the body—the dead fish, as is well known, turns belly upwards—to this purpose, therefore, the otolith apparatus and its connexion is developed. Automatic progression is controlled by the "neck reflexes" resulting from movements of the head.

The next item of importance is provision that the animal can locate its prey or danger. This sense, it is suggested, is situated in the "lateral line" organ through which it obtains perception of the movements of the medium in which it lives, and is able to respond to vibrations up to some 15 D.V. per second. This "lateral line" was formerly considered to represent a branch of the tenth nerve, but it is now held by one school to correspond more closely to the eighth. Thus, together with the primitive reflexes which control balance and progression, is developed a "sense," which utilizes these to the animal's welfare.

In the next medium—air—the "lateral line" disappears and a "cochlea" and auditory nerve begin to develop; whilst, when we come to the animals living on land, and the automatic control of movement in a third dimension is no longer necessary, the third otolith (the asterisk in the lagena) ceases to be found. "Balance" and "perception of sound" it is suggested, are associated intimately from very early phylogenetic times, and that this close association still persists is evidenced not only by the proximity of the vestibular and

¹ Since this communication I have seen a hydrocephalic child in whom certain of these reflexes can be elicited.—A. R. T.

auditory organs, but by the obviously still persistent influence of sound on movement (e.g., dancing, melody, marching, or for instance the inevitable "start" that follows unexpected noises).

This theorizing on the sequence of development may be carried yet further: The use of "sound" in controlling movement, demands an apparatus for producing "sound," and thus necessity for "voice," and its mechanism is developed—and with "voice" comes "language," and, finally with "language" that attribute only found in the highest animals—intelligence.

Postscript.—As the communication to the meeting was chiefly colloquial, its printed record is offered rather in the form of a "paper" so that the valuable work of the authors shall suffer as little as possible by faulty phraseology, or omissions. The communication included demonstrations of the "neck" and "otolith" reflexes in the guinea-pig and rabbit, illustrations (by the epidiascope) of some of the apparatus used, of some of the experiments, of photographs, and enlargements of certain diagrams, which appeared in the original publications mentioned hereunder.

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DISCUSSION.

Mr. SYDNEY SCOTT welcomed Mr. Tweedie's account of his experiences at Utrecht, for he had brought to the notice of the Section some of the most important facts in connexion with this new research in otology since the original discovery by Flourens, that the labyrinth was concerned in some way or other with co-ordination. He regarded Winkler's experiments as perfectly wonderful. Everyone had realized the great difficulty attending the differentiation of function of the saccule, utricle and semicircular canals. We could interfere physically with the labyrinth as a whole or with the semicircular canals more or less individually, without necessarily disturbing the utricle or saccule, but until the idea was conceived of employing *centrifugal force to detach the otoliths* from the utricle or saccule, without interfering with the canals or tympanic structures, the difficulties had seemed insurmountable. He believed the results so far obtained threw so much new light on the subject, that the physiology of the labyrinth in its bearing on the nervous system would have to be reconsidered. He asked if Mr. Tweedie could say whether different results were obtained by centrifugalization before and after decerebration? and had the observers whose investigations he had described, been able to exclude any possible effect of high-speed centrifugalization on the blood supply of the brain cortex? He assumed of course that this had been fully considered. He would like to ask whether those investigations had led the observers to negative Bárány's hypothesis in connexion with localization of arm and leg areas in the cerebellar cortex?

Dr. WILLIAM HILL asked to which of the two the utricle was more closely related—to the saccule, or to the semicircular canal system?

Dr. ALBERT A. GRAY said he agreed with the view put forward by Mr. Tweedie that these reflexes were developed in response to stimuli of movement. This was borne out by the facts that, in the case of animals with delicate head movements, the vestibule and canal were well developed. In fish, which had a graceful and easy movement, it was so. Amphibia showed a less developed organ of equilibration; and the same condition was found in some reptiles such as tortoises. The movements of crocodiles and lizards were quicker and more delicate, and the apparatus was consequently better developed than in tortoises. Birds had large canals with a rich nerve-supply. The same held good to a less extent in mammals. He recently prepared the labyrinth of a giraffe, in which the canals and vestibule were very well developed. In the porpoise and sea cow there was a poor equipment in this regard. Therefore comparative anatomy confirmed the idea put forward, that it was the delicacy of movement which brought about the development of the organ. It was not so easy to explain why the cochlea developed from the saccule. The winged insect developed a hearing apparatus entirely independent of the statocyst. It had its otolithic apparatus in one structure, while the hearing apparatus might be in one of various parts of the body such as the forelegs. Dr. Gray thought that the development of a hearing organ was to be associated with the change from a life in water to a life in air. Air was a compressible medium, and water was incompressible.

Mr. TWEEDIE (in reply) said the animals had been kept a year after centrifugalization and showed no apparent symptoms except the disappearance of those reflexes. As to Bárány's work, Hoshino showed that the cerebellar hemispheres were only a subsidiary centre, and so long as the cortex of the central lobe of the cerebellum remained, it did not much matter whether there was a cerebellar hemisphere or not. If total labyrinthectomy were done on each side, the only reflexes which persisted were the neck reflexes. In answer to Dr. Hill, there was no doubt, he thought, that the utricle was the parent of the semicircular canals.

A Modification of the Mastoid Operation for Early Suppuration in A-cellular Mastoids.

By DAN MCKENZIE, M.D.

THE modifications consist in making only a short incision corresponding to the upper half of the auriculo-mastoid angle; in an exposure limited to the suprameatal triangle and its immediate neighbourhood; and after the antrum has been opened, in the provision of drainage through a window made in the postero-superior wall of the membranous meatus, the posterior incision being entirely closed.

The operation is suitable only for early suppuration and when the mastoid process is a-cellular. In cellular mastoids when the cells have become infected the ordinary Schwartze's operation is obviously necessary, and if the disease is chronic the radical operation is called for.

In children the operation is quickly and easily performed. The "window" in the meatus closes readily.

The object of the meatal window is to secure a more direct drainage of the antrum than can be obtained in a cellular mastoid from the posterior wound after the auricle has been replaced.

The operation may be recommended to those who favour early drainage of the mastoid antrum in suppuration of the middle ear which is not promptly cured by paracentesis, but its use is limited to this single variety, as infected cells in the body and tip of the process lie below the level of the meatal window.

DISCUSSION.

Mr. SYDNEY SCOTT said that he was afraid it would be easy to overlook groups of unsuspected mastoid cells. He preferred to open the mastoid behind, and make certain of its anatomical structure and what cells there were to be opened. Although Dr. Dan McKenzie was in favour of the procedure, he could not feel justified in adopting it, or recommending others to do so, in place of Schwartze's operation.

Mr. ARTHUR CHEATLE said the only way to ascertain whether an a-cellular temporal bone was being dealt with was to have a skiagram taken. With regard to making an opening in the meatus, the cases did so well under the ordinary procedure, that he did not care to run additional risk by doing the operation now described.

Dr. DAN MCKENZIE replied that he did not feel any disappointment with regard to what he carried out, and intended to continue the operation in suitable cases.

The Thermionic Valve of Professor J. H. Fleming—a Piece of Apparatus capable of amplifying Sound, with a Demonstration to show the Possibilities of applying the Thermionic Valve to aid the Deaf.

By ROBINETT SCRUBY.

(Introduced by Mr. FREDERICK SPICER.)

THE thermionic valve consists of a glass bulb, similar to an electric light bulb, inside which is placed a filament, a metallic plate and a spiral wire (called the grid). The grid, the filament and the plate are connected to metal contacts on the insulated base of the valve. The air is exhausted from the bulb to a more perfect vacuum than that in the electric light bulb; when the filament is heated electrons radiate from it; these are charged negatively. The temperature of the filament is varied by a rheostat. If the filament is slightly heated, few electrons will be radiated; when the filament is heated to a high temperature, electrons will be radiated freely.

A high voltage battery is connected to give a positive charge to the plate. The positive charges on the plate attract the negative electrons from the filament. The telephone receiver is connected to the plate through the high tension battery.

The electric impulses from the telephone transmitter vary the potential of the grid. The current from the telephone transmitter is low tension and passes through a transformer before reaching the grid and when sound waves impinge on the diaphragm of the transmitter the grid is given a positive and negative charge alternately. Therefore, when the grid is negatively charged the negative electrons from the filament will be repelled and will not pass through to the plate, and very little current will flow through the telephone receiver; when the grid is positively charged the negative electrons will be attracted from the filament and will readily pass through to the plate; consequently a heavy current will flow through the telephone receiver. The sound waves control the electric energy on the grid. The grid in turn is the valve between the telephone transmitter and telephone receiver. Therefore the small amount of current in the telephone transmitter supplied by the low tension battery controls the grid in the valve, which in turn controls the high tension current passing through the telephone receiver or earpiece. Thus we get a real amplification of speech.

As regards results with the deaf: So far the thermionic valve has not proved any more efficient than one of the simple forms of telephones for the deaf, which have the most suitable pitch, wave form and strength, for the particular case, built up from the data derived from testing the case with a properly designed testing instrument. Yet we know that such an instrument does not amplify the sound but alters the quality of the sound, whereas the thermionic valve amplifies the sound to a marked degree. This bears out the fact that it is better to alter the voice to suit the particular form of deafness than to shout at the deaf person.

An electrical aid, sound collector, tube or trumpet will be found to give the best result if made up as follows after a careful test:—

	Wave form	Pitch	Volume of sound
Middle ear ...	Sharp peaked, not rich in harmonics	High	According to degree of deafness
Otosclerosis ...	Rounded	High or low	According to degree of deafness
Nerve deafness ...	Well rounded, rich in harmonics	Low	No great volume of sound

Electrical aids can be varied in wave form, pitch, and volume of sound, but are seldom used for nerve deafness.

Sound collectors, tubes and trumpets, can only be varied in wave form but certain types are very useful for nerve deafness.

When we are in a position to vary at will the wave form and pitch of the thermionic valve there is every reason to believe that the deaf will get a very much increased range of hearing.

I wish to thank Mr. W. H. Pettifor for the results of his tests and for the instruments he had placed at my disposal, Mr. S. G. Brown for the loan of his instruments, and the numerous aural surgeons who have allowed me to make use of their hospitals and have helped me with their diagnoses.

Section of Otology.

President—Dr. A. LOGAN TURNER.

Otitic Meningitis.

By G. J. JENKINS, F.R.C.S.

CASES of affection of the meninges via the labyrinth, treated by translabyrinthine drainage with recovery. Three of the cases are good examples of the symmetry of pathological processes. One of the cases had translabyrinthine drainage of the meninges through the right ear in June, 1921, and again through the left ear in October, 1921. The labyrinths are functionally destroyed. This patient shows how slight the disability is after destruction of the vestibular element of the labyrinth in the human subject.

Case I.—T. J. F., male, aged 30. Admitted to Lambeth Hospital June 19, 1921, very ill, complaining of severe occipital headache. Some rigidity of neck. Knee-jerks present. Temperature 101° F. Edema over right mastoid. Tendency to keep head turned to right. Foul otorrhœa. No vomiting. Occipital headache.

June 21: Seen by G. J. J. Mental condition fairly good. Somewhat torpid.

History from patient: Discharge from right ear twelve months, ? cause. Says he never had discharge before, and never from left ear. Earache off and on in right ear for one and half years and never before. Deafness: Right, one and half years; slightly deaf before then, and completely deaf in right ear a few days; tinnitus at times in right ear. Vertigo: Two occasions, first one month ago, second last four days; cannot give particulars. Headache? frontal and occipital; continuous and bad four days. Vomiting: One month ago, and again six days ago.

Lying on right side: Retraction of head, slight but definite; will not allow flexion of head beyond, say, 20° from the vertical. Some tenderness of the posterior muscles of neck.

Reflexes: Plantar, flexor both sides; patellar, feeble both sides, equal; abdominal, not obtained either side; pupils react to light.

Definite periosteal thickening, and tenderness over right mastoid.

Right ear: Much offensive discharge, no tympanic membrane. Granulation in mid-ear.

Left ear: Offensive discharge, similar to right. Considered too bad a witness for tuning-fork tests.

Hearing: Cannot hear conversational voice in right ear, and difficult to be sure of amount of hearing in left, as he has to be roused by sharp questions.

Nystagmus: With eyes deviated to left, spontaneous fine irregular nystagmoid movement; with eyes deviated to right, *nil*.

Fundus of eyes not examined.

Test of Rombergism not applied.

Caloric test: Hot water in right ear, no effect on the nystagmus. Cold water in left ear not tried.

(Note inaccuracy of the history given by patient.)

General anæsthetic same afternoon of above examination. Lumbar puncture: Cerebro-spinal fluid, very turbid, examined immediately. Report on cerebro-spinal

fluid by Dr. Perdrau: fluid turbid, no clotting; total protein, 0'04 per cent.; sugar, a trace; chlorides, 0'677 per cent.; cells, 960 per cubic millimetre. Differential count: Polymorphs, 77 per cent.; lymphocytes, 21 per cent.; endothelials, 2 per cent. Direct examination, occasional pair of cocci. Culture sterile. Wassermann reaction negative.

Operation: Radical mastoid right side. Dense outer wall, with deep layer of cells extending back to lateral sinus. Pus to sinus. Fistula of the external semicircular canal. Stapes not recognized. Inferior labyrinthotomy and opening made into the internal auditory meatus for drainage. Seventh nerve injured in the internal auditory meatus.

June 23: Some headache. Facial paralysis right side. No vomiting. Cerebro-spinal fluid flow very free.

June 24: Cerebro-spinal fluid flow free. Lumbar puncture. Fluid turbid.

June 30: Patient very well. Cerebro-spinal fluid by lumbar puncture. Report: Protein, 0'02 per cent.; sugar, diminished; chlorides, 0'731 per cent.; cells, 8 per cubic millimetre, all lymphocytes; not quite a normal fluid.

Progressed to complete recovery from the meningitis.

November 9, 1921: Again admitted to Lambeth Hospital.

November 10: Note by Dr. Stebbing, 10 a.m.: Severe left side headache; groaning delirium; vomiting; absolutely deaf, no nystagmus, knee-jerks present; plantar, right present, left?. 2 p.m. note by G. J. J.: Patient excited; completely deaf; questions in writing. Reads badly on account of refraction error. Can be made to answer questions, but condition of patient makes him a bad witness. Shouts his answers. History of complete deafness, vertigo and vomiting coming on about five days ago. He had been quite well until this attack.

Tenderness over left mastoid; no obvious œdema; much pus in left meatus. Granulations in left middle ear. Right ear still discharging; ? slight paresis of left face; head straight; slight rigidity of neck.

Plantar reflexes: Both flexor; abdominal present, feeble; knee-jerks present, feeble.

Nystagmus: A spontaneous nystagmoid movement, with eyes deviated to the right, very fine and irregular, nature doubtful.

Caloric test: With cold or hot water in left ear, no effect.

Anæsthetic immediately after examination.

Lumbar puncture: Cerebro-spinal fluid cloudy. Report by Dr. Perdrau: Protein, 0'05 per cent.; sugar, trace; chlorides, 0'663 per cent.; cells, 433 per cubic millimetre; polymorphs, 75 per cent.; lymphocytes, 25 per cent.; plasma, *nil*; micro-organism, *nil* direct or culturally.

Operation: Radical mastoid, dense outer wall; cholesteatoma; large opening into external canal. Seventh nerve free in granulations. Nerve injured here during operation. Granulations in middle ear. Superior and inferior labyrinthotomy. Opening into internal auditory meatus. Cerebro-spinal fluid in *small* quantity.

November 11: Comfortable. Cerebro-spinal fluid coming away freely. Facial paralysis left side. No increase of nystagmus or nystagmoid movement noted.

Progressed to condition as seen to-day. Facial paralysis recovering on both sides. See note on demonstration as to the man's efficiency.

Example of symmetry of pathological processes: Chronic otorrhœa in both ears. Anatomical symmetry. Fistula of both external semicircular canals. Meningitis via the labyrinth on both sides.

Thanks are due to Dr. Stebbing for his early recognition of the condition.

Case II.—A. W., male, aged 21. Admitted King's College Hospital, December 15, 1921. History difficult to obtain as patient somewhat torpid. Headaches: Five weeks in occipital region; worse three or four days, and seemed worse at night. Discharge: From both ears as long as he can remember. Earache: Off and on for years; bad in left ear a few days ago. Deafness: Always slightly deaf; think completely deaf in right ear since 1918. Vomiting: Seven days.

Ears: General mastoid tenderness on left side; offensive discharge from both ears; much swelling and redness of the posterior wall of meatus on both sides; on right side more than left. Postero-superior perforation of both tympanic membranes. Granulation in right ear.

Tuning fork test : Weber, to left ; Rinne, left, negative to C₂, right, sound all to left ear. Bone conduction good in left ear. Hearing to conversational voice, right *nil*, left 12 ft.

Nystagmus : Eyes to left, coarse mixed nystagmus, with often a prolonged slow phase to the right. Eyes to right, *nil*. Slight paresis of the left external rectus muscle. No facial paresis.

Reflexes : Abdominal, present both sides and equal ; knee-jerks, good, equal ; plantar, both flexor equal. No rigidity or tenderness of the neck muscles. Rombergism : Falls to the left. Caloric test : Hot water in right ear, no effect ; no change in spontaneous nystagmus. Cold water in left ear not tried.

Anæsthesia : Eyes, right, disc swollen and margins flabby ; left, normal.

Lumbar puncture : 5 c.c. slightly turbid cerebro-spinal fluid. This fluid examined by House Surgeon, who reported about 400 cells to cubic millimetre, that cells were almost all lymphocytes, and that there were a great many plasma cells. Resident Pathologist supported above finding, and found increased sugar. Fluid was examined whilst operation on left ear proceeded.

Operation : Left ear, radical mastoid ; dense mastoid with deep layer small cells ; cholesteatoma ; large fistula into external semicircular canal ; granulations in middle ear ; descending process of incus destroyed ; stapes not recognized. Right ear, radical mastoid ; condition similar to those found in left ear in every particular. Superior and inferior labyrinthotomy. Internal auditory meatus not opened at this operation.

December 16 (day after above operation), 10.30 a.m. : General condition worse. Mentally worse. Complained of much occipital pain, and pain in back. Definite rigidity of neck. Tenderness of posterior muscles. Abdominal reflex feeble both sides. Knee-jerks present, but very difficult to obtain. Plantar doubtful. Kernig's sign well marked. Other symptoms and signs as before.

Anæsthetic about fourteen hours after operation of 15th. Lumbar puncture : Cerebro-spinal fluid much more turbid. Report on fluid by pathologist of the hospital : Cells per cubic millimetre, 2,120 ; polymorphs, 90 per cent. ; lymphocytes, 10 per cent. ; plasma cells, *nil*. Cultures, sterile ; total protein, not estimated ; globulin, increased ; sugar, diminished.

Further operative procedure on right ear : Translabyrinthine drainage of meninges.

December 17 : Patient better, but still slight headache and slight rigidity of neck. Knee-jerks very feeble, but obtained both sides. Dressings had to be changed twice on right ear during the night for hæmorrhage (? cerebro-spinal fluid). As the cerebro-spinal fluid flow seemed to have ceased the patient was given an anæsthetic, and after careful cleansing of the cavity of the right ear, the drainage way was cleared. The way seemed to have been blocked with clot. Lumbar puncture done, and cerebro-spinal fluid drawn off. Report by pathologist of hospital : Cells per cubic millimetre, 900 ; polymorphs, 70 per cent. ; lymphocytes, 25 per cent. ; plasma cells, 5 per cent. ; total protein, 0.08 per cent. ; globulin, increased ; sugar, diminished. No bacteria seen by direct examination. Cultures sterile. Convalescence rapid and complete recovery.

Note rapid change in clinical aspect and in character of cerebro-spinal fluid in about fourteen hours after first operation. Also the change in cerebro-spinal fluid within forty-eight hours after the drainage of the meninges.

This is also a good example of the symmetry of pathological processes : (a) Chronic otorrhœa, both sides ; (b) perforation of tympanic walls on both sides, about same size and same situation ; (c) fistula of both external semicircular canals.

Thanks are due to Dr. Masterman of the Camberwell Infirmary for recognizing the serious nature of the condition, and so promptly dealing with the patient.

Case III.—H. B., male, aged 19, admitted King's College Hospital, June 12, 1919. Patient had travelled from Liverpool the day of admission to hospital. When seen, he was lying on a couch at the hospital in a collapsed state—moist skin, very pale, with small pulse. Mental condition seemed fair. Complained of mental confusion, severe giddiness and headache.

History from patient : Deafness slight in both ears since infancy ; complete deafness, left ear, three days. Discharge : Both ears since infancy ; ? cause. Earache : Off and

on in both ears since infancy; bad in left ear about four days. Tinnitus: Machinery noise and whistling off and on since early life; not noticed these any worse recently. Vertigo: Slight for some months but very bad for three days; falls to the left. Vomiting: Two days. Headache: General; feeling of great heaviness of the head. Had an operation done on each ear in early childhood—at about 6 years of age—for discharge from the ears.

Right ear scar over mastoid. Has appearance of having had a radical mastoid operation. Granulations in the tympanic cavity and antrum regions. No tympanic membrane or ossicles to be seen. Discharge.

Left ear: Scar over mastoid; offensive discharge; remains of malleus; sinus in posterior wall of meatus; marked tenderness over mastoid and slight periosteal thickening.

Tuning forks: Weber to right ear. Rinne, left, not heard by air conduction and by bone to right ear. Hearing of voice: Not recorded.

Nystagmus: Eyes to right, marked horizontal and rotary nystagmus; eyes to left, slight irregular nystagmoid movement.

Rombergism: Fell to the left, could not sit up straight.

Caloric: Hot water in left ear—no effect.

Anæsthetic, about four hours after examination: Lumbar puncture, turbid fluid; fluid not microscopically examined.

Operation: Radical mastoid on left ear. Granulations and pus in the antrum. A sinus found extending toward the ampulla of the superior semicircular canal. Could not distinguish stapes. Superior and inferior labyrinthotomy, and translabyrinthine drainage of the meninges. Cerebro-spinal fluid came away freely.

Day after operation: The cerebro-spinal fluid drained away freely and soaked the pillow in a short time. Patient much relieved. Pain and mental confusion gone. Rapid recovery.

July, 1920: Had a return of giddiness. Radical mastoid operation in right ear, granulation in the old cavity. Sinus extending towards ampulla of the right superior semicircular canal. This ear took a long time to heal and it was repeatedly noted that pressure in region of the above sinus caused marked giddiness. The walls of the cavity are covered with epithelium and the regions of the superior and inferior labyrinthotomy operation are indurated by very clean cut depressions.

Now uses wick in right ear with good effect.

This is a good example of the symmetry of pathological processes in the ear, the sinus extending toward the ampulla of the superior semicircular canal.

Case IV.—G. E., male, aged 24, Fourth London General Hospital (Territorial), April 8, 1918. Had been discharged from the Army for neurasthenia and otitis media and was waiting in the hospital for "papers to go through."

Seen in bed in badly lighted ward. Patient restless. Mental condition cloudy.

History from patient: Army papers not available. Discharge: Left ear. Thinks it began when in France, 1917, and continuous up to present time. Deafness: Left ear for some years. Passed into the Army in 1915, with slight deafness, thinks he has been "stone deaf" in left ear since "blown up" in France, 1917. Earache: about two weeks in left ear. Never before, very severe at first. Vertigo: Slight attacks for some months—attributed to neurasthenia. About ten days ago whilst in bed, "felt something go" in the left ear followed by severe vertigo. Now falls to left when standing up.

He has felt dazed ever since something happened in left ear. Now has a general headache and confused feeling. Marked rigidity of neck. Tenderness in occipital region. Resents examination. Reflexes normal.

Left ear: Periosteal thickening and tenderness over the mastoid. Purulent discharge. No further description on record.

Right ear, nil.

Tuning forks: Weber to right; Rinne, right position, left to right ear.

No record of hearing of conversational voice.

Anæsthetic: Lumbar puncture, turbid cerebro-spinal fluid.

Operation: Radical mastoid. Inferior labyrinthotomy and translabyrinthine drainage of the meninges.

April 9: In every way much better. Rigidity of neck disappeared. Free flow of cerebro-spinal fluid.

Patient left hospital against orders in less than three weeks from time of operation. Wound not healed.

Present condition: The walls of cavity are soundly healed—covered with epithelium, and a clean cut depression can be seen where the inferior labyrinthotomy was made.

Case V.—A. E., male, aged 35. Seen as out-patient at King's College Hospital, November 9, 1914, complaining of giddiness and discharge from the right ear.

History: Discharge from right ear since 14 or 15 years of age, off and on. Earache: Severe in right ear when "under canvas," about a month or five weeks ago. Deafness in right ear slight for many years, worse when in camp and much worse now. Vertigo: Some giddiness four or five weeks. Unsteady in walking. Never fell. Worse last few days. Objects move left to right. Perforation of membrana flaccida and outer wall of attic on right side. Pus from perforation on exhaustion. Hearing: With Bárány noise apparatus in left ear, shouting not heard in right; Rinne, — on right mastoid to left ear. Rombergism: Falls to right.

Advised to come into hospital.

Admitted into King's College Hospital as an urgent case, December 11, 1914. Examined this case late that night. Patient looking very ill and complaining of mental confusion and occipital headache. Vomiting for two days. Nystagmus: Fine irregular nystagmoid movements with eyes directed to left. Caloric test: Cold water in right ear, no effect.

Anæsthetic immediately after examination: lumbar puncture, turbid fluid; complete examination of fluid not made.

Operation: Radical mastoid, inferior labyrinthotomy, translabyrinthine drainage of meninges.

No notes on convalescence.

REMARKS.

Eight cases of translabyrinthine meningitis that recovered after translabyrinthine drainage of the meninges; the patients were asked to attend, but only five have presented themselves. There was lepto-meningitis in all these cases of meningitis, and presumably the labyrinth has been part of the track of the infection to the meninges. In most of the cases the labyrinthitis was obviously recent.

Case I is unique. The symmetry of the pathological process is complete. There was chronic otorrhœa, fistula of the external semicircular canal and septic labyrinthitis in both ears and septic meningitis from each ear in turn. Patient had translabyrinthine drainage on the two occasions of meningitis and on both occasions recovered. He now has functional loss of both labyrinths. As has just been demonstrated to you he can walk straight, with eyes open or shut, jump backwards or forwards, hop on right or left foot, bend forwards and laterally without falling. Standing in upright position he can localize an object after being turned in various directions with the eyes shut, as well as a normal man. He seems to be able to do most things an ordinary man can do. He also can follow a moving object with his eyes, his head being fixed, and keep his eyes fixed on an object whilst rotating his head in various planes—just as a normal man would do. You saw him being rotated at a fast rate a great number of times, then get out of the rotation chair and walk straight away as if nothing had been done to him. This effectually proves that the labyrinth has been destroyed. He is in the position of a normal person who is having simultaneously equal stimulus applied to both labyrinths.

Case II indicates the importance of early diagnosis of meningitis. In four-

teen hours a cell count in cerebro-spinal fluid was changed from 400 morpho-lymphocytes to 2,120 cells, 90 per cent. polymorphonuclears. This case also shows that the sugar may even be increased in the early stages of meningitis. It serves also as a good example of the symmetry of pathological processes, namely: (a) chronic otorrhœa; (b) acute mastoiditis—with swelling on posterior-superior wall of meatus; (c) cholesteatoma; (d) fistula of external semicircular canal in both ears.

Case III: Sinus leading to ampulla of superior semicircular canal on both sides.

DISCUSSION.

Dr. A. A. GRAY (Chairman) said this series of cases was most important from the view of recovery from meningitis. Not long ago meningitis was looked upon as a hopeless condition, but the outlook was different now, for many cases recovered. The behaviour of a person with both labyrinths destroyed was interesting from the pathological and physiological standpoints. Had the patient been to sea since the operation, and if so, did he experience sea-sickness? It might bring to light further information as to whether the centre of disturbance, in sea-sickness, resided in the vestibular canals.

Mr. TILLEY said this series of cases were the most valuable the Section had seen for many years. Would Mr. Jenkins operate through the labyrinth in all cases of meningitis of otogenic origin as opposed to lumbar puncture in those which did not show organisms in the puncture fluid? He could recall certain cases in which the patients had been desperately ill with all the symptoms of acute meningitis, yet they had got well after lumbar puncture alone. He took it that they were not instances of septic meningitis but were merely inflammatory. In other words, if there were no micro-organisms in the cerebro-spinal fluid, would Mr. Jenkins consider the state of the case demanded the translabyrinthine route?

Dr. W. HILL asked whether the fistula sign was present in any of the cases.

Mr. JENKINS said it was present in two only.

Mr. J. F. O'MALLEY said that once having arrived at a diagnosis of meningitis, it had been the custom to regard the case as fatal. It was now possible to save early cases by dealing with them in the systematic way Mr. Jenkins had adopted. Assuming that Mr. Jenkins was able to make a diagnosis of purulent meningitis from examining the cerebro-spinal fluid, and that he could not be sure the infection had gone through the labyrinth, did he consider that the ideal method of treatment would be that carried out in these cases?

Mr. W. STUART-LOW said that he understood it was at the early stage of meningitis—that was, at the stage when lumbar puncture generally gave some relief for a time—that Mr. Jenkins operated. He (Mr. Stuart-Low) had had two cases in which definite symptoms of meningitis had developed after operation. In his opinion it was reprehensible to syringe such cases, as likely to jeopardize the recovery of the patient; he therefore used a spray to remove the accumulated discharges. He found suction, however, of the greatest benefit and service. He had had small glass bell-shaped suckers made with rubber edges, and these he applied to the inner wall of the tympanic cavity, and used suction to draw septic discharges out of the labyrinth through the round and oval windows. After translabyrinthine drainage had been established by operation he had found this method most effective and much better than trusting to drainage alone. He was certain that at least two of his patients who were going rapidly downhill after operation were rescued by this method.

Mr. E. D. DAVIS said that this valuable series of cases clearly indicated when labyrinthotomy should be done. How did Mr. Jenkins proceed after the removal of the promontory in performing inferior vestibulotomy?

Mr. JENKINS (in reply) said that this work had followed upon the description of the operative procedure for labyrinthitis by West and Scott. The operation had been slightly modified; no two operators did exactly the same operation. In answer to Mr. Tilley, if a patient had a meningitis from infection through the labyrinth, i.e., labyrinthitis and meningitis following it, it was imperative to open the internal auditory meatus, whatever else was done. The internal auditory meatus must be regarded as a pocket, and if the drainage was not thorough it would constitute a focus of infection. A certain number of cases were bound to get well with lumbar puncture if the meningitis was of the irritation type; but he knew of no way of ascertaining whether micro-organisms had got into the meninges or no. The lumbar puncture was a long way from the original site of infection, and few cells might be found in the cerebro-spinal fluid, and yet in the internal meatus there might be a serious local infection. Therefore, if a patient had meningitis translabyrinthine in origin, drainage should in all cases be through the labyrinth; lumbar puncture could only be regarded as helpful, not curative; to rely upon it would be to lose patients' lives. His operative details did not differ much from those of West and Scott; he paid particular attention to a thorough cleaning up of all regions where pus was lurking. He advised superior and inferior labyrinthotomy. He syringed the whole region with iodized alcohol before he opened the internal auditory meatus. His object was to prevent any chance of secondary infection from organisms in the middle ear, which were present in all such cases. In two instances last year the patients seemed to be doing very well, and then the wound became septic; there seemed to be a re-infection of the meninges, and the patients died. In answer to Mr. E. D. D. Davis he said that, having taken away a piece of the promontory, he went on with the straight chisel, keeping as low as possible in the internal meatus, and, having arrived there, he used a little rough instrument which cut outwardly as it was withdrawn, and the opening was enlarged downwards and backwards. He abstained from putting anything from the middle ear into the internal auditory meatus, i.e., in the nature of a drain or wire; he used a wick from the external auditory meatus to the opening in the promontory. If the cavity was larger than usual he left the posterior wound open and used a gauze drain. He raised the legs of the patient and kept the head low, and the patient lay on the operated side. In two cases he washed through, in one with eusol, in the other with normal saline. Both patients seemed to be in a very desperate condition, but they recovered.

Aural Exostosis—Second removed from the same Meatus Nine Years after First.

By RICHARD LAKE, F.R.C.S., and A. J. WRIGHT, F.R.C.S.

MALE, fine physique, seen with the late Mr. Harold Mole. Large single exostosis removed from posterior wall of right external auditory meatus. Some discharge before removal. When membrane seen after operation, anterior perforation visible—? pre- or post-operative. Growth large and deep; it was removed through post-auricular incision.

Three years later, anterior meatal wall swollen. In October, 1920, aural discharge developed. On account of narrowed meatus, difficulty experienced in treatment, even with attic cannula, but meatus became dry. Patient showed marked mental depression.

November, 1920: Second growth removed through meatus. ? Attached by broad base to anterior wall. After removal attachment was found to be to floor of meatus. Uninterrupted recovery. Small marginal perforation noticed, which healed readily. One of us has seen a somewhat similar case of exostosis recurring after operation. In this case mental depression was also marked.

DISCUSSION.

Dr. W. HILL said there were very few cases of recrudescence of these obstructions. In one case he had had to operate three times; there were great hyperostoses, the aperture being nearly closed. The third removal seemed to have been successful. He operated post-aurally. This case had been rather one of hyperostoses than of multiple exostosis.

Mr. E. M. WOODMAN asked whether any members had had experience of destroying exostoses by the electric drill through the meatal route.

Mr. TILLEY said Mr. Field had effected removal with the electric drill, but in those he himself had tried he was disappointed, on account of the granulations and the trouble in dressing them afterwards, due to pain and occlusion of the meatus, which often recurred after the operation. He had long since employed the post-oral operation, which was simple and safe. He agreed that in some of these cases it was not merely an exostosis, but a hyperostosis of the meatal wall. In the Museum of University College Hospital there was a specimen with a smooth, eburnated, ivory-like surface, but it had a broad base, and occupied the greater part of the posterior bony wall of the meatus.

Mr. A. J. HUTCHISON said he had been watching a case of the kind. There was exostosis in both ears. Mr. Cumberbatch had operated on one ear in 1892. The exostosis in that ear recurred to such a degree that it was impossible to tell which ear had been operated upon. No change whatever had occurred during the last fifteen years.

Mr. H. J. BANKS-DAVIS said the mere fact of the presence of an exostosis in the meatus did not mean that operation was required. In many cases in which operation had not improved the hearing, the failure was due to the presence of exostosis inside the middle ear as well as outside.

Mr. LAKE replied that he did not like the drill in these cases; he used it only once. As a rule he did not turn the ear forward.

Otosclerosis—with a possible Bearing on the *Ætiology* of the Disease.

By RICHARD LAKE, F.R.C.S.

I WOULD draw the members' attention to two special points in the following cases, namely: the incidence of the disease, and the effect of the removal of septic foci upon it.

Miss M. C., aged 24. First seen December, 1917. Recommended by Dr. Shackel, of Mayfield. Alleged deafness since the age of 12. Treatment: "Hectine" injections ampoule A, two weekly.

Second visit, June 14, 1918: Considerable improvement. Third visit, August 30, 1918: Fatigue period of ten seconds remarked. Next year, severe cold, became very deaf, and when seen on January 31, 1919, both promontories were red. I feared no further treatment would be of any use, and although I scarcely hoped for success, I removed her tonsils. The result was excellent, though unforeseen. Both fundi lost their red hue, hearing improved to a marked extent.

Patient returned in June, left fundus red, and, despite treatment, the hearing deteriorated. In May, 1920, patient saw Dr. Albert Gray, who agreed as to the diagnosis, and made valuable suggestions as to treatment. Still thinking that a septic nidus might be the source of the trouble, I sought for another focus of infection, and found pus in the left maxillary antrum. Antrum

explored on May 27, 1920 : Full of pus. Disappearance of red reflex. January, 1921 : Red reflex again on right side. Teeth examined by Mr. Every-Brown, who discovered dental caries and caries of jaw, and X-ray examination showed disease around the sockets. Carious teeth and affected portion of jaw removed. No benefit.

Exhibitor would be grateful for any suggestions as to further treatment.

Case II (not shown).—Woman, aged about 25, recent history of rapidly increasing deafness; both ears showed red reflex. Tonsils removed without delay. Two weeks later, redness of fundi gone and hearing as good as ever.

Case III.—Lady, aged 30. This was the case described at the Twelfth International Congress as a case of otosclerosis, relieved by radium. Relief not permanent, fundi again became red, and hearing deteriorated until January, 1916. Various arsenical preparations seemed to keep hearing stationary. During this time she was seen by Sir William Milligan. Tonsils removed June 21, 1921; hearing improved up to time of writing. Can now carry on a certain amount of conversation without speaking tube.

DISCUSSION.

Dr. A. A. GRAY said he saw the patient in the first case about two years ago; she was very dull of hearing, and it was a typical case of otosclerosis. The case illustrated what he believed to be a general truth, namely, that when dealing with otosclerosis, all septic foci found should be removed. That seemed to be the most which could be done for otosclerosis at present. This patient certainly improved after the operation, though her condition had become somewhat worse recently; but that might be because some infection was still occurring.

Mr. STUART-LOW said that he agreed with Dr. Gray's remarks and thought the improvement in hearing in cases of otosclerosis during a catarrh might be due to the Eustachian tube becoming filled up with mucus from the nose and throat. Mr. Lake's cases showed the importance of doing everything to improve the patient's general health and justified one in trying to do all that was possible for those suffering from otosclerosis. Enucleation of the tonsils was, therefore, warrantable on the score of general health, but he was always very careful to take every precaution to avoid much hæmorrhage during the operation, as this might result in deafness being increased.

Dr. DONELAN thought Mr. Lake was to be congratulated on applying the principle of curing joint infections by removal of purulent foci as a means of improving otosclerosis. If Mr. Lake was satisfied that the antrum in the first case was still a cause of offence, why was it not proposed to drain it thoroughly?

Mr. LAKE (in reply) said that he brought the cases forward thinking they might constitute a stimulus to further work on the subject. In answer to Dr. Gray, he did not know whether members had tried the effect of keeping the ear moist in post-suppurative cases with odourless paraffin; he had found that good results followed that treatment. In reply to Dr. Donelan, he said that he had washed the antrum out once or twice since, but without finding evidence of infection.

Exhibition of Pontimeter.

By T. B. JOBSON, M.D.

This instrument, which I have named the pontimeter, is for measuring the depth of the bone to be cut away before the complete removal of the "bridge" in the mastoid operation. It is a sliding callipers made in the shape of the

Dundas-Grant probe with the Heath ribbed type of handle, which gives great delicacy of touch. With its help one can graduate the last blow of the chisel a little more accurately. When a Heath operation is done it is useful to know the exact depth of the bone to leave—usually $\frac{1}{4}$ in. In use the slide is drawn up so that the points are about an inch apart. The distal point A is inserted in the aditus and the slide lowered until the proximal point B rests on the bridge. The depth of the bridge A B can then be read off instantly on the scale, which is graduated in millimetres. The instrument can be used as an ordinary mastoid probe when the points are approximated.



Case of Acute Inflammation of the Middle Ear, with Empyema of Antrum in an Acellular Bone, with Dense Outer Antral Wall.

By ARTHUR H. CHEATLE, C.B.E., F.R.C.S.

MIDDLE-AGED lady. No history previous ear trouble.

May 20, 1920: First seen; deafness in and discharge from right ear. The week previously, while in bed with chill, had pain in right ear for four days, when it began to discharge and has continued to do so since. Pain ceased on rupture and had not recurred. On examination, slight purulent discharge; small perforation in posterior superior segment of membrane. Hearing very deficient, conversational voice heard at 4 in., whisper at 1 in. The tuning fork on the nose referred to the affected ear. No labyrinthine symptoms or signs. No mastoid signs. The left ear, nose, pharynx and nasopharynx normal. Temperature normal. No pain. Gentle syringing with boracic lotion ordered.

June 4, 1920: Still slight discharge with swelling of the membrane round small perforation, and marked deafness. Suspicion of œdema over the posterior superior deep meatal wall. No pain. Sleeping well. Temperature normal. No mastoid œdema or tenderness.

June 11, 1920: Condition the same, but meatal œdema had increased. No mastoid signs, pain or temperature. Operation in afternoon. Chiselling begun over antrum; after going through about $\frac{1}{4}$ in. of dense bone, antrum opened, and thick pus welled up. Antrum thoroughly exposed so that walls could be examined and the cells lining the inner aspect of the outer wall removed. No mastoid cells were present. The wound was lightly packed and perfect hearing and healing resulted.

I regret that a bacteriological examination was not made. I relate the case because it is not a common one and because of the signs and symptoms presented by acute suppuration in an acellular bone with a dense outer antral wall. Pain and headache were absent after the rupture of the membrane. The temperature was normal throughout. The discharge was scanty. There were no mastoid signs in the way of œdema or tenderness. The hearing was unusually diminished. Operation was undertaken because of the œdema in the deep meatus and the "hanging fire" of the case.

DISCUSSION.

Dr. DONELAN said he saw a case of acute otitis media with perforation two years ago. The patient had the usual treatment with drops, &c., for some months. He had continued well until he had sudden violent pain in and above the ear last week. There was no mastoid tenderness, but there was swelling of the inner end of the posterior wall. On this indication, a globular mastoid abscess of about 1 cm. diameter was evacuated after cutting through $\frac{7}{8}$ in. of the densest bone. The site of operation was swabbed with ether, a half-inch rubber drain placed in the meatus and the mastoid incision sutured.

Mr. TILLEY said it was exceptional for the temperature to be normal throughout a case of acute mastoid inflammation; possibly in Mr. Cheatle's case, absorption was prevented by the hardness of the bone. From the teaching standpoint, the most important sign in determining whether an operation on the mastoid should be performed was the swelling of the deep posterior meatal wall. It was almost pathognomonic of pus being retained under tension.

Case of Localized Suppurative Meningitis over the Motor Cortex following Acute Mastoid Suppuration; Drainage; Recovery.

By W. H. OGILVIE, M.S.

(Introduced by T. B. LAYTON, M.S., D.S.O.)

GIRL, aged 12. Admitted into Guy's Hospital.

History: Sudden pain in left ear, November 19, 1921. Otorrhœa commenced evening of November 20; ceased morning of November 21, but recommenced in the evening.

Condition on admission: Temperature 101.4° F., pulse 128. Slight purulent discharge from left ear. No swelling, redness, or superficial tenderness over mastoid. Tenderness on firm pressure over mastoid antrum and tip of mastoid process.

Operation: Antrum opened; found to contain granulations, but little pus. Mastoid process chiselled away down to dense bone surrounding lateral sinus. This bone was normal. Dura of middle fossa exposed; smooth, white and pulsating. Infected cells and carious bone in tip and along anterior border of mastoid process. Cavity treated with B.I.P. and sewn up over glove drain.

Progress: Temperature and pulse down for thirty-six hours, then commenced to rise. November 25, 10 p.m.: Temperature 103.6° F., pulse 140; mental condition normal. November 26, 10 a.m.: Temperature 101.4° F., pulse 120; very drowsy; did not appear to grasp questions; eye reactions normal; knee-jerks and plantar reflexes normal; no motor paresis.

Operation: Wound opened. No further bone disease discovered. Dura of middle fossa further exposed, but found normal. Lumbar puncture: Cerebro-spinal fluid under slight tension.

Examination of cerebro-spinal fluid: Direct films showed lymphocytes and polymorphs in excess. No organisms. Slight reducing power; albumin, 0.03 per cent.

November 27: Condition dull, as before.

November 28: Drowsiness and inability to answer, increased. Obvious paresis right arm and hand, and right side of face. Reflexes: Right abdominal diminished, Babinski on right side. Eyes react normally to light; no optic

neuritis. Dr. Symonds diagnosed localized meningitis over the left motor cortex, involving the face and arm area.

Operation: Mastoid incision continued upwards. Temporal muscle split in line of fibres. Bone removed freely in upward and forward direction. Dura smooth, not bulging. When incised, subdural space and brain normal in appearance. More bone removed, and dura opened to the Sylvian fissure: appearance still normal. Exposure continued upwards by removal of bone and incision of dura, till a roughly circular patch of purulent meningitis, the size of a two-shilling piece, appeared, overlying the middle third of the pre-central convolution. A glove drain was placed leading from this area to the lower angle of the wound, which was then closed by suture of temporal muscle and skin, leaving the dura open.

After-history; November 28 to 30: Several Jacksonian fits, involving eyes, right face and right arm. November 30 to December 11: No more fits; development of hernia cerebri; rapid improvement in mental condition; gradual improvement in paresis of arm and face. December 12: Right-sided sensory Jacksonian fit, involving arm, trunk and leg, and lasting about ten minutes; following this, marked astereognosis in right hand.

Operation: Upper end of wound opened about 1 in., and a No. 8 rubber catheter pushed under dura in direction of face-arm sensory area in post-central convolution. Escape of a collection of clear cerebro-spinal fluid. Glove drain put along track, and four-hourly eusol dressings applied.

Improvement continuous. Arm and face paresis cleared up rapidly. Hernia cerebri had subsided in four weeks. Left hospital January 25, 1922. Wound healed, quite well. Astereognosis had almost gone.

DISCUSSION.

Dr. SYMONDS said the diagnosis was an interesting problem. The girl had been ill only nine days, and when seen by him she had complete flaccid paralysis of the right arm, with fairly complete paralysis of the lower side of the face, some aphasia, and entire inability to put out her tongue to order. There was very little paralysis of the lower limb. The completeness of the paralysis of the upper limb, together with the fact that the motor functions of the lower limbs were practically intact, made it certain that there was a superficial lesion of the cortex, rather than a deep abscess; indeed nine days seemed scarcely long enough for abscess to develop. Lumbar puncture twenty-four hours previously revealed bacterial infection of the meninges, the excess of cells in the cerebro-spinal fluid consisting largely of polymorphs. The only reasonable conclusion seemed to be that there was a localized patch of meningitis over the motor cortex in the face-arm area. There had been no fits preceding the paralysis. The patient recovered very rapidly from the paralysis, the recovery being initiated by a series of Jacksonian fits, as if those fits represented the penultimate phase of the recovery. He had seen a similar occurrence once before, in a frontal affection, spreading from the frontal sinus. He would like to hear what was the method of spread from the mastoid to the meninges; also as to the prospects of recovery if she had been treated by simple lumbar puncture without drainage. Bacteria were not cultivated from the cerebro-spinal fluid.

Dr. A. A. GRAY (Chairman) said he had never seen a similar case: the spread from the mastoid was a most unusual one. There might possibly have been an abscess in the cells at the root of the zygoma: it would be a more direct route than the passage from the mastoid antrum up to that region.

Dr. WILLIAM HILL asked if the patient was shown as an instance of post-operative infection of meninges from exposing them at the operation, or whether it was assumed that the mastoid symptoms were there before the first operation.

Mr. NORMAN PATTERSON said that recently he had had under his care a boy upon whom, last November, an ordinary cortical mastoid operation had been performed; after that he was sent to the convalescent home, having apparently recovered. Three weeks ago, however, he returned to the hospital with a temperature of 104° F., and looking very ill. The ear was soundly healed, and there was no swelling over the mastoid. Vidal's test was negative, and the physicians could not find any cause for the temperature. A point of extreme tenderness was discovered, about the area of a sixpence, 2 in. behind the external auditory meatus. On opening the mastoid it was found to be quite normal. The sinus was pink in colour, and not apparently diseased. He followed the sinus backwards to a point opposite the tender area. Here he discovered a small abscess. This abscess was separated by a distance of at least 2 in. from the mastoid antrum. How had the infection spread there?

Mr. RITCHIE RODGER said there was often extension through the bone at some little distance from the mastoid antrum, but how the infection spread in this case it was difficult to say. He had seen a case of the kind referred to by the Chairman. After operating for acute mastoid, as the patient was not doing satisfactorily, he had opened again, and found the zygomatic cells had escaped attention. Before he had reached to the end of the infective process he had gutted out $\frac{3}{4}$ in. of zygoma. Later there were symptoms of temporo-sphenoidal abscess, and in his bone removal he had to get to the region opposite to the middle of the zygoma before arriving at an area of discoloured dura which led into the abscess of the temporo-sphenoidal lobe. In the case of a child 6 years old an abscess had formed in the sinus, well above the mastoid process. The medical attendant had opened this abscess, which was treated as a discharging tuberculous abscess. The oculist later reported double optic neuritis, and the physician regarded it as a case of tuberculous tumour of the brain. Later, symptoms of active meningitis occurred, and he (Mr. Ritchie Rodger) was asked to see the case. There was a two days' history of otorrhœa long before. He found the remains of a mastoiditis, which had resolved; there were a few tags of diseased mucosa in an enlarged antrum, but nothing more; yet there was an erosion of the squamous portion of the temporal bone, and leading from that there was an abscess of the temporo-sphenoidal lobe, containing four ounces of pus. The child recovered, but was still blind. Infection could spread from the superficial cells in front of or behind the mastoid process, and so to more distant parts of the brain.

Mr. COLLEDGE mentioned a case similar to Mr. Ogilvie's, though his (Mr. Colledge's) ended fatally. It was in a man aged 38, who had had otorrhœa all his life; he had been ill a week when he was sent to the hospital. On admission he had the classical symptoms of abscess of the temporal lobe; he was hemiplegic and aphasic. He had a cholesteatoma from which a track led to an abscess in the temporal lobe. It was successfully drained, but the patient developed a high temperature, evidently had meningitis, and died. At the necropsy an area of localized meningitis was found over the motor area of the size of a half-crown. He thought the two lesions masked one another, and the symptoms attributed to an abscess in the temporal lobe were due really to pressure from the meningitis over the motor area.

Mr. OGILVIE replied that he paid particular attention to the zygoma, which was normal. The infection was round the anterior border of the lower half of the mastoid. At both operations the dura was normal. It seemed difficult to think that bone spread had occurred. Mr. Jenkins had suggested to him that the spread might have been through the veins and diploë and through communicating veins, which sounded unlikely, as the flow of the blood stream was in the opposite direction; so that, for this to occur, it would be necessary for there to be thrombosis, and for infection of the clot to take place. The intervening bone was removed at the last operation, and it was healthy. He did not know whether the two infections could have been simultaneous, i.e., infection of the mastoid and of the meninges from the blood stream. The localizing symptoms in the brain did not appear until five days after the operation, and nine days after the onset of the ear symptoms.

Case of Unilateral "Nerve Deafness" in Disseminated Sclerosis, with Immobility of Opposite Vocal Cord.¹

By Sir JAMES DUNDAS-GRANT, K.B.E., M.D.

G. W., AGED 47, a subject of disseminated sclerosis, complains of deafness in the right ear which developed in 1916 and was preceded by "bilious attacks" with giddiness in 1915. Post-rotational nystagmus on both sides on February 3, well-marked but the past-pointing defective. Galton whistle heard on the affected side at the mark 2'8 only, but on the normal side at 1'6. Bone-conduction on the affected side very much diminished. Amongst other features are to be noted diplopia on looking down and complete immobility of the left vocal cord.

In a case resembling this (Hess, Dissertation, 1888, "Schwartz's Handbuch der Ohrenheilkunde," i, p. 507) there was found to be complete destruction of the left median acoustic nucleus by a focus of sclerosis, while the same nucleus on the right side contained a considerable number of diseased ganglion cells. The left auditory nerve was sclerotic in a considerable part of its extent; the deafness in this case was on the left side. Moos is of the opinion that disturbance of hearing in disseminated sclerosis depends most probably upon a sclerotic degeneration of the auditory nucleus and nerve stem.

Postscript.—On February 17, 1922, patient was submitted to re-examination with the cold air test, with the following results: On the *right* side, there was practically no nystagmus, giddiness or past-pointing; on the *left*, cold air after forty-six seconds produced active nystagmus to the right, past-pointing (specially marked with left hand) to left, and falling to the left. The palate is symmetrically paretic. Left vocal cord still completely paralysed.

¹ Patient shown at the Section of Laryngology on February 3.

Feb. 14

Section of Otology.

President—Dr. A. LOGAN TURNER.

Cases of Functional Deafness.

By DAN MCKENZIE, M.D.

WHATEVER hesitation may be felt in placing reliance upon the vestibular reactions in the diagnosis of functional or hysterical deafness, no dubiety can exist when absolute nerve deafness is suddenly recovered from. This absolute test is illustrated in the following cases.

Case I.—Years ago an old lady came to the Central London Hospital complaining that she had been stone-deaf for several weeks. Examination bore out her statement: the hearing for the voice and also for the tuning-fork by air and by bone was entirely absent. Both meatuses were blocked with impacted cerumen; this was removed by syringing. As soon as the meatuses were cleared the patient surprised us by saying that she could then hear, and on examination we found that while there still remained a certain amount of nerve-deafness, probably senile, she was able to hear the moderately loud conversational voice. Obviously, the obstructive deafness superimposed upon the slight nerve-deafness had induced, by suggestion, a complete loss of all sound perception.

Case II.—Male, aged 56, a man of unusual intelligence. Operated on for sessile osteomata occluding both external meatuses and causing deafness, especially severe on the left side. On this side, where the meatus was completely blocked, the auricle was reflected forward after a post-aural incision, and the bony meatus was enlarged with gouge and mallet. On the right side, the operation was undertaken without displacing the auricle. Left facial paralysis, which is proving now to be transitory, followed the operation, and the reactionary swelling of both meatal walls led to a continuation of the obstructive deafness. Ten days after operation the patient suddenly became absolutely deaf to all sound in both ears, without any spontaneous nystagmus, although a little giddiness was complained of. The caloric reactions were negative, but the swollen and occluded meatuses forbade any reliance being placed on this test. In view of all the circumstances, operative and clinical, a diagnosis of functional deafness was made, and a good prognosis hazarded. A month later the patient was able to hear the tuning fork by bone conduction in both ears, and a few days later when in the street the hearing in the right ear suddenly returned "with a crash" which made him stagger. At the

present time (six months after operation) the hearing in the right ear is normal, save that loud sounds are felt to be unpleasant. There is still deafness, but only obstructive deafness, in the left ear.

DISCUSSION.

Mr. JENKINS asked where the osteomata were ?

Mr. STUART-LOW said he presumed that the osteoma was of the sessile and ivory type and situated far in the meatus on the posterior wall near the membrana tympani. A bony growth of such a character would necessitate very heavy hammering for its removal, and the probability was that this hammering, by giving rise to an effusion into the aqueductus Fallopii, explained the occurrence of the temporary facial paralysis that took place. In order to obviate what he considered very dangerous brain-shaking even in an ordinary mastoid operation, he had invented lead-headed chisels and mallet, the use of which materially lessened such concussion.

Mr. VLASTO said that when he was in the Navy, he was one day talking in the ward-room when he suddenly noticed he had become very deaf in one ear. He came to town and consulted three surgical colleagues, Mr. Cheatle, Mr. Scott and Mr. Cleminson. Mr. Cheatle diagnosed his case as one of neurosis, as there was definite absence of nerve conduction on one side, and said the tests were not conclusive. The deafness in that ear, with a singing sound, persisted three months. Then hearing gradually returned, and it was now normal. If the tests in these cases were not conclusive, the condition might be a neurosis.

Dr. MCKENZIE (in reply) said that in regard to the facial paralysis the conclusion to which he had come, on a review of the case, was that he had probably gone a little astray. It was difficult to locate the facial nerve from the meatus, and especially from a deformed meatus, without the guidance of the antrum. In doing the radical mastoid operation one found the aditus, and knew from it the position of the nerve. But in the meatus, with its curve forward and the shelving forward of the posterior wall the facial nerve lay further forward and nearer to the chisel than would be supposed. He thought that in future the simplest thing in these cases would be to make an opening into the antrum first, and having thus localized the antrum and the facial nerve, one could estimate when, in the bony meatus, the region of danger was entered. Some time ago he did some measurements on the facial nerve and found that it lay only 2 or 3 mm. behind the posterior wall of the meatus, and a little deviation in an operation on the bony meatus would bring one into the facial canal. The osteoma was of the sessile variety, the whole of the posterior meatal bony wall being advanced; the growth was so near the membrane that the latter was involved. He could not see the membrane before operating as the meatus was completely occluded.

Dr. LOGAN TURNER (President) said he thought that if the facial paralysis was due to effusion into the nerve sheath from the act of chiselling as suggested by Mr. Stuart-Low, there would be more cases of facial nerve paralysis after the performance of mastoid operations.

Case of Extradural Abscess, Meningitis and Cerebellar Abscess ; Recovery.

By E. D. D. DAVIS, F.R.C.S.

E. C., A WOMAN, aged 26, was admitted to hospital on December 4, 1921, with a diagnosis of meningitis and suppuration of the left ear. The left ear had discharged pus off and on for two years, and fourteen days before admission

the ear ached and commenced to discharge. Her doctor saw her on November 26, and treated her for an abscess of the ear. On December 1, the patient had severe headache and vomited for ten hours, and became very noisy and restless.

The patient was first seen three days later when she had attacks of severe left occipital and frontal headache, but answered questions clearly. Temperature 100.8° F., pulse 92, respirations 24. Patient looked very ill. Pupils contracted with normal reactions. No nystagmus. Face normal. Rigidity of neck, slight tenderness of temporal fossa. Neither tenderness of mastoid nor tenderness along jugular vein. Left ear showed offensive purulent discharge with granulation tissue in the roof, and an attic perforation. Warm irrigation of left ear produced vertigo, but no nystagmus. Hearing: Conversational voice, 6 ft.; bone conduction normal; Weber + on left. Lumbar puncture: Turbid fluid reduced Fehling's solution; neutral reaction; numerous pus cells, no organisms.

Left radical mastoid operation immediately performed. A large quantity of foul pus in the lateral sinus groove, and extending forwards between the dura and petrous bone to the aqueduct of the vestibule and internal auditory meatus. Lateral sinus covered with granulation tissue, but collapsed on plugging the upper end, and afterwards, when opened, was found to be patent with no thrombosis. The posterior fossa was exposed right up to the labyrinth and facial canal, and as there was a sinus in the roof of the middle ear, a large area of middle fossa dura was exposed. Both areas of dura mater appeared to be normal. The wound was left open with a view to opening the dura later.

Patient was much better, headache relieved. Temperature normal, pulse 88 for eight days, but on December 12 she complained of severe headache and vomited. Lumbar puncture: Clear sterile fluid. Patient drowsy, nystagmus to left, optic discs congested, no œdema. Definite inco-ordination, left hand, dysdiadokokinesis, grasp weaker than right. Left tendon Achillis and knee-jerk increased; later, ankle clonus.

Diagnosis by Dr. Adie: Posterior fossa abscess.

Posterior fossa trephined behind lateral sinus, and after repeated exploration some foul-smelling pus, suggesting the presence of *Bacillus coli*, was evacuated. Puncture of the dura in front of the lateral sinus produced no pus. After this operation, patient recovered slowly, but had one set-back when the drainage tube of the posterior fossa was removed too soon, and had to be replaced. The pus from the extradural abscess produced pneumococci and *Bacillus proteus*. Lumbar puncture fluid on December 20 produced pus, pneumococci and *Bacillus proteus*. The labyrinth tests, which will be carried out after complete convalescence, will be reported.

DISCUSSION.

Dr. LOGAN TURNER (President) asked whether Mr. Davis thought the cerebellar abscess was there all the time, the symptoms being masked by the meningitic symptoms. He also inquired why the exhibitor did not in the first instance explore the cerebellum through the area of the bone internal to the sinus, rather than behind it. The patient had an extradural abscess between the labyrinth and the sinus.

Mr. G. J. JENKINS said that the result in this intricate case was excellent. He recognized the difficulty of making a complete examination under these conditions. Mr. Davis spoke of pus cells being found in the cerebrospinal fluid; he (Mr. Jenkins) supposed that polymorphonuclears were meant. This was a very important finding, because in cases of cerebellar abscess in which there was no definite meningitis, he had usually found the cell count showed lymphocytes in greater proportion: he now had a

case with 97 per cent. lymphocytes, 3 per cent. plasma cells, the protein in the fluid 0.04 per cent., and no change in the chlorides. In this case the count was that of purulent meningitis—a large pus-cell count. He would like to know the sugar estimate, the proteid content, &c. The case raised a very important point in regard to cerebellar abscess. There were two kinds of abscesses in the posterior fossa: those involving mainly the meninges, and those involving the cerebellum itself. Was the pus in close relation to the anterior wall of the fossa, in relation to the lateral sinus, or in the substance of the cerebellum?

Mr. J. F. O'MALLEY said that Mr. Davis's case was one of great interest. Did Mr. Davis consider that the route of the infection was directly into the posterior fossa, along the lateral sinus? Or did he think it was translabyrinthine? From the examination of the ear he (Mr. O'Malley) thought it probably was not translabyrinthine, as the bone-conduction was evidently good. In this case, however, there appeared to have been two intracranial complications. At the first operation there were pus cells, and on the second occasion the fluid was clear and sterile, and from that, one was inclined to assume that the meningitis was not dependent on the abscess, the abscess being probably the antecedent condition, and the meningitis being the acute manifestation which brought the patient for advice.

Sir WILLIAM MILLIGAN agreed with Mr. O'Malley that the abscess had probably been present all the time, and that it was the lateral sinus condition which brought the patient immediately under observation. In his experience, the majority of cases of cerebellar abscess had a certain amount of meningitis, though it was a question as to what was meant by meningitis. No organisms were found in this case, and it was important to know whether the cells found were pus cells. He did not regard a case as purulent meningitis unless organisms were found in the fluid. In the present case there had been much localized irritation, and some lymphocytosis in the theca. He would raise the question whether, in the case of an extensive cerebellar lesion such as this, it was not advisable to make a counter-opening for drainage. When there was a septic process spreading along the petrous bone to the internal auditory meatus, and the cerebellar abscess was opened through that route, was it not safer to make an opening behind the lateral sinus and do counter-drainage? In many cases drainage was often very imperfect, therefore recently he had carefully followed the counter-drainage plan, and felt sure his results had been better. He did not think it added to the risks. The anatomy of the cerebellum was so peculiar, as also was the way in which the tentorium acted, that drainage of these cases was even more difficult than in the case of cerebral abscess. The result in Mr. Davis's case was a surgical triumph. He (Sir William Milligan) had had a very similar case a year ago, but with the addition of an extradural abscess, a cerebellar abscess with thrombosis, and also, at any rate clinically, septic cerebellitis. For days the patient had hovered between life and death, but finally recovered. Much bone was taken away, and counter-drainage was employed; a large cerebellar hernia resulted, which had now disappeared, and the patient's mental condition was now good.

Sir JAMES DUNDAS-GRANT endorsed Sir William Milligan's contention as to the value in these cases of making a counter-opening to secure good drainage. In a case which he had shown before the Section many years ago there was thrombosis of the lateral sinus in which ligation was done, but symptoms of cerebellar abscess soon supervened, the arm on that side becoming quite flaccid. He opened the abscess in front of the sinus, but pus poured out continuously until he made a counter-opening behind it. After that it dried up with extraordinary rapidity, and the patient went through the War as a driver in the Artillery. In the present case the subdural abscess seemed to have originated in the sacculus endolymphaticus. In view of the difficulty in diagnosing these cases, Mr. Davis deserved congratulation.

Mr. CLEMINSON said the question asked as to why Mr. Davis did not explore in front of the lateral sinus, reminded him of an unfortunate experience in the case of a girl upon whom he operated for cerebellar abscess, the dura being opened in front of the

lateral sinus by the removal of a triangle of bone. A drachm of pus was in that way evacuated, but the patient almost immediately stopped breathing, though the heart continued to beat. Attempts made for two or three hours to induce respiration failed, although the heart still continued to beat during this time. In trying to find an explanation of this he had wondered whether the œdema of the opposite side of the cerebellum, when the pus had been evacuated, had pushed the medulla against the edge of the foramen magnum and so interfered with the respiratory tract; and whether, therefore, in a similar case, it might not be wise to do a decompression operation on the opposite side first. At the post-mortem examination nothing more was found.

Mr. SMALLEY said he had had a case in which a boy was brought in comatose, with a lesion on the right side; evidently he had an intracranial abscess. Ether was given, and he (Mr. Smalley) had just begun the operation—no bone had been removed—when the breathing stopped, and for that reason he thought it was a cerebellar abscess. Artificial respiration could not be carried on, and he therefore did a "rush" operation, so that in five minutes he had opened the cerebellum and let out over an ounce of pus. Breathing then recommenced and it went on straight away. He lived three or four days after, and then died of cerebellitis. The stoppage of respiration in that case was due to pressure on the medulla, a pressure which was already there.

Dr. LOGAN TURNER (President) referred to the fact that Sir William Milligan years ago had drawn his attention to the value of doing lumbar puncture when confronted with such difficulties as had just been mentioned. When breathing ceased, lumbar puncture could be done more quickly than the completion of the operation.

Mr. E. D. D. DAVIS (in reply) said he did not know why this patient had recovered, unless it was because it was a pneumococcal and *Bacillus coli* and not a more virulent streptococcal infection. There was a perisinus abscess which tracked forwards to the internal auditory meatus. The wall of the sinus was covered with granulation tissue and the cerebellar abscess was discovered near the bulb of the lateral sinus. He believed that infection arose from the perisinus abscess and not through the labyrinth, because the hearing of the affected ear was good, and warm irrigation produced vertigo but no nystagmus. At the time of the first operation there was no clinical sign of cerebellar abscess, and the neurologist, Dr. Adie, at his second examination, eight days after the first operation, discovered definite signs of a posterior fossa or cerebellar abscess. He (Mr. Davis) said it was presumed that this abscess had developed later and was not present at the time of the first operation. The dura was punctured and explored in front of the lateral sinus with a negative result; but repeated exploration through a large opening in the posterior fossa behind the lateral sinus produced a quantity of foul pus. The posterior fossa was opened behind the lateral sinus because a more thorough exposure of the brain was obtained and, in a case where the diagnosis was doubtful, exploration and access was much easier, and this was the route preferred by Mr. Waggett. In his (Mr. Davis's) experience, the cerebro-spinal fluid of obvious cases of meningitis confirmed by autopsies was frequently sterile and contained no organisms in spite of the presence of pus.

Case in which both Meatuses are closed by False Membranes.

By W. M. MOLLISON, M.Ch.

C. J., FEMALE, aged 42, has always been completely deaf in the left ear, and has used a trumpet for the right ear for nine years. She thinks the hearing is getting worse in the right ear. Both meatuses end blindly, probably the result of suppuration.

Lateral Sinus Suppuration, with an Unusual History.

By NORMAN PATTERSON, F.R.C.S.

BOY, aged 11; admitted November 23, 1921. History of watery discharge from left ear since September 9, 1921.

On examination: Left membrane inflamed, Shrapnell's membrane bulging, small perforation in attic region. Protrusion forwards of auricle, slight tenderness over mastoid. Incomplete mastoidectomy by Mr. W. Morris. Pus found in mastoid cells.

December 9, 1921: Sent to Convalescent Home.

Readmitted January 16, 1922: history of a swinging temperature for a fortnight. January 11 to 12: Temperature ranging from normal to 104° F.: no rigors.

On examination: Tympanic membrane found normal, mastoid region normal, no tenderness. Very tender spot size of a sixpence situated over lateral sinus, 2 in. behind external auditory meatus. Left wrist: joint painful on movement, and slightly swollen. Examination by physician negative. Widal reaction negative.

Operation by exhibitor. Before commencing operation a cross was made with a chisel over the tender area, so that after reflecting the soft tissues the exact region could be located. Mastoid opened up, healthy. Sinus exposed, pink in colour; followed backwards as far as tender area, still pink. Sinus opened in usual position, contained dark blood-clot. Clot followed backwards. Exactly opposite tender area sinus contained about half a drachm of pus. Clot extended backwards nearly to the torcular herophili. Clot entirely cleared out until free hæmorrhage occurred from proximal end. Internal jugular ligatured in neck. Patient's temperature remained irregular until February 1. After that it became normal, and has remained so. Boy has made a good recovery.

Examination of the pus showed streptococci to be present. This case seems to be a very unusual one, first, because all signs of mastoid suppuration had cleared up before the second operation, and also as regards the formation of an abscess in the sinus at such a great distance from the mastoid.

Very Extensive Infection of Lateral Sinus and Jugular Vein, followed by Recovery.

By NORMAN PATTERSON, F.R.C.S.

GIRL, aged 17. Six years ago scarlet fever, followed by right otorrhœa and deafness. Admitted November 19, 1921, suffering from frontal headache, vomiting, rigors and constipation.

On examination: Dull, speech slow, sleeping most of the time. Headache mostly occipital. Marked deafness in the right ear. Several rigors. Seen by exhibitor in medical ward and immediately operated on. Mastoid operation performed. Lateral sinus exposed, and pus evacuated. Abscess extended nearly as far as torcular herophili. Free bleeding obtained from this end of sinus. Internal jugular exposed in neck, and thrombosed. Attempt made to get below clot by dividing lower attachment of sternomastoid, but found impossible. Ligature passed round vein at lowest possible point, but here clot was present. In spite of the fact that there was a clot in the vein below the ligature, the patient made an excellent recovery.

Pus from sinus showed streptococci.

DISCUSSION.

Mr. G. J. JENKINS, referring to the case of lateral sinus suppuration, with unusual history, said he had seen more of these quiet infective cases in the last two or three years than in the rest of his experience; there had been one at King's College Hospital almost identical not long ago. He advised that the patient should be kept under observation. There had been slight discharge from the ear, but when examined the tympanic membrane had healed. Mr. Cheatle operated, and found the lateral sinus was thrombosed, and there was an extensive abscess formation in the sinus. The patient recovered. It reminded him (Mr. Jenkins) of three or four cases he had seen of healing in the middle ear, the infection persisting in some other part, and causing abscess. One patient he saw with acute middle-ear trouble in May, ear trouble again in August, and a further occurrence in September, and again it healed. The patient had never been quite comfortable; had always had dull occipital pain. He explored in October and found the air cells normal and the mastoid healthy, but there was an extradural abscess in relation with the lateral sinus. In regard to the case of infection of lateral sinus and jugular vein, the lesion was very extensive, and reminded him of a similar case which had been in the hands of the physicians for two or three days. The patient had lateral sinus thrombosis, the lateral sinus being affected in the lower part. The internal jugular vein was involved to the junction of the great vein in the thorax. In the middle of the neck no jugular vein could be found, but there was an abscess in the place of the internal jugular. At the time of the operation this patient had residual abscesses from the germinal infection, one in the forehead, another in the arm. Streptococcus was found. There was complete recovery. He thought it did not matter what the particular organism was; what did matter was, what was the resistance of the patient to the bacterial infection.

Mr. STUART-LOW said that it was pretty well agreed that cases such as these were almost always post-influenzal, and that there was in influenza a great tendency to the formation of clots in the blood-vessels owing to profound changes in the blood. He had often found it very helpful to give quinine for at any rate forty-eight hours before operating, and in at least two cases recently the patient had recovered without operation, the clots having resolved. Quinine in considerable doses apparently had the double action of aiding the absorption of the blood-clots and preventing their septic invasion and pus formation.

Dr. LOGAN TURNER (President) said that the results in Mr. Patterson's cases were excellent. In the first of them probably some mastoid cells were left in the postero-superior angle after the Schwartz operation. It was necessary to expose the mastoid very carefully at its base, in case a cell in that situation might be overlooked; extension from there into the sinus was possible. Did Mr. Patterson ligature the internal jugular in all these cases? Its necessity in the second case was obvious, but was there any bleeding from the lower end in the first case?

50 Patterson: *Infection of Lateral Sinus and Jugular Vein*

Mr. NORMAN PATTERSON replied that there was no bleeding at the lower end of the sinus in the first case, and he fully expected to find a thrombus in the jugular. In answer to Mr. Stuart-Low, he feared that if he had relied on quinine for the treatment of these patients they would not have been here to-day.

Section of Otology.

President—Dr. A. LOGAN TURNER.

A Note on the Resonating System in the Cochlea, with Demonstration of a Model, illustrating the Action of a hitherto neglected Factor.

By GEORGE WILKINSON, F.R.C.S.

ASSUMING that the basilar fibres form a series of resonating elements, they will vibrate in accordance with the formula

$$n = \frac{1}{2l} \sqrt{\frac{t}{m}}$$

when n = number of vibrations per second, l = length of fibre, t = tension, and m = mass of unit length.

We know that the basilar fibres are differentiated as to length, i.e., they increase uniformly in length from the base to the apex of the cochlea.

That they are also differentiated as to tension is extremely probable, in view of the fact that they are attached to the outer bony wall of the cochlear galleries by a fibrous structure, the spiral ligament, which diminishes progressively in bulk and density from the base to the apex (Albert Gray).

My contention is that they are also differentiated progressively as to mass. In stringed instruments, e.g., the string-board of a piano, the bass strings are increased in mass by being wrapped with copper wire. They are "loaded." Similarly the basilar fibres are loaded by a mass of cochlear fluid which moves as they move.

Suppose a small transverse sector of the basilar membrane to move from its central position towards the scala tympani. Its movement will displace a certain amount of fluid, and the displacement will travel along the scala till the membrane closing the round window is bulged to a similar extent. A quantity of fluid equal to that displaced by the movement of the sector of the basilar membrane will be displaced across each cross section of the scala from the level of the sector to the round window. A similar quantity of fluid will be displaced in the opposite direction in the scala vestibuli. The result will be the same whether the movement originates from an impulse applied to the stapes (forced vibration), or vibratory movements of the sector itself (free vibration). No sector can move without a simultaneous movement of the cochlear fluid. The mass of the fluid moved is quite definite and invariable for each sector. It does not depend on the amplitude of the movement. This only affects the amplitude of the displacement of fluid, not its mass.

The mass of fluid moved may be defined as that of a double column of fluid, the base of which is equal in area to the surface of the sector, and the length to the sum of the distances of the sector from the round and oval windows. The mass of the sector itself, with its adherent organs and cells, will be inconsiderable as compared to that of the fluid which constitutes the load. Its specific mass will not differ greatly from that of the fluid in which

it is immersed. The whole mass may therefore be reckoned in terms of that of the column of fluid. The fluid itself has a specific mass not greatly differing from water. In centimetre-gramme units the mass will be equal to the volume of the double column.

As m is mass of unit volume, it will not vary with the variations in transverse breadth of the basilar membrane. For sectors of the same width it will vary only with the distance of the sector from the round and oval windows. In other words m progressively increases from base to apex of the cochlea. This will cause a differentiation in the periodicity of vibration of the sectors in the same sense as the variations in length and tension. The load is greater on the sectors that are longest and least tense.

The formula for vibrating strings

$$n = \frac{1}{2l} \sqrt{\frac{t}{m}}$$

when applied to the transverse sectors of the basilar membrane becomes

$$\frac{1}{2l} \sqrt{\frac{t}{\bar{a}b'}}$$

where \bar{d} is the sum of the distances of the sector from the round and oval windows, and b is the width of the sector.

The model shown is a brass box in two chambers (scala vestibuli with ductus cochlearis and scala tympani). A window closed by a rubber membrane opens into each. To one of the membranes is attached a small wooden plunger, the "stapes." The chambers are divided by a "basilar membrane" formed of strands of fine brass wire stretched transversely, and plastered over with fine paper saturated with formalized gelatine. The tension of the threads has been regulated by suspending from them a graduated series of weights, calculated according to the above formula. The whole is completely filled with water. It is set in resonant action by tuning forks applied to the stapes. Finely powdered blue enamel is used as an "indicator" to show the level of the vibrating segment. The compass of the strings is four octaves—32-64, 64-128, 128-256, 256-512 D.V.¹

We can now recognize a threefold differentiation of the basilar fibres for length, tension and mass. The variation is in the same sense in each case. The fibres that are shortest are tightest and lightest. Those that are longest are loosest and most heavily "loaded."

Their periodicities must therefore extend without a break over a considerable scale.

We know approximately the amount of variation of the factors l and m . The variations of l are sufficient alone to give a range of rather more than 1 octave, those of m rather more than 2 octaves. The remaining 6 to 6½ octaves of the audible scale therefore depend on variations of t , i.e., the tension exercised by the spiral ligament. Now it requires a 4,096-fold variation of tension to give a range of 6 octaves. If one compares the slight and sparse appearance of the fibres of the spiral ligament in the apical coil with the bulky, dense, compact appearance of the ligament in the basal turn, so large a variation of t appears not improbable.

In an earlier paper² I have given the calculations for the maximum and minimum tensions required to give a range of 10½ octaves in the cochlea.

¹ The model will be fully described later in the *Journal of Laryngology*,

² *Journ. Laryng. and Otol.*, 1921, xxxvi, pp. 557-566.

The figures obtained are not impossible. The maximum works out to about one quarter of the breaking strain of tendinous fibres of the same tenuity as the fibres of the basilar membrane.

If it be admitted that the fibres of the basilar membrane possess this degree of differentiation, it follows that they *must* vibrate in sympathetic resonance with any oscillatory impulse applied to the stapes, whose frequency falls within the limits of their range of periodicities. Whatever the connexions of the fibres may be within the cochlea, it would be an extremely difficult matter to suppress their resonant action. I am unable to conceive of any functional significance for this threefold differentiation and consequent range of periodicities, other than for the purpose of sound analysis by resonance.

DISCUSSION.

Professor URBAN PRITCHARD said this subject had always been interesting to him. For two reasons, he had never been able to see that the basilar membrane had anything to do with appreciation of sound. First, because the fibres there were not free, as one would expect them to be if they were to be used as vibratory agents; and secondly, because in the upper part of certain cochleas there was no basilar membrane at all, but there was an organ of Corti.

Dr. A. A. GRAY said that this demonstration was the most important contribution which had been made to the subject for years, and that this was the first time he had seen a satisfactory instrument illustrating the movements of stretched membranes in response to sound vibrations. Ewald, about thirty years ago, tried to show instruments to demonstrate the subject in this way, but he was not very successful. Mr. Wilkinson's instrument indicated not only great patience, but also great mechanical ingenuity. Many attempts had been made to disprove the resonance theory, but none had been experimental. Rutherford and Ewald did not succeed, and the late Dr. Waller only advanced theories. The present apparatus was the first to demonstrate the membrane vibrating in sympathy with harmonic vibrations; it was a great step forward in the solution of this problem concerning the action of the cochlea. Mr. Wilkinson had shown what was the third factor in determining the movements of the basilar membrane; previous factors determined were the length of the fibres, then the tension, and now there was added the factor of mass, which would correspond with the thickness of strings in air vibrations. These all pointed to analysis by sympathetic resonance. It was true that when he (Dr. Gray) wrote on the subject in the year 1900, the Helmholtz theory was looked upon askance, and the "telephone" theory was holding the field. When it was found that there was no evidence that nerve impulses corresponding in frequency and character could be carried up nerves and analysed, the theory was abandoned. Now we were brought back to the view of sympathetic resonance as the means of analysis. Some writers had assumed that there were pressure patterns in the basilar membrane; but as he (Dr. Gray) had pointed out¹ it had not been shown how a pattern could be formed, except by sympathetic resonance.

He had also spoken of a point which had some bearing on the matter: when Mr. Tweedie had referred to the evolution of the cochlea:²—It might be considered strange that this marvellous mechanism of sympathetic resonance was present in the cochlea, and that sound waves were analysed by this means. Regarded, however, from the standpoint of evolution, it would be surprising if there were any other method by which we could appreciate minute disturbances in the air. The case was different when animals were living in an incompressible medium, e.g., water; for it would not matter much whether the movements were harmonic, or not. But when the medium was changed from water to air, which was compressible, a different condition of matters arose, and, so far as he knew it, it was only vibrations which were harmonic in character that could be carried any distance through a compressible medium without undergoing

¹ *Journ. Laryng. and Otol.*, 1921, xxxvi, pp. 584-594.

² *Proc. Roy. Soc. Med.*, 1922, xiv (Sect. Otol.), p. 23.

loss by dispersion; i.e., vibrations in which the velocity of the particle varied with the size of the angle which represented the time. That was the only way in which movements of small magnitude could be conducted to any great distance. Those animals then which could appreciate these harmonic vibrations were the most likely to survive. Mr. Sydney Scott had said, some months ago, that he had examined the hearing of amphibians, and had found response to a few low notes only, not to the higher notes. Therefore, with amphibians the power of response to harmonic vibrations was probably very slight. Why should not the frog and other amphibians respond to high notes if their basilar membrane was equally perceptible to notes high and low? So that, from the standpoint of evolution, far from this view being an unlikely one, it was the most probable; he (Dr. Gray) would be surprised if the analysis could be made in any other way than by means of sympathetic resonance.

He agreed with Professor Urban Pritchard that the fibres were not able to vibrate individually, but that did not constitute an objection to the theory. It did not matter so much whether the fibres vibrated separately, the important matter was the point at which the maximum amplitude occurred. When pressing a point into the skin, numbers of nerves might be stimulated, but one predominant sensation at a point caused the others to be ignored.

Mr. RITCHIE RODGER asked whether Mr. Wilkinson found that he was compelled to enlarge the helicotrema in the model, and if so, whether a combination of the two theories would be found to meet the case. He presumed that the model demonstrated represented a stable condition between the scalæ, whereas in Nature there was the displaceable basilar membrane, which according to the opponents of the resonance theory conveyed the vibrations. If there were a displaceable membrane, there was no need for such a large helicotrema.

Sir JAMES DUNDAS-GRANT said a step had been taken in the same direction by Professor McKendrick, who had made a large model, to represent the scalæ of the cochlea; inside this model, at varying distances, he had placed flapping valves, "tuned" to certain periodicities. When he gave the appropriate number of impulses at the end corresponding to the fenestra ovalis, the attuned valve picked it up and vibrated in resonance with this but not with other numbers. What Mr. Wilkinson had now shown was one of the finest illustrations of science. Sounds had been shown to be analysed in the cochlea, but they had to be combined in the brain so that a compound sensation might be appreciated by the individual. The advance made by Mr. Wilkinson was so great as to be almost staggering.

Mr. WILKINSON (in reply) said that he had received kind encouragement from Sir Arthur Keith who had lent him sections, and had taken an appreciative interest in the model. He (Mr. Wilkinson) had not seen Professor McKendrick's model, but had read a brief description of it. Reference had been made to the central function in analysing sounds. The analysis of sound was ultimately a matter of measuring short periods of time, and the measuring of time was essentially mechanical. There was no known way of measuring time except by reducing it to measurements of length by means of uniform motion; either linear, circular, or pendular motion. Thus the objection to the supposition that sounds could be analysed centrally was fundamental. He (Mr. Wilkinson) considered that the central mechanism was concerned rather with synthesizing or blending simple sensations into perceptions, than with analysing compound sensations into their elements. He feared he had not made himself clear as to the helicotrema. In the model this was not relatively larger than in the cochlea, but when he started experimenting with the model he feared to make it sufficiently large, lest too much of the energy of the vibrations should be dissipated. He found, however, that unless it were made reasonably large, differential response was imperfect. In answer to Dr. Gray's question as to whether there was any way of forming sound patterns except by resonance, Mr. Wilkinson said he thought there was no proof that this was possible. The inertia of the fluids had to be reckoned with, and this point had received insufficient consideration. Ewald's artificial cochlea misrepresented the basilar membrane in every particular; there was no differentiation in length, and the windows were placed in a haphazard relation to the basilar fissure. Whatever differentiation of tension

there might be was fortuitous, such as might result from unequal contraction of the rubber film in drying. If one differentiated in a haphazard way one got haphazard results. The "travelling bulge" theories of Meyer and Ter Kuile might serve for an elaborate analysis of a single wave impulse, but with a series of impulses it became resonance pure and simple. Only the section of the basilar membrane in tune with the period of the impulse would regularly make room for the fluid displaced by the thrust of the stapes.

Specimen of Internal Auditory Meatus dilated and occupied by New Growth, involving the Auditory Nerve.

By Sir JAMES DUNDAS-GRANT, K.B.E., M.D.

THE case of cerebello-pontine tumour from which this specimen is taken presented many features of left-sided cerebellar disease. The labyrinthine tests (taken under considerable difficulties on account of the patient's condition) gave the following results: Very fine spontaneous nystagmus to the right and very coarse to the left. Bone conduction: Right side, normal; left side, shortened; on the vertex heard only in the right ear. Left ear, quite deaf. Caloric tests with cold air (normal about 30 seconds): Left, horizontal canal, no nystagmus after 60 seconds; left, vertical, very doubtful slight nystagmus at an uncertain interval (probably merely the original spontaneous nystagmus). Right, horizontal, nystagmus after 30 seconds (normal); right, vertical, no nystagmus after 90 seconds. There was an intense optic neuritis and among other nerve lesions paralysis of the left sixth nerve which was reduced to the size of a thread.

The specimen shows dilatation of the internal auditory meatus, containing a portion of the tumour. It has unfortunately been considerably damaged in the removal.

The involvement of the nystagmus-producing fibres from the opposite vertical canals in accordance with the current views suggests a cerebello-pontine tumour in the pons; these fibres are believed to run near the middle line and, therefore, to be liable to compression by a tumour pressing on the parts. The corresponding track for the horizontal canal (of the sound side) lies in the medulla and farther from the middle line.

Dr. A. A. GRAY said tumours of the cerebello-pontine angle would have more attention in the future. Every one must have missed cases of it in the past through not knowing the symptoms. More light was now being thrown on the cases. One of the difficulties in diagnosis was due to the long duration of the symptoms.

Malignant Disease of Temporal Bone with Involvement of Cranial Nerves.

By J. S. COLLIER, M.D., and LIONEL COLLEDGE, F.R.C.S.

PATIENT, a man, aged 40, was sent up on February 21, 1922, with headache and pain in frontal region and right facial paralysis. He was quite deaf in the right ear, with complete loss of bone conduction: the deep part of the meatus was filled with a red mass. The man was rather drowsy with sluggish but equal pupils. No optic neuritis. On opening the mastoid process it was found full of growth. The facial nerve was exposed in the aqueduct for about

a quarter of an inch. There was a fistula into the external semicircular canal. The canals were found full of growth and removed. The dura mater of the middle fossa was covered with growth over an area about one inch by half an inch. The ossicles were surrounded with growth, but there was no pus anywhere. Further exploration showed growth extending between the inner and outer tables of the skull in every direction. The operation was therefore abandoned. The patient was at first much relieved of his pain and headache, but this has since recurred. About three weeks after the operation a boss the size of a crown piece appeared on the right frontal bone and another just above the right ear. Difficulty in swallowing and inability to protrude the tongue followed soon after, the right half of the palate and both sides of the tongue, but especially the right, being paralysed. The right vocal cord was paralysed. Weakness of the sterno-mastoid and trapezius have since appeared. It seems therefore that the cranial nerves on the right side from the seventh to the twelfth are involved. At present some stiffness of the neck suggests extension to the occipito-atlantal joint. Unfortunately the pathologist was unable to give a definite opinion on the nature of the tissue and fragments of temporal bone submitted to him for examination.

Facio-hypoglossal Anastomosis.

By LIONEL COLLEDGE, F.R.C.S.

PATIENT, a girl, aged 16, was operated upon a year ago for mastoid disease and meningitis secondary to suppuration in the labyrinth of the right ear. The cochlea was opened and found full of pus. Cerebro-spinal fluid was turbid and after six lumbar punctures became clear and sterile. The *Staphylococcus aureus* was cultivated from the cerebro-spinal fluid on three occasions. The facial nerve was not seen but a complete facial palsy resulted. As this showed no improvement after six months and the reaction of degeneration was present, the hypoglossal nerve was divided and the central end anastomosed to the peripheral end of the facial. The peripheral end of the hypoglossal was anastomosed to a slip from the spinal accessory. Some wasting of the right half of the tongue followed, but this is now very slight. Patient complained also for two or three months of discomfort in the shoulder, but the sterno-mastoid was more affected than the trapezius. These symptoms have now disappeared. Now, six months later, tone has returned to facial muscles at rest and movements of the tongue are associated with movements of the right half of the face, but there is dissociated voluntary control of that side of the face.

DISCUSSION.

Dr. DAN MCKENZIE said that dissociated movements were undoubtedly present in this case. One corner of the girl's mouth moved without there being any movement of tongue or shoulder. He did not think, however, that this operation had ever succeeded in obtaining for the face the movement due to an emotion apart from the patient's direct volition. The most striking change after anastomosis was the improvement in the tonus of the face; the face did not look so blank as in cases of absolute paralysis. This girl showed no improvement of the orbicularis palpebrarum, but it was early yet to say anything about that; it might improve later on. An important point in anastomosis operations was as to when intervention should take place. If we waited long the muscles atrophied unless nutrition was maintained by massage and electricity,

and if the operation were done two or three years after the onset of the paralysis, the operation failed, because the muscles had lost their being. If one operated early one did not get a very good result; and most of the cases which were not operated upon at all regained considerable movement of the face after a considerable time.

Dr. A. A. GRAY said the remarks about the emotions not being expressed were very interesting; this result took much from the charm of the face, and from the charm of the operation also. He agreed it was extraordinary how these cases recovered function without operation even many months after the paralysis had occurred.

Cerebellar Hernia.

By DAN MCKENZIE, M.D.

PATIENT, a girl, aged 15. Operation, three months ago, for symptoms of pronounced meningitis. Mastoid was cellular and free from disease, but filled with foetid pus, which was traced into a large cerebellar abscess cavity. This was drained. Patient not under my observation during convalescence. About a month ago hernia made its appearance.

Sir Charles Ballance says the best treatment of cerebellar hernia is to operate and make the hole in the dura larger. Then there is subsidence without further trouble, and after this the skin can be united over the gap.

Mr. CLEMINSON said he had seen a case in which a decompression operation was done with good effect on the opposite side of the head.

Postscript.—Since this patient was shown the hernia protrusion has become covered with epidermis and is much shrunken. It is evidently undergoing spontaneous cure.

Otogenic Meningitis spreading from the Roof of the Antro-tympanum.

By DAN MCKENZIE, M.D.

FEMALE, aged 28, admitted to the Central London Throat and Ear Hospital, January 23, 1922, with double mastoiditis. The clinical history shows no important point. The patient died of meningitis five days after admission and the case is reported for its post-mortem findings, as I have been told that some doubt has been expressed as to whether meningitis ever does start from the roof of the antro-tympanum.

Post-mortem Report.—Pia: On the surface of the right temporo-sphenoidal lobe, the pia applied to the surface of the dura over the tympanum held in its meshes a thick lymph; its surface was rough and taggy. An infiltrate extended out covering an area of 1 in. or so; granular yellow lymph extended along the sinuses and had accumulated at the vortex on that side. Dura: After stripping it was possible to see a change, swelling and loss of lustre at the site of attachment to the roof of the tympanum. The pia was stuck to this area of the dura by very recent tags of fibrinous exudate. All sinuses were free from all ante-mortem clotting. Section of the clot showed no abnormality. The dura over the sinus had retained its lustre and healthy appearance. Bone: The roof of the tympanum was the seat of a circular opening $\frac{1}{4}$ in. in diameter. The bone was spongy, where normally dense and eburnated. The edge was uneven at the internal quadrant. This area showed an ischæmia, and the substance a sandy, crumbling consistence of necrotic bone.

Notes.—There can be no doubt of the primary osteomyelitis extending beyond the area of operation. This had set up by organization a path of distribution through the dura to the pia. The bone of the roof was crumbly, spongy and cancellous, unlike the more common egg-shell bone. The sinuses were thoroughly searched and contained no tittle of evidence of septic thrombosis. The base was peculiarly free from all infiltration of lymph and the cisterna contained clear fluid.

(Signed) GEORGE SCOTT WILLIAMSON.

At the Provincial Meeting of the Section held at Leicester, April 29, 1922, a Demonstration was given by Miss Carter (Member of the College of Teachers of the Deaf) on the "Oral Methods of Teaching Deaf Mutes." Cases of acute suppurative otitis media after various forms of treatment were shown; and a Discussion took place in which Dr. Logan Turner (President), Dr. Macleod, Mr. J. A. Keen, Sir William Milligan, Dr. William Hill, Mr. Mark Hovell, Mr. E. D. D. Davis and Dr. Friel, took part.

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The Society does not hold itself in any way responsible for the statements made or the views put forward in the various papers.

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Section of Pathology.

President—Dr. W. S. LAZARUS-BARLOW.

Observations upon the Action of Radium and X-rays on the Mononuclear Leucocytes of the Blood of Rats.

By J. C. MOTTRAM.

(From the Research Department, Radium Institute, London.)

It has been shown that the exposure of rats to X-rays over a wide range of dosage is followed within an hour by a 50 per cent. decrease in the number of circulating lymphocytes [1], and that a second exposure has no effect upon the lymphocytes remaining in the circulation until the numbers have returned almost to the normal, when a similar disappearance of lymphocytes from the circulation occurs [2].

These remarkable results, in which effect is not proportional to dosage, have received no explanation. They suggest that there are two kinds of lymphocytes—one very sensitive to radiation and the other comparatively resistant.

It had been casually observed that the average size of the mononuclears remaining in the circulation after radiation was larger than the average size found in normal animals. Measurements of these cells were therefore made to decide whether or not this was the case. The following method was adopted:—

A few drops of blood from the rat's tail were delivered into 5 c.c. of 0.5 per cent. acetic acid; the mixture was shaken and then centrifugalized. The sedimented leucocytes were then mounted in a drop of 0.5 per cent. acetic acid between a thin cover glass and a slide, a ring of vaseline forming the seal. The mononuclears were then drawn with a camera lucida, using $\frac{1}{12}$ in. objective, No. 4 eyepiece, tube 172 mm., which gave a scale of 1 mm. equals a micro-millimetre.

The drawings so made—blood taken before and after exposing the animal to radium ($\frac{1}{2}$ rad., screen 0.1 mm. lead) being used—on being placed side by side showed clearly that the mononuclears were, on an average, of larger size after radiation. The figure shows a sample of the drawings made in this way.

Measurements of the largest diameter of the nuclei of these cells were made with dividers and rule, and it was found that the mononuclears measuring less than 7μ especially disappeared as the result of exposing the animal to radiation, whereas those of 7μ and over were comparatively unaffected.

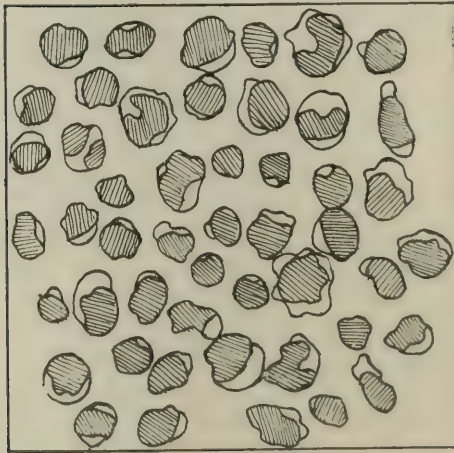
(a) Further observations showed that during the period of regeneration, during which the animal is not sensitive to a second exposure, few small mononuclears returned to the circulation, whereas a return to sensitiveness was associated with a return of the small variety of mononuclears.

(b) A few observations were made on animals suffering from a lymphopenia due to their having been kept on a vitamin-free diet. These animals were found to have fewer small lymphocytes in their blood than normal animals. On their being exposed to radium it was found that the resulting fall in the number of circulating mononuclears was less than in normal animals.

These two sets of experiments (a) and (b), though the number of animals experimented upon was small, lend support to the conclusion that the small variety of mononuclear is especially sensitive to radiation, as measured by their disappearance from the peripheral circulation following exposure of the animal to radiation.



Before Radiation.



After Radiation.

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- [1] RUSS, CHAMBERS, SCOTT and MOTTRAM, *Lancet*, 1919, i, p. 692. [2] RUSS, CHAMBERS and SCOTT, *Journ. Path. and Bact.*, 1920, xxiii, p. 384.

Section of Pathology.

President—Dr. W. S. LAZARUS-BARLOW.

Cleft Palate in Animals.

By FREDERICK HOBDAY, C.M.G., F.R.C.V.S., F.R.S.E.

A FEW days ago, when removing the puppies from a bitch with dystocia, I came across the specimen which I exhibit this evening. We in the veterinary profession are, I believe, agreed as to its heredity, and that it is a defect which often follows too close in-and-in breeding.

It is met with occasionally in horses and cattle, but is much more common in the dog. I have also seen it several times in the cat. I have seen it twice in the horse, and the accompanying figure is from the palate of a mare, aged 22. She had lived with it in that condition the whole time (fig. 1). During and immediately after feeding she had an objectionable nasal discharge, the colour and character of which depended upon the nature of the food she was taking at the time. She was in regular work until within a few weeks of her death, and was destroyed owing to severe lameness.

By a curious coincidence, I saw a case, similar to that just recorded, at a racing stable within a few hundred yards of the place where the mare was out at pasture. In this case the subject was a thoroughbred filly, 5 months old, and worth, if normal, about £500. Shortly after her birth the stud groom had observed that when the animal was sucking the mare the milk would come down the nostrils. At the time I saw it, both fluid and semi-fluid food returned that way. The patient was in good condition, as various nutriments, such as cod-liver oil and Mellin's food, had been administered, in addition to the ordinary diet. An examination revealed a cleft at the back of the mouth about $1\frac{1}{2}$ in. long, and involving mainly the soft palate. Treatment was impossible owing to the small space in which to work and the difficulty of manipulating the necessary instruments and sutures.

In the dog hare-lip and cleft palate are frequently combined, although they may occur separately (fig. 2). They are especially common in short-nosed breeds, particularly bull-dogs, griffons, pugs, Japanese and Pekingese. I have frequently seen as many as three or four puppies (usually fine specimens and well formed otherwise) in a litter, with one or both of these defects. If one does not observe the condition when the pup is born, it soon becomes manifest when the little animal tries to suck, as the milk returns down its nostrils (fig. 3).

Hare-lip is readily operable in certain cases. The operation is carried out in precisely the same way as in human surgery—by scarifying the edges and uniting them together (figs. 4 and 5).

The operation for cleft palate is much more difficult. It consists in making an incision on either side, separating the palate from the bone, scarifying the edges, and then suturing them together.

I have seen the condition twice in the adult cat. In each case the hole was about the size of half a melon seed, elliptical in shape, and exactly in the centre of the palate; and in each instance the result of operation was a

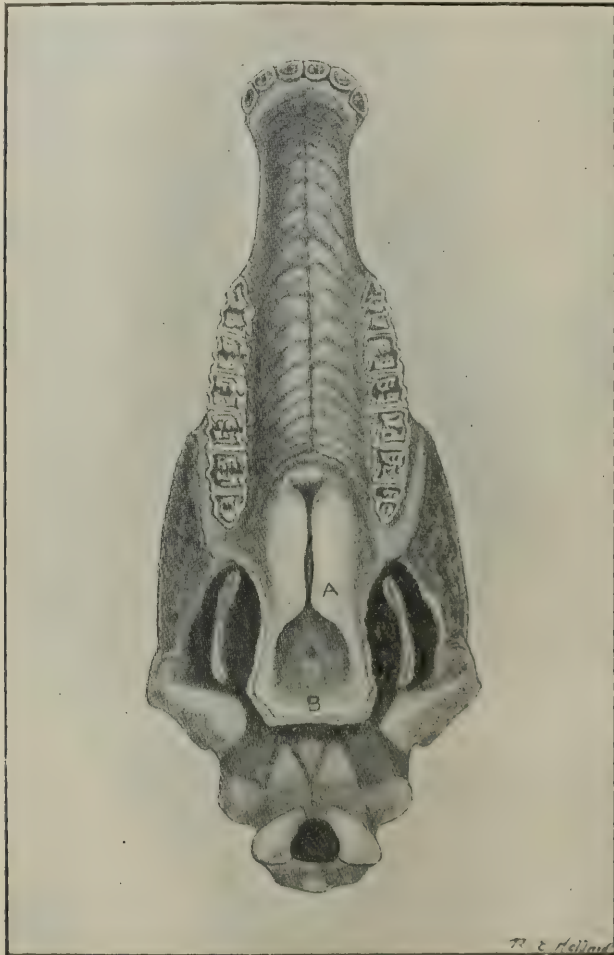


FIG. 1.—The skull of a mare, 22 years old, showing a cleft (A) of the soft palate.
(From the *Veterinary Journal* (Hobday and Ridley), 1912, p. 160.)

satisfactory improvement. Of the various breeds of the cat the well-bred Persian is the one which gives the highest proportion of cleft palates; and here again the animals are very much in-bred, and the aim of the owner is to breed them with flat, short faces, and as little nose or palate as is possible.



FIG. 2.—The head of a bull-dog pup with double hare-lip (A, B) and cleft palate (C). (Hobday, "Canine and Feline Surgery," p. 75.)



FIG. 3.—The head of a bull-dog, 3 years old, with median hare-lip, unassociated with cleft palate. (Hobday, "Canine and Feline Surgery," p. 93.)

Hobday: *Cleft Palate in Animals*



FIG. 4.—A bulldog pup, 5 months old, with unilateral hare-lip and cleft palate.



FIG. 5.—The same after operation.

Section of Pathology.

President—Dr. W. S. LAZARUS-BARLOW.

Further Attempts at the Experimental Production of Carcinoma by Means of Radium.

By W. S. LAZARUS-BARLOW, M.D., F.R.C.P. (President).

(From the Cancer Research Laboratories, the Middlesex Hospital.)

THE experiments to which reference is made in the present paper are a continuation of those described in 1918.¹ In that paper were recorded the effects of introducing minute quantities of radium bromide (1) beneath the skin of mice and rats; (2) enclosed in gall-stones which were placed by operation within the gall-bladders of rabbits. A certain measure of success was obtained but the results were clearly not conclusive. In consequence, experiments were started in which it was sought to combine the local action of radium with a general lowering of vitality of the experimental animals. It had been shown by Price Jones [1] that, in rats in which the K content of the blood had been artificially raised by feeding with potassium metaphosphite (KPO_3), the development of tumour grafts is favoured, and by Russ and Mottram [2] that a rat immune to Jensen's rat sarcoma can be rendered susceptible by exposure to suitable dose of X-rays. Hence, introduction of radium into the rat was conjoined with either or both of these ancillary procedures.

It is not proposed to describe either the procedure or the findings in detail since the results in this series of experiments, also, remains inconclusive. But it may be stated that (1) forty-eight rats were subjected to experiment; (2) the radium bromide in quantities varying from 0.011 mg. to 0.15 mg. was enclosed in minute sealed glass tubes; (3) the radium tubes were introduced intraperitoneally; (4) the exposures extended up to 279 days; and (5) the ancillary treatment was carried out before and after introduction of the radium tube. The radium tube soon became anchored within the abdominal cavity, usually close to the stomach in the neighbourhood of the spleen or at the brim of the pelvis.

Careful post-mortem and microscopical examination of a large number of tissues in each animal indicated four conditions which could be regarded as possible criteria of neoplastic growth. A, a local condition around the tube which, microscopically, was possibly one of new growth; B, a local condition of endothelial proliferation; C, presence in the lungs of cavities containing

¹ *Proc. Roy. Soc. Med.*, 1918, xi (Sect. Path.), p. 1.

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degenerated material and lined by a multiple flattened cellular layer which resembles squamous epithelium: D, the presence of large cells completely filling the gastric lymphatics and suggestive of lymphatic permeation by a new growth. The occurrence of one or more of these criteria in any animal is shown in the accompanying table:—

TABLE I.

Number of animal	Treatment to reduce immunity	LOCAL		Metastasis lung ?	Permeation stomach	Metastasis gland ?
		Growth possible	Endothelial proliferation			
19	Irr.	+	+	...
25	"	+	...	+
28	"	+	+ ?	...
29	"	+	...	+	+	...
40	"	+
41	"	+	...	+	+	...
45	"	+
47	"	+
1	"	...	+
2	KPO ₃ Irr.	...	+	...	+	...
3	Irr.	...	+
4	"	...	+
5	"	...	+	+
6	"	...	+	...	+	...
7	"	...	+ ?
9	"	...	+	+	+	...
10	KPO ₃ Irr.	...	+	...	+ ?	...
11	"	...	+ ?
12	Irr.	...	+	...	+ ?	...
13	"	...	+
14	"	...	+
15	KPO ₃ Irr.	...	+	...	+	...
16	Irr.	...	+	...	+ ?	...
20	"	...	+ ?	...	+ ?	...
22	"	...	+
26	"	...	+
30	KPO ₃	...	+
31	Irr.	...	+	+
33	KPO ₃	...	+	...	+	...
34	"	...	+	...	+	...
37	Irr.	...	+ ?	+
42	"	...	+
43	"	...	+	...	+ ?	...
44	"	...	+
21	"	+	...	+
32	"	+
35	"	+
35A	KPO ₃	+
8	KPO ₃ Irr.	+	...

If the entire series of results be summarized it appears that no criterion was found in nine animals, one criterion was found in nineteen, two criteria were found in seventeen, and three criteria were found in three. Grouped differently the table shown below is produced (*see* Table II, p. 9).

The principal difficulty in interpreting the results is that the extent of the change (with the exception of those in the lungs) is limited. When a condition was found which recalled malignant disease in man, microscopic examination almost invariably showed that the main mass was either completely degenerated or frankly inflammatory. In one instance an attempt was made to graft portions of the wall of a pulmonary focus into other rats, but without success.

TABLE II.

Degenerative changes alone	9
Local neoplastic growth possible:—								
Alone	3
With ? metastasis lung	1
With permeation stomach	2
With ? metastasis lung and permeation stomach	2
								—
							Total.....	8
Local endothelial proliferation:—								
Alone	12
With ? metastasis lung	3
With permeation stomach	10
With ? metastasis lung and permeation stomach	1
								—
							Total.....	26
? Metastasis lung:—								
Alone	3
With ? metastasis gland	1
								—
							Total.....	4
Permeation stomach alone	1
								—
							Grand total...	48

In the cases in which tissue around the radium tube was suggestive of new growth, the microscopical appearances were mainly those compatible with squamous or columnar carcinoma or with endothelioma. The most striking case was one in which the tube was at the pelvic brim and the surrounding tissue resembled squamous cell carcinoma in some parts and columnar cell carcinoma in others. Photomicrographs of this case are appended (figs. 1 and 2, pp. 10 and 11).

In respect of the influence of KPO_3 and X-rays in leading to the occurrence of one or more of the criteria to which reference has been made above, it was found that with KPO_3 alone four cases out of seven (57 per cent.) were positive, whereas with X-rays alone thirty cases out of thirty-six (83 per cent.) were positive and with KPO_3 + X-rays five cases out of six (83 per cent.) were positive.

In the paper already cited, I referred to the difficulty of inducing carcinoma experimentally in rats by means of radium. Recently Leitch [3] has stated his failure to produce the condition in these animals by means of tar. The results now communicated appear further to point to the conclusion that rats are remarkably resistant in respect of induced cancer.

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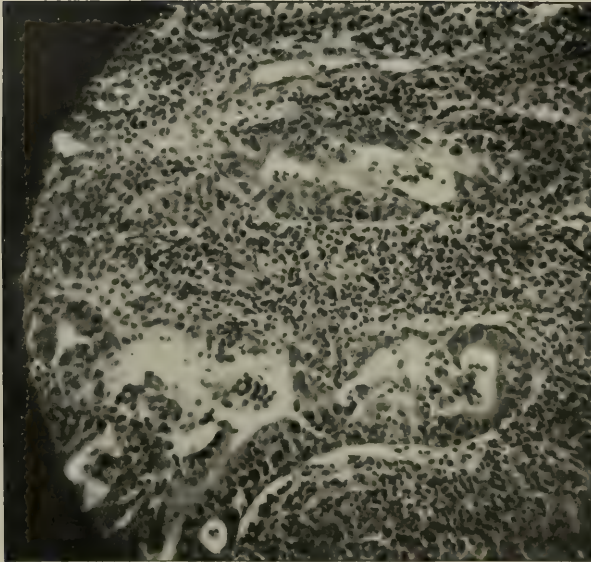


FIG. 1.—Rat 25. Eight weekly half-pastille doses of unscreened X-rays, under standard conditions, subsequent to intraperitoneal introduction of a sealed thin glass tube containing 0.025 mg. RaBr₂. Total exposure to radium fifty-four days. Died May 18, 1919. Microscopic section of tissue round tube at brim of pelvis. Note that the highly cellular tissue of inflammatory type contains two irregular masses of material, having a transitional appearance between squamous cell carcinoma and columnar cell carcinoma. Areas having definite characters of each variety singly were numerous in other sections. Paraffin. Haematoxylin, eosin. ($\times 250$.)

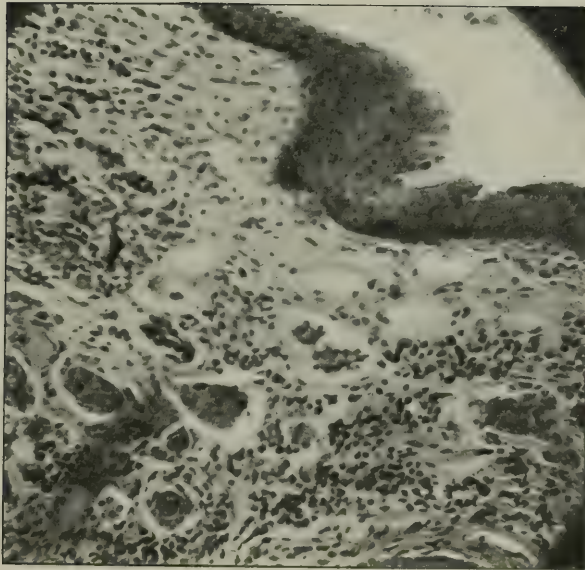


FIG. 2.—Same rat. Portion of wall of a cavity in lung (? metastasis) lined by flattened cells and containing débris. The lining cells appear to be squamous, but no prickle spaces are present. Mitoses are numerous, but are not seen with this magnification. Many transverse sections of permeated channels are present in the consolidated tissue around the cavity.

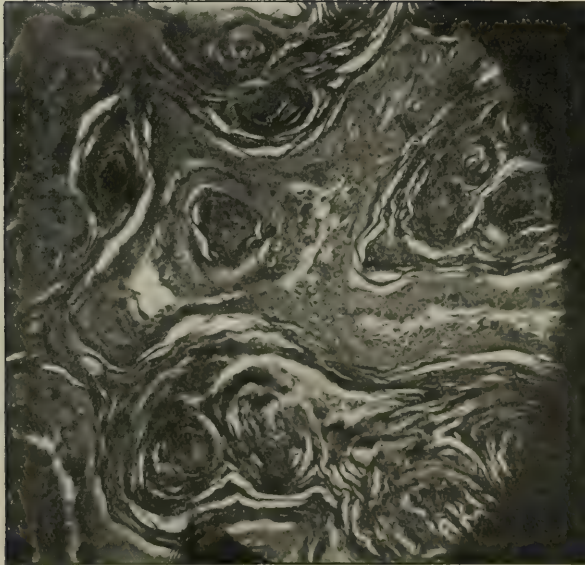


FIG. 3.—Rat 28. Five weekly quarter-pastille doses of unshielded X-rays, under standard conditions previous to, and eight similar doses subsequent to, intraperitoneal introduction of a sealed glass tube containing 0.15 mg. RaBr₂. Total exposure to radium, sixty days. Killed: October 20, 1919. Microscopic section of stomach in neighbourhood of tube, which was attached to the lower edge of stomach and formed with the surrounding tissue, a mass the size of a wheat grain. Note condition suggestive of an intensely keratinized squamous cell carcinoma. This is not due to the plane of section, for identical appearances were given when the paraffin block was cut at right angles. Paraffin. Haematoxylin, eosin. ($\times 250$.)

Section of Pathology.

President—Dr. W. S. LAZARUS-BARLOW.

Invasion of the Nerves in Carcinoma of the Sublingual Salivary Gland, associated with Carcinoma of the Tongue.

By S. G. SHATTOCK, F.R.S.

THE invasion of the nerves in a carcinomatous district is so rarely observed that I venture to record the following instance of it, partly for this reason and partly for certain considerations it raises.¹

That nerves may become surrounded, without being invaded, by the growth of a carcinoma or sarcoma, is well recognized. Doubtless the dense, fibrous structure of the perineurium in nerves of medium size, and of the superadded epineurium in those constructed of multiple fasciculi, offers the same obstacle to invasion as fibrous membranes do in general. Whether local compression of the nerves involved in a tumour or their complete invasion, is the explanation of those cases where a malignant growth proceeds without pain is a matter for investigation. Carcinoma of the stomach, for example, may grow to a large size without any accompanying pain; and this, apart from its necrosis and destruction. Mr. Wilfrid Trotter would apply to such cases his theory of the causation of pain in intestinal colic and enterospasm—viz., that the source of pain in the latter is not the proper muscular spasm, but the stretching of the adjoining peritoneum which the spasm involves. Thus a gastric carcinoma which leads to pyloric stenosis, and consequent abnormal gastric peristalsis would be accompanied with pain; otherwise it might not. The occasional involvement of motor nerves in malignant disease is proved by the facial paralysis at times met with in malignant tumours of the parotid gland; and by the paralysis of the vocal cord from involvement of the recurrent nerve in carcinoma of the œsophagus.

But to come to the case itself. It was that of a man, aged 58, who had had syphilis at the age of 25, and was admitted under Mr. Sargent, in 1914. For the past five months the tongue had become enlarged and hard, and had ulcerated on the left side. On examination it was found fixed, and the floor of the mouth much infiltrated. The parts were successfully removed. *There was persistent headache on the left side.* The microscopic section exhibited is of the sublingual salivary gland invaded by squamous-celled carcinoma extending from the tongue. The proper glandular tissues, acini, and ducts, are unaffected,

¹ My colleague, Professor L. S. Dudgeon, has likewise seen a second example of nerve invasion, in a carcinoma of the uterus.

though considerable areas of lymphocytes occur in the connective tissue. The carcinomatous infiltration of the connective tissue of the gland is of the finest kind, the cells lying often in single rows in the tissue clefts, and producing by their interconnexion an open net irresistibly suggestive of lymphatic permeation. A considerable number of the nerves have become invaded. The invading groups of cells, as shown in the accompanying figure, are not only distributed through the endoneurium supporting the nerve fibres, but, what is of particular interest, they lie also in narrow rings in the circular clefts or lymph spaces of the perineurium. In at least one nerve, the continuity of the cell circle in the perineurium with the carcinomatous strands which permeate the surrounding connective tissue was quite clear. This demonstrates that the invasion has taken place through the perineurium where the latter becomes attenuated as the nerve diminishes in size; it is not necessary to assume that invasion takes place particularly at the nerve endings. In the cross sections of some of the nerves, moreover, the endoneurium is free, and the narrow lines of carcinoma cells are confined to the circular clefts, proving that extension by the lymph-spaces is taking place in advance of that in the endoneurium. The occurrence of keratinizing nests places the histological diagnosis beyond dispute. Once the cells have reached the lymph-spaces of the perineurium they could grow as freely as, if not more so, than a culture *in vitro*, for they are nourished by the plasma which percolates between them and carries away the products of auto-intoxication. Indeed, there seems no reason why, under such circumstances and the ease with which the cells may be shifted, the proliferation might not extend indefinitely to the central nervous system.

In regard to the surgical removal of any such carcinoma itself, it is obvious that the division of nerves would inevitably be followed by recurrence, should their invasion have extended beyond the area of operation.

The more interesting question, however, raised by such nerve invasion is how far it may be concerned in the referred pain which arises in some cases of carcinoma, particularly of the tongue. In that recorded there was persistent headache on the left side—that of the carcinoma. As Head observes in his classical paper,¹ "Malignant disease of the tongue may cause pain in the ear or over the back of the head, in addition to the local pain in the tongue itself. . . . If the disease involves the anterior part of the tongue, pain is complained of over a spot close to the point where the mental nerve passes through the deeper structures of the jaw to supply the skin. This is the maximum spot of the mental area. This pain in front of the lower jaw is sometimes associated with superficial tenderness. If the lateral part of the tongue only is implicated, the patient complains of pain 'in the ear,' and behind the ramus of the lower jaw on the side affected. In such cases the hyoid area is not uncommonly found to be tender. If the lesion is situated on the dorsum behind a line about 3 to 4 cm. from the tip, the pain is in the throat, close to the cornu of the hyoid bone, and may be felt at the back of the head in addition." The skin is tender also in these positions. The areas may extend as the lingual disease does; i.e., pain appears also in the nose.

In regard to the sensation of pain, it will be recalled that the pain arising in the trunk of a nerve is referred to the nerve-endings—"peripheral reference" (Trotter). Any purely local pain in the trunk itself would be that referred to the endings of the *nervi nervorum*. But what is meant by "referred

¹ *Brain*, 1894, xvii, p. 418.

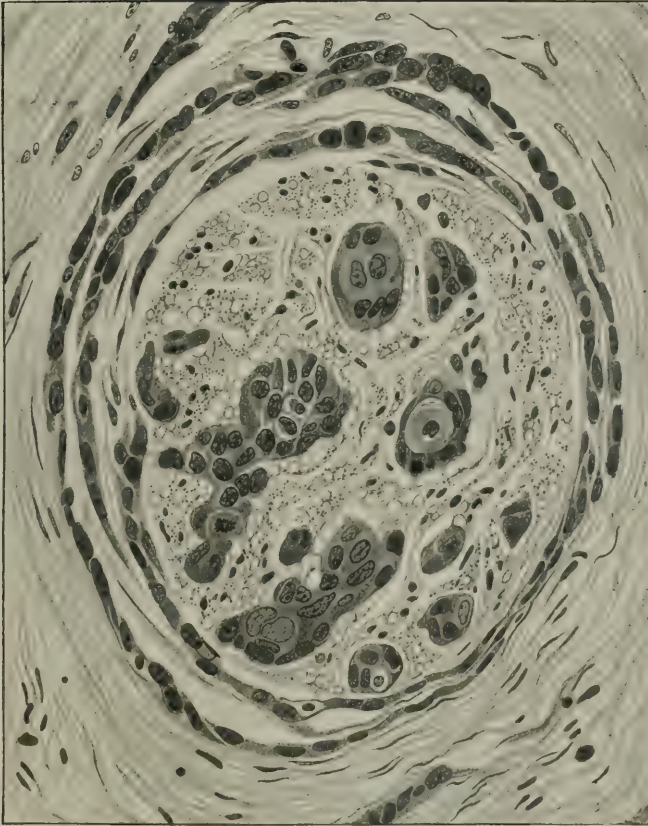


FIG. 1.—A small nerve in transverse section, lying in a carcinomatous area of the sublingual salivary gland, associated with carcinoma of the tongue. Groups of squamous cells are not only scattered through the endoneurium, but lie in rings in the circular lymph spaces of the perineurium. (Obj. $\frac{3}{8}$ in.)

Figurae explicatio.—Sectio nervi in carcinomate glandulae salivariae sublinguali inclusa, tumore cum linguae carcinomate conjuncto. Cellulis carcinomatosis invaduntur non endoneurium solum sed perineurii lacunae lymphaticae quoque annulares.

pain" is that referred to the distribution of another or of other nerves in addition.

It might repay investigation at the autopsy on a case of carcinoma of the tongue to ascertain whether the trunk of the lingual nerve was invaded beyond the organ itself; perhaps as far even as the Gasserian ganglion; and whether such an extension might not lead to pain referred to branches connected with the lowest trunk of the trifacial, to the auriculo-temporal branch, and the mental branch of the inferior dental.

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Section of **Psychiatry.**

President — Dr. BEDFORD PIERCE.

Further Pathological Studies in Dementia Præcox, especially in relation to the Interstitial Cells of Leydig.

By FREDERICK W. MOTT, K.B.E., M.D., F.R.S. (Part I), and
MIGUEL PRADOS Y SUCH, M.D. Madrid (Part II).

(*Pathological Laboratory of the L.C.C., Maudsley Hospital, Denmark Hill.*)

PART I.

HAVING in previous communications paid especial attention to the arrest of spermatogenesis and regressive atrophy of the spermatic tubes, and having only made some brief references to the morbid changes in the interstitial cells in various forms of mental disease, I thought it would be of interest to systematically examine, describe and compare the histological appearances of the interstitial cells in the different forms of insanity with the normal at different ages and with one another. The literature is fully considered by Dr. Such in Part II.

THE INTERSTITIAL CELLS OF LEYDIG.

The interstitial cells of Leydig are polygonal in shape with a round nucleus. The cytoplasm of the mature cell is considerable, and contains a substance which takes the eosin stain remarkably well. Consequently sections stained by hæmatoxylin eosin, or Heidenhain hæmatoxylin eosin, show the interstitial cells remarkably well.

I have examined all the material of the hundred cases of patients dying in hospitals or asylums with the view of determining more precisely the condition of the interstitial cells in relation to spermatogenesis.

THE LEYDIG CELLS IN THE PRE-PUBERTAL PERIOD OF LIFE.

At birth the testis consists of the spermatic tubes, made up of a delicate basement membrane containing embryonic, undifferentiated, rather small, epithelial cells, consisting mainly of nucleus with a well-marked chromatin skein. There is abundant loose intertubular connective tissue in which are seen columns, islands and islets of cells, the polygonal outlines of which are quite distinct; the cytoplasm is stained pink, and in the centre is a round nucleus, with a well-developed nuclear membrane containing a chromatin network, both of which are stained deep blue by the basic dye. No nucleolus is visible (*vide* fig. 1). Lipoid granules are visible in many of the cells as revealed by scharlach stained frozen sections, and by vacuoles in hæmatoxylin eosin stained sections mounted in Canada balsam.

NORMAL TESTIS AT BIRTH.

The embryonic tubules, and between them the interstitial tissue, which consists almost entirely of interstitial cells, isolated or in groups, with distinct polygonal outline of cytoplasm stained by the eosin, or a nucleated vacuolated syncytium of what was originally polygonal cells. I shall speak of a syncytium because when the cells are distinctly vacuolated their outline is ill-defined, and they appear to be interconnected. The nuclei in this unstained or faintly stained syncytium are of varied size and shape; many of these are only faintly stained by the basic dye and are deficient in chromatin, and the appearances are not unlike those observed in the regressive atrophy of dementia præcox. Where this is taking place there are a number of more deeply-stained nuclei of fibroblasts. This is the final stage of regressive atrophy. In other places one sees the earlier stage of regressive atrophy, viz., groups of polygonal cells with the cytoplasm faintly stained pink, and the nuclei hardly stained and appearing only very pale blue; in the immediate neighbourhood of these are mature polygonal cells with pink cytoplasm and round nuclei with chromatin network well stained. In these cells which are undergoing regressive atrophy the nuclei instead of being round are irregular in outline, crenated and polymorphic. Here we have all stages of regressive atrophy of the interstitial cells seen in dementia præcox (except pigmentary degeneration), but this is a physiological and not a pathological process. It will be interesting to trace the further stages of the disappearance of these cells after birth.

THE PRE-PUBERTAL DISAPPEARANCE OF THE LEYDIG CELLS.

The Interstitial Cells at Four Months.

Examination of sections of the testis of an infant aged 4 months showed that the tubules are now twice the size owing to proliferation of the embryonic epithelial cells and are closely approximated. This is due to the almost complete disappearance of the interstitial cells of Leydig (*vide* fig. 2 to compare with fig. 1). Sections examined with an oil immersion lens show oval and round nuclei lying in a vacuolated syncytium, but no pronounced *eosin*-stained cells can be seen. These nuclei are not connective tissue nuclei but the nuclei of the Leydig cells. Frozen sections stained with scharlach show lipoid granules corresponding to the vacuoles. Examination of the tubules with an oil immersion shows numbers of very fine lipoid granules in the syncytium of embryonic cells. The presence of lipoid granules in this situation and in the interstitial tissue and Leydig cells, shows that the formative process is still proceeding, but the size and close proximity of the tubules is indicative of its terminal phase. I have been unable, owing to my not having the material, to trace the stages of regressive atrophy between birth and 4 months and after this time to 10 years.

The Interstitial Cells at Ten Years.

Examination of sections of the testis of a boy aged 10 who died of fracture of the base of the skull. The tubules are not any larger in some portions of the section than those found in the testis of an infant aged 4 months and show no more differentiation of structure. Occasionally one finds the first evidence of differentiation by the appearance now and then of definite spermatogonia and some of the cells show mitotic figures. In some parts of the organ the tubules are closely approximated and the appearances resemble those described as

occurring at 4 months. In other parts the tubules are separated by a considerable amount of interstitial tissue and groups of polymorph oval and round nuclei, like those of immature Leydig cells, can be seen, but only very occasionally can a small polygonal cell with pink cytoplasm and a round central nucleus be observed. The interstitial tissue contains no lipoid, for frozen sections stained by Sudan III show no fat in the tubules or interstitial tissue. Function has not therefore commenced in the interstitial cells. As the Sertoli cells do not appear to be present yet, it seems probable that with their appearance the Leydig cells would mature and function, but further observations at later ages are necessary to determine whether this be so. I have taken some pains to show the conditions of the interstitial cells in the pre-pubertal period of life because after birth there is a regressive atrophy and cessation of function, and the appearances presented by the interstitial tissues, at birth and afterwards, in a way correspond to the appearances met with in the regressive atrophy of the interstitial cells in dementia præcox. Moreover, the fact that the development of the spermatic epithelium corresponds with the appearance of lipoid in the interstitial cells and the tubules, and its absence with the signs of an absence of formative activity in the tubular epithelium, supports the view that these cells of Leydig perform the double function of providing a hormone and the raw material for formative activity of the spermatic epithelium.

THE LEYDIG CELLS IN THE PUBERTAL AND POST-PUBERTAL PERIOD OF LIFE.

The testis of a boy aged 15, who died from shock of injuries, showed active spermatogenesis, lipoid granules in the Sertoli cells and interstitial lipoid; this interstitial lipoid was found in drops and droplets in the lymphatics and lymphatic clefts, also in the Leydig cells, which have now reappeared in the interstitial tissue. Their reappearance is, therefore, synchronous with the first appearance of the secondary sexual characters which it has been proved they determine.

They were found subsequently at all ages up to extreme old age—80, 85 and 86—though in diminished numbers and corresponding, generally speaking, with the degree of spermatogenic activity, although by no means always, for sometimes these cells could be found in fair numbers while there was little evidence of active spermatogenesis, and the converse. Mr. Kenneth Walker, who has been working in the Maudsley Laboratory on the prostate, has kindly furnished me with the following results, which are in accord with this conclusion (*vide* Table II, p. 24). I am now only referring to cases dying in hospital and to certain asylum cases, but not to cases belonging to the biogenetic psychoses in which there is a partial or complete regressive atrophy.

I came to the conclusion from the examination of the testes of young adult cases dying from shock shortly after receiving severe injury and the testis of the boy above referred to, that these Leydig cells have a comparatively short life and are continually maturing, decaying and being renewed (*vide* fig. 1, Plate I). All stages of small nuclei resulting from active division can be observed, followed by division of the cell and growth to the mature cell, such as was seen in the newborn child. The cytoplasm of the normal mature cell is abundant and is stained by the eosin dye a deep pink, so that with a low power, islands, columns and islets of cells can easily be recognized in the interstitial tissue.

When examined with an oil immersion details can be observed which cannot readily be seen with a low power, e.g., the amount of chromatin in the nucleus

can be gauged and varying degrees of vacuolation in the cytoplasm corresponding to lipoidal contents can be estimated. I have come to the conclusion from my observations that vacuolation and disappearance of the pink cytoplasm is associated with a tendency to make the outline of the cells ragged or indistinct; and if marked, to convert the island of cells into a nucleated pale vacuolated syncytium.

THE LEYDIG CELLS IN PROLONGED SEPSIS.

I found this pale vacuolated syncytial condition in various cases of prolonged sepsis such as suppurative pericarditis with empyema, cerebro-spinal meningitis and several cases of gunshot injury of the head. I regard the pink staining substance as the antecedent, if not the actual hormone material, therefore the above described condition of the Leydig's cells is indicative of an exhausted condition and temporary failure in their function; and associated therewith was an arrest of the final stages of spermatogenesis.

It may be hypothesized that the vital impulse of the social organism (the body) was all concentrated in an attempt to preserve the life of the individual; the formative energy of the testis being in abeyance, the vital energy of the sex instinct was available for prolonged formative cell activity of phagocytes and pus formation.

I have shown that in many fatal cases due to microbial invasion of the body, such as tuberculosis, pneumonia, broncho-pneumonia, typhoid and dysentery, active spermatogenesis in all stages can be seen in the majority of cases; in some, especially those dying within a short time of the onset of the disease, pink eosin-stained islands and islets of Leydig cells are seen. Indeed, a case of infective endocarditis, aged 21, that developed a cerebral aneurysm and died suddenly of apoplexy from its rupture, showed normal interstitial cells and normal active spermatogenesis. But as a rule the pathological changes indicating functional exhaustion of the interstitial cells are more marked when there are circulating microbial toxins from chronic diseases than can be observed in the epithelial cells of the spermatogenic tubes. The spermatogenic cells may be protected by the basement membrane and the abundance of lipid cholesterol ester with which it is covered, both within and without.

THE STRUCTURE OF THE BASEMENT MEMBRANE OF THE TUBULES AND ITS RELATION TO LYMPHATICS.

The basement membrane of the tubules appears to consist of a delicate connective tissue lined externally by flat endothelial cells (*vide* figs. 1 and 2, Plate I). When the membrane undergoes thickening owing to regressive atrophy of the spermatogenic cells, these structures undergo proliferation. In a case of dementia præcox that died of chronic nephritis and in which the testes showed microscopically advanced regressive atrophy, yet owing to the œdema were nearly of normal weight (*vide* Table IV, No. 13), microscopic examination of sections of the organ showed that the thickened membrane was separated by the œdema into layers. The Sertoli cells and spermatogonia rest therefore upon a layer of connective tissue and endothelial cells, external to which is a lymph space, the external wall of which is similar to that of the basement membrane. External to this lymphatic space, and resting upon it, are the Leydig cells. These anatomical dispositions can often be clearly made out in normal tissues. Examination of frozen sections stained with scharlach and hæmatoxylin show drops and droplets of lipoid substance in this lymphatic space and in the lymphatic clefts of the interstitial tissue, also in the Leydig cells. Very fine

droplets can be seen in the Sertoli cells, and these undoubtedly serve for the growth and development of the spermatozoa.

THE ORIGIN AND DESTINATION OF THE INTERSTITIAL LIPOID.

The question arises: Does this lipid, which is seen in the interstitial lymphatics, serve as the raw material elaborated by the Leydig cells for spermatogenesis, or does it represent a waste product of spermatogenesis to be taken away by the circulation? The following arguments are in favour of the former hypothesis:—

(1) The correspondence between the disappearance of Leydig cells and cessation of spermatogenic epithelial growth.

(2) The reappearance of Leydig cells before signs of spermatogenesis.

(3) The presence of lipid in the interstitial cells.

(4) The presence of lipid in the Sertoli cells when the remaining cells have undergone complete regressive atrophy, and when this occurs interstitial cells containing lipid also exist.

(5) The interstitial cells by this hypothesis would perform a double function, viz., by a decomposition of the eosin-staining substance (corresponding to a zymogen) a hormone would escape into the circulation and the lipid would pass into the lymphatic space surrounding the tubule and thence into the Sertoli cells possibly by the active intervention of the endothelial cells.

(6) In the undescended testis, the tubuli seminiferi are inactive and undeveloped but the interstitial hormone cells persist; they contain these lipid granules, so that they retain the function of providing a sexual stimulant apart from spermatogenesis. They thus provide the mental and bodily conditions required for coitus. Not only do these cells retain their anabolic function of storage of the zymogen and production of hormone, but the presence of lipid granules in their interior shows that in the undescended testis they retain a katabolic action in their interior. In the freemartin—a bull with undescended testis—I found a non-existence of seminiferous tubules, but the gland consisted of fibrous tissue and fibroblasts, amongst which are large polygonal cells with round nuclei; many of these interstitial cells contained lipid granules stained black by osmic acid.

It is well known that cryptorchids have sexual desire, and that ligature of the vas deferens, on both sides, which causes complete obstructive atrophy of the tubules, also exposure of the testes to X-ray which destroys spermatogenic function, leave the interstitial cells intact and with their integrity the sexual appetite persists.

ANALOGY OF THE CELLS OF THE THECA INTERNA OF THE GRAAFIAN FOLLICLES AND THE CELLS OF LEYDIG.

Examination of the ripening Graafian follicles shows that the cells of the theca interna take the eosin stain and that if the section be stained with scharlach it will be found that in these cells and between them and between the cells of the zona granulosa are numbers of lipid granules.

A section of the ovary of a child aged 18 months shows the eosin-stained theca interna surrounding a Graafian follicle. Inasmuch as from early infancy onward Graafian follicles with these cells of the theca interna are continually formed, it may be presumed that they have a function. Now the somatic cells possess both male and female characters but the male are dominant; it seems therefore probable that these thecal cells are continually being formed

under the stimulus of follicle development. As they do not become mature enough to rupture but form atretic follicles, it may be assumed that this follicular formation is for the purpose of secreting a hormone to maintain the female characters in the somatic cells.

Young pullets that had had the ovaries removed developed into birds that look like cockerels and behave like cockerels. The fact that castration in early life does alter the mental and bodily characters of the individual, yet does not produce insanity, indicates that the changes in the central nervous system in dementia præcox are not dependent upon the regressive atrophy of either the interstitial or the spermatogenic structures, but that dementia præcox and dementia presenilis constitute an innate germinal defect which is manifested obviously in the two tissues of the body essential for the preservation of the species, viz., the brain, the organ of external relation and the reproductive organs.

THE VITAL IMPULSE.

There is an inherent lack of durability or vital energy in the neurones, especially those latest developed ontogenetically and phylogenetically. But this lack of vitality in dementia præcox is probably not confined to these two tissues; being of biogenetic origin it affects sooner or later all the active functioning tissues of the body and there is a corresponding deficiency in oxidation processes.

I do not recollect having seen a case of dementia præcox in a congenital syphilitic, nor have I observed a positive Wassermann reaction in the blood or fluid of cases of dementia præcox, although a great number have been examined. There is no reason why a case of congenital syphilis should not develop symptoms of dementia præcox. Indeed, I have seen a case of juvenile general paralysis with symptoms of hebephrenia. I have occasionally seen cases of dementia præcox diagnosed general paralysis on account of delusions of grandeur, but they gave negative Wassermann reactions and at death no naked-eye or microscopic signs of general paralysis were found. I have not found any evidence to correlate any intimate connexion between this disease and syphilis or alcoholism in the parents.

The reason for the absence of acquired syphilis in dementia præcox cases is that the male is not as a rule attracted to the opposite sex, indeed feeling his inadequacy he usually rather shuns and avoids females.

THE SEXUAL FUNCTION AND AUTO-INTOXICATION.

It has long been thought, and it has been taught by Kräpelin, Urstein and other authorities, that an intoxication arising from a disturbance of the normal functions of the sexual glands is an essential pathological condition in dementia præcox. If there be an auto-intoxication by disorder of the sexual functions is it direct or indirect in its effects upon the functions of the brain?

An excess or deficiency of the sexual hormone may certainly cause a disturbance of the normal biochemical equilibrium of the endocrine gland function, sufficient to make a latent potential psychotic individual actively anti-social, and thus reveal the mental disease.

If it be granted that the psycho-physical energy of the sex instinct is activated by hormones secreted by the interstitial cells a deficiency would be associated with a depression of psycho-physical energy and the mental disorder would be revealed.

Now there is in dementia præcox usually a simultaneous regressive atrophy

of both the interstitial cells and the spermatogenic cells of a progressive character; moreover, as a rule, there is a correspondence in the intensity and degree of the atrophy of these two essential structures in the male organ of reproduction (*vide* Table I). It will therefore be interesting to study the male reproductive organs (1) in extreme old age; (2) in general paralysis of the insane; (3) in post-adolescent psychoses; (4) in dementia præcox.

The Testes in Octogenarians.

I have examined the testes of three octogenarians suffering with senile dementia, aged respectively, 81, 83, and 86. In the old man of 81 there was active spermatogenesis. The testes were of average normal weight and presented a fairly normal macroscopic appearance; beyond some atrophy of the tubules and thickening of the basement membrane, the tubes and the spermatozoa found in the tubules were for the most part normal as regards the staining reactions. There was certainly, as compared with a young normal adult, a diminished spermatogenic activity and a very considerable diminution in number of the interstitial cells. Small islands and islets of pink eosin-stained cells could be discerned with a low power, and examined with an oil immersion these cells presented a fairly normal appearance with the exception that many of them were pigmented (*vide* fig. 1, Plate IV). The sections presented less evidences of deficiency of functional activity than did the great majority of the cases of dementia præcox, and of several of the cases of presenile atrophy occurring in males at an age between 50 and 60, who presented signs and symptoms of dementia præcox (*vide* fig. 3, Plate I), or in cases of involuntional melancholia.

The other two cases, aged 83 and 86, showed a much more advanced failure of spermatogenic function, and the tubules exhibited a marked regressive atrophy in many respects corresponding to the third stage of regressive atrophy of dementia præcox. Excepting in the following facts that many of the tubes showed heterotypical mitosis and here and there a few spermatozoa, the chromatin of the nuclei presented a normal staining reaction; here and there small islands of normal stained, but pigmented, Leydig cells could be seen in the interstitial tissue.

The pigment in the cells, it may be presumed, is evidence of senility; but we shall find that in a large proportion of the cases of dementia præcox occurring in young adults dying under the age of 30 and in the presenile cases (*vide* Table IV), pigmented cells are present (*vide* fig. 4, Plate I), and in one case of dementia præcox who lived fifteen years after the onset of symptoms the great majority of the interstitial cells that still remained were pigmented (*vide* No. 27, Table I).

Having thus established the fact that the hormone cells persist to a very old age, which accords with the known fact of persistent sexual desire, it follows that if in young adults we have a condition in many ways similar to this senile change, it may be regarded as probable that there is a germinal precocious senility, and therefore of formative capacity of the reproductive organs in dementia præcox, which, arising at puberty or early adolescence in the great majority of cases, progresses and finally leads to a complete loss of reproductive power.

The Testes in General Paralysis.

The average weights of the testes after removal of the epididymis and tunica albuginea of twenty-four cases of general paralysis were 16·5 and 16·9 grm., or together 33·4 grm.

The average weights of the testes after removal of the epididymis and tunica vaginalis in twenty-seven cases of dementia præcox were respectively 12.5 and 13 gm., together 25.5 gm. (Table I).

The average weight therefore of the testes in dementia præcox is nearly 8 gm. less than in general paralysis.

Examination of an emulsion of the testis in cases of general paralysis by dark ground illumination in the twenty-four cases, with very few exceptions, showed spermatozoa, whereas in the twenty-seven cases of dementia præcox quite two-thirds showed no spermatozoa.

I have examined microscopically the testes in twelve cases of general paralysis unselected. The average age at death of these twelve cases was 49.5. The youngest was 28 and the eldest 58. The heaviest pair of testes was 30 gm. each (age 46) and the lightest pair was 11.5 gm. and 12 gm. respectively. There were five of the twelve cases which upon microscopic examination showed generalized active spermatogenesis in the tubules and fairly normal interstitial cells and lipid; the average weight of the pair of testes in these cases was 44 gm. and the average age at death was 38. Of the remaining seven of which microscopic examination was made, it was observed that in most cases there was normal active spermatogenesis taking place in some of the tubes, but scattered about in the organ were strands and areas of completely atrophied tubules without cells of Sertoli. Often one testis would show this more markedly than the other, and not infrequently there were naked-eye appearances of old inflammatory conditions such as adhesions of the tunica vaginalis and one with melon seeds. These areas of atrophy were either due to a local specific inflammatory condition or probably more often to a gonorrhœal epididymitis with secondary atrophy from obstruction of the vasa efferentia. In these latter cases amidst the atrophied tubes or more often in the neighbourhood were visible under a low power islands and islets of eosin-stained Leydig cells. It may be remarked that in most of the cases, especially those whose age was over 40, the interstitial cells contained pigment. In only one of these twelve cases could I not see under a low power some evidence of eosin-stained clumps of Leydig cells. In most of the cases they were less numerous than in the normal man of a similar age, but this may be due to the fact that in the majority of cases death occurred in or past the prime of life and many had suffered from an intercurrent, often chronic, disease which would tend to exhaust the Leydig cells and arrest spermatogenesis. Yet comparatively to the cases of dementia præcox (*vide* Table I) and the post-adolescent cases presenile (*vide* Tables III and IV) the difference was striking. I have only once found absolutely complete arrest of spermatogenesis in this disease, and that was in a case of prolonged seizures (status), inanition and exhaustion. Even in testes weighing less than 12 gm., evidence of considerable atrophy, I have found some of the tubules showing active heterotypical mitosis and spermatogenesis. Moreover, the nuclei and the mitotic figures showed a good basophil reaction, contrasting in this respect to the tubules in the earlier stages of dementia præcox where as often as not I found unequal basophil nuclear staining.

The Testes in Post-Adolescent Psychoses.

In Tables III and IV there are thirteen cases of post-adolescent psychoses. If we analyse these Tables we see that in a case of Korsakoff psychosis (No. 5) dying at the age of 55 the testes weighed 14 gm. each; there was a recent arrest of spermatogenesis which I associated with a carcinomatosis of the cirrhotic

liver and paracentesis; it will be noted, however, that Leydig cells were visible with a low power, and pigmentation was not observed. The cases of manic-depressive insanity, in which there was no terminal dementia, exhibited active spermatogenesis but the interstitial cells were relatively deficient. In the cases where the manic-depressive condition terminated in dementia and in the cases in which symptoms like those of dementia præcox—cases which it may be permissible to call dementia presenilis—occurred, the testes were greatly diminished in weight and exhibited appearances similar to those observed in advanced dementia præcox (*vide* Cases, Tables III and IV, and fig. 3, Plate I).

The investigations are not sufficiently advanced to do more than conjecture that the manic-depressive state may possibly be related to the deficiency in the formative activity of the Leydig cells and the influence of such deficiency on the endocrine system. The close relation of manic-depressive insanity to dementia præcox is shown by the fact it may end in a terminal dementia and the microscopic appearances of regressive atrophy of the testes in such cases resemble those met with in dementia præcox. It would be interesting to examine the brains in these cases for I expect we should find similar histological changes also.

THE MORBID CHANGES OF THE TESTES IN DEMENTIA PRÆCOX.

I will now analyse the findings in the twenty-seven cases of dementia præcox contained in Table I. It may be mentioned that in the majority of the cases sections of frozen tissues were stained with scharlach and hæmatoxylin-eosin. Lastly, to prevent shrinkage, blocks of the tissues were embedded in celloidin, taken out and allowed to skin over, then placed in chloroform and subsequently embedded in paraffin and cut in serial sections; in this way shrinking was prevented and the cells were thus prevented from dropping out.

SUMMARY OF THE RESULTS OF THE MICROSCOPIC EXAMINATION OF THE TESTES IN DEMENTIA PRÆCOX.

The results of the microscopic examination of the twenty-seven cases of dementia præcox are summarized in tabular form. In a previous publication in which I devoted special attention to the condition of the spermatic tubules in twenty-three cases, I divided the cases into three classes or stages of regressive atrophy of the spermatogenic cells.¹ In the *first stage* I included those in which the changes could be discovered by comparison with the normal. Active spermatogenesis could be observed in numbers of tubules, but examination with an oil immersion lens showed that the heads of the spermatozoa were not infrequently of irregular shape, unequal size and staining with eosin instead of the basic dye. There was one case, No. 6 (*vide* fig. 4, and fig. 2, Plate II, Part II).

The *second stage* I have divided into early and late: Nos. 8 and 11, early stage. In these there were many tubules showing active spermatogenesis, but many in which there was very obvious regressive atrophy of the spermatogenic cells and thickening of basement membrane. There were pronounced changes of the Leydig cells in both (*vide* fig. 2, Plate IV, Part II).

The *Second Stage*.—More pronounced spermatogenic regressive atrophy of tubules, thickened basement membrane, Nos. 1, 4, 5, 7, 12, 14, 18, 23, 27, but still some tubules show spermatozoa. No eosin clumps of Leydig cells seen with low power (fig. 4, Plate IV, Part II).

¹ Figs. 4 and 5, Plate II, Part II.

The Third Stage.—No spermatogenesis, tubules atrophied, often containing only Sertoli cells, with lipid granules, thickened basement membrane. No normal Leydig cells—generally increase of fibroblasts (figs. 5 and 6, Plate II, Part II).

There is a general correspondence in the amount of regressive atrophy of the interstitial cells and the spermatogenic epithelium. In the normal, and in certain forms of mental disease, especially general paralysis, epilepsy, senile dementia, organic dementia, Korsakoff psychosis and some cases of manic-depressive insanity without dementia, the interstitial cells can be seen as eosin-stained clumps with a low power (*vide* figs. 1 and 3, Plate IV). This occurred in only two cases, in which the patients had only been eighteen months in the asylum (*vide* fig. 2, Plate I). As I have mentioned that in prolonged and extensive suppuration occurring in hospital cases the eosin substance disappears, it might be said that the absence of clumps of eosin-stained cells visible with a low power was due to the intercurrent disease, especially pulmonary tuberculosis, which accounted for death in seven of the twenty-seven cases. But four of the number died of acute lobar pneumonia after a few days' illness and some from acute or subacute dysentery, and the normal Leydig cells were not visible in all but two of these, presumably early cases (4 and 6, Table I). Moreover, in general paralysis and other psychoses or brain disease, the intercurrent disease did not cause a disappearance of the eosin-stained clumps of cells. It may, therefore, be presumed that in dementia præcox there is a regressive atrophy and failure of function of the interstitial cells coincident with, and in great measure proportional to, the regressive atrophy of the spermatogenic epithelium. With this regressive atrophy is a diminution and in a few cases an almost total disappearance of the interstitial lipid.

The pathological changes thus appear to affect the functions of the interstitial cells and the spermatogenic epithelial cells. In some few cases, especially in the cases of psychoses, death occurring in the post-adolescent stages of life, the two tissues are not equally affected. In the manic-depressive type several cases showed fairly normal spermatogenesis and a marked diminution of normal interstitial cells. Again, in a case of chronic Korsakoff psychosis with cirrhosis of liver and carcinomatosis, the spermatogenic tubules showed no spermatogenesis but fairly normal interstitial cells. A glance at Mr. Kenneth Walker's cases of men dying late in life, shows that sometimes the degree of regressive atrophy of the two tissues do not appear to coincide (*vide* Table II, p. 24). Of course, allowance must be made for the fact that this is not conclusive evidence, for one part of the testis may show active spermatogenesis, another not; and again, one part may show normal interstitial cells, another not.

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PHOTOMICROGRAPHS.

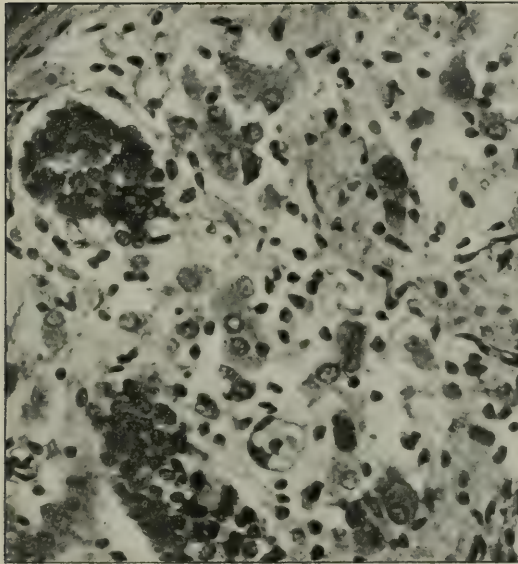


FIG. 1.

(1) Section of testis of new-born child. Showing embryonic tubules and polygonal mature interstitial cells with round nuclei. Many small immature cells are present but not so distinctly seen lying in loose areolar tissue. (Staining hæmatoxylin-eosin. Magnification 410.)

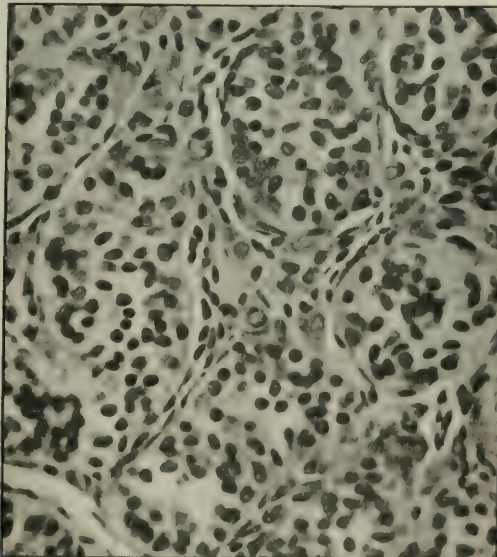


FIG. 2.

(2) Section of testis of child, aged 4 months. The tubules are nearly double the size and approximated; here and there are small areas containing a few small faint pink polygonal cells, but for the most part the normal interstitial cells have disappeared. The portion of the section in the centre containing the residue of the interstitial cells was found after search. (Staining hæmatoxylin-eosin. Magnification 430.)

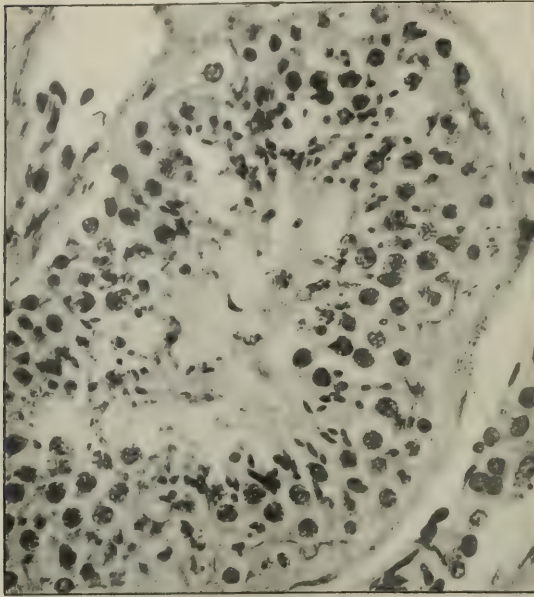


FIG. 3.

(3) Section of testis, Case 6, Table I. Low power, showing tubule with active spermatogenesis. In some places the spermatozoa are normal in appearance and staining reaction; in others they are of unequal size and abnormal form and staining. (Staining Heidenhain-hæmatoxylin. Magnification 500.)

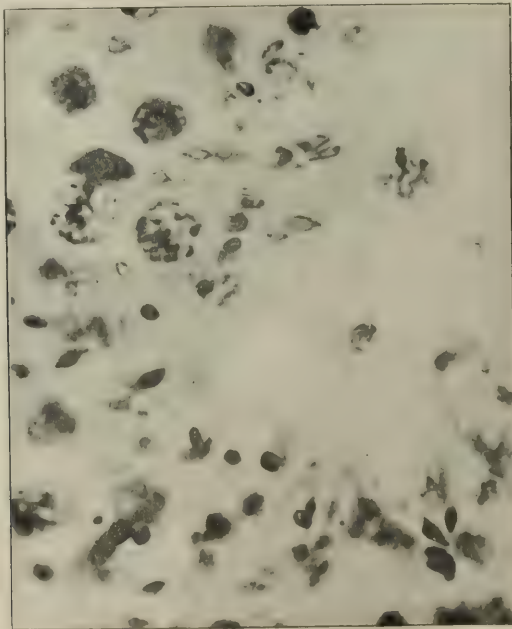


FIG. 4.

(4) High power (oil immersion) of the same section, showing a group of these degenerated spermatozoa. Observe the heads of the spermatozoa are of unequal size and shape, and do not take the nuclear stain, although the chromatin in the adjacent cells is well stained. (Magnification 1,200.)

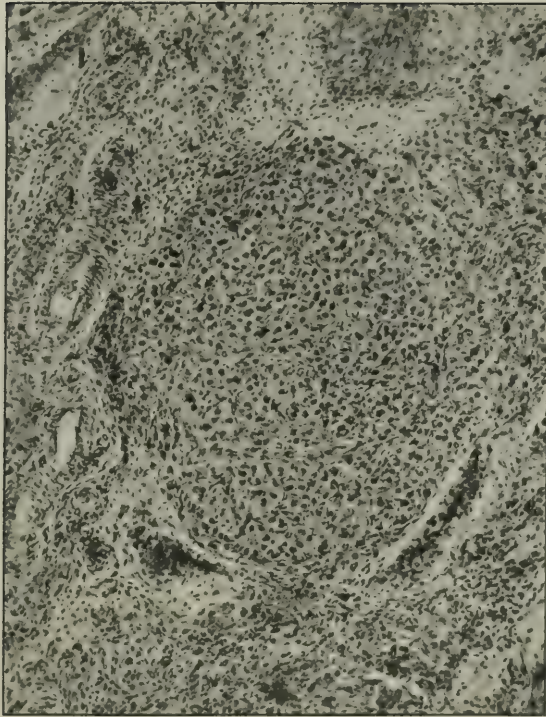


FIG. 5.

(5) Section of testis. Nodule of interstitial cells. Case 25, Table II. Extreme atrophy of tubuli seminiferi. The interstitial cells form a nucleated syncytium. Some contain lipoid substance, and the cytoplasm does not stain with eosin. (Magnification 120.)

EXPLANATION OF PLATE I.

FIG. 1.—Section of testis from a case of death from shock due to severe injuries. A number of interstitial cells are seen of varying size. The eosin-stained cytoplasm is vacuolated. The nuclei are well stained by the basophil dye, and show in two of the cells active mitosis.

FIG. 2.—Section of testis from Case 6, Table II. Many interstitial cells are seen around a small vessel. They are not so numerous as in fig. 1, and there is a vacuolated syncytium in the lower part of the figure. The flattened cells of the basement membrane are increased.

FIG. 3.—Section of testis of dementia præcox advanced second stage. A small vessel in centre. Above thickened basement membrane with flattened nuclei a few small immature Leydig cells are seen. A few large with vacuoles. A large number of oval and polymorphic pale nuclei, some of which are fibroblastic, are seen. Compare with figs. 1 and 2. The appearances are similar to the regressive atrophy of the interstitial cells seen in photomicrograph fig. 3 of the infant's testis at 4 months.

FIG. 4.—Section of testis of primary post-adolescent dementia, Table IV, Case 10. Nearly all the interstitial cells are markedly pigmented. The nuclei are large, oval and pale, indicative of a deficiency of chromatin.

All the specimens were stained with hæmatoxylin eosin, and the magnification one-twelfth oil immersion, and No. 4 ocular Zeiss.

PART II.

The Morbid Histology of the Testes in *Dementia Præcox*.

By MIGUEL PRADOS Y SUCH, M.D. Madrid.

SIR FREDERICK MOTT has placed at my disposal the testes of a number of his cases, and I have examined sections which I have prepared by the Del Rio-Hortega silver method, which is especially useful for demonstrating the spermatozoa and nuclear changes. I have also examined a number of his sections of testes stained by hæmatoxylin-eosin with a view of showing the changes in the interstitial cells in dementia præcox and other diseases when compared with the normal. My findings agree with the description which he has given in Part I. But before proceeding to describe the results obtained by my investigations I will describe the Del Rio-Hortega method, which I generally used. Of the various modifications of this method the following was the one generally used by me, as the tissues had been hardened in formol:—

NUCLEAR STAINING.

Sections cut by the freezing method are taken from distilled water, placed into the silver solution (*see below*), and warmed at a temperature of 50° to 55° C. until slightly yellow in colour. They were then placed direct into a 2 per cent. solution of formol (neutralized by calcium carbonate) until the section becomes black. Washed in distilled water for about one minute and then placed into gold chloride solution (1 in 500) until the section becomes grey, usually about half a minute. Then into a 5 per cent. solution of hyposulphite of soda for five minutes: in this it becomes a rose colour. Wash in water, fix section to slide, blot off excess of water, and dehydrate with a creosote-carbolic-xylo mixture (creosote, 10 c.c.; carbolic acid, 10 grm.; xylo, 60 c.c.), and mount in Canada balsam.

Silver Solution.—5 c.c. of a 10 per cent. solution of aq. No. 31, precipitated by 20 c.c. of a 5 per cent. solution of carbonate of sodium; dissolve precipitate by few drops of ammonia, and add water to 55 c.c.

PLATE I.

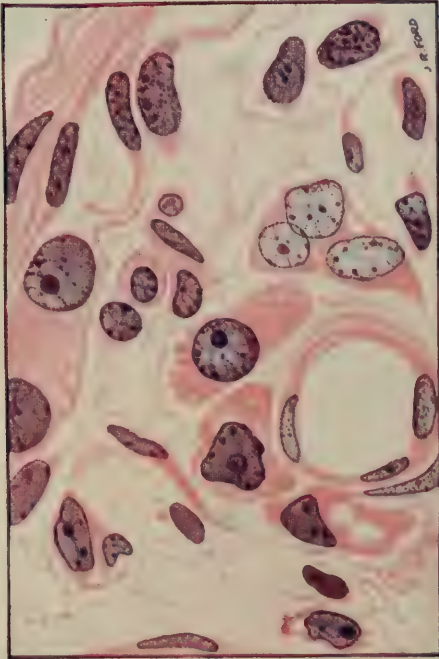


FIG. 2.

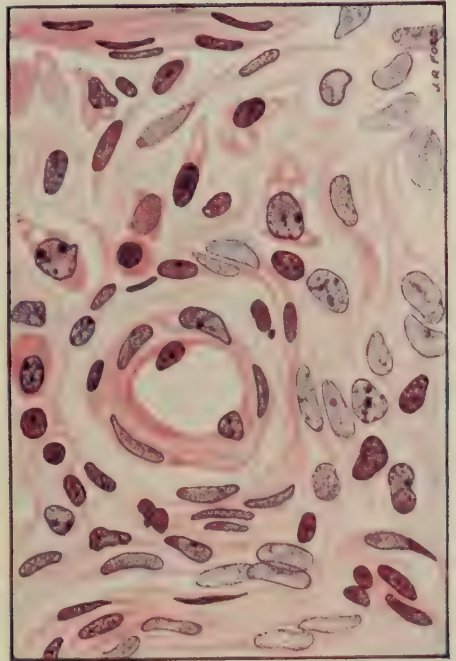


FIG. 3.

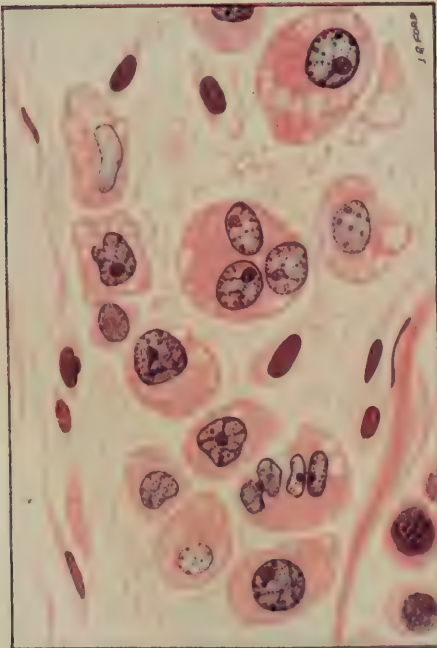


FIG. 1.

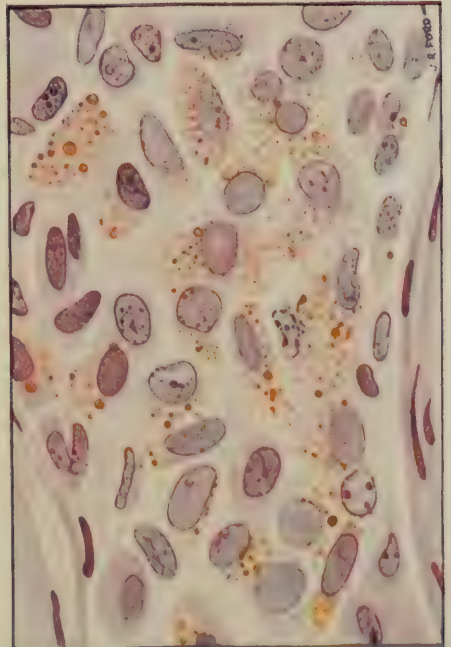


FIG. 4.

MOTT and SUCH:

Further Pathological Studies in Dementia Praecox.

SPERMATOGENESIS.

I have examined sections of a series of testes from the following cases and Plates II, III and IV with the descriptions summarize the histological changes I have observed.

(1) Normal, in which death resulted from shock caused by severe injuries (fig. 1, Plate II). It will be observed that the heads of the spermatozoa are uniformly stained and uniform in size and the spermatocytes all show nuclear skeins; there is no thickening of the basement membrane.

(2) Section of testis of No. 6, Table I, Part I. Section of the tubule in which the earliest changes can be observed. In many of the tubules these changes would not be found. Compared with the normal many of the heads of the spermatozoa are imperfectly stained, they are not of uniform shape and size; the tails are imperfectly formed and fewer spermatocytes show the nuclear skein; the basement membrane shows some thickening (*vide* fig. 2, Plate II). This change in the spermatozoa was observed also by the hæmatoxylin-eosin method of staining (*vide* photomicrograph, fig. 4).

(3) Section of testis of No. 8, Table I. Section of a tubule in which early changes can be observed in the spermatozoa similar to the previous one but rather more marked (*vide* fig. 3, Plate II). It will be observed that the testes in both these cases were of fair weight and the duration of time in asylum was under two years and showed first stage of regressive atrophy.

(4) Section of testis of No. 5, Table I, second stage of regressive atrophy. Some few tubules showed spermatogenesis but the great majority showed absence of spermatogenesis; formation of spermatids were found in some tubules, in others no heterotypical mitosis (*vide* fig. 4, Plate II).

(5) Section of testis of No. 15, Table I, third stage of regressive atrophy; no spermatozoa; no evidence of heterotypical mitosis, great thickening of basement membrane and overgrowth of fibroblasts (*vide* fig. 5, Plate II).

(6) Section of testes of No. 12, Table I, third stage of regressive atrophy. Great deficiency of nuclear stainable substance, absence of heterotypical mitosis, great increase of interstitial tissue and thickening of basement membrane (*vide* fig. 6, Plate II).

It will be observed by reference to the table that in these cases the weights of the testes correspond fairly well to the three stages of regressive atrophy of the organs. The results in the main confirm the findings obtained by Sir Frederick Mott by the methods he employed.

INTERSTITIAL TISSUE.

Examination of the interstitial tissue by the Del Rio-Hortega method. The normal case showed the following appearances. Single polyhedral cells with cytoplasm fairly deeply stained containing a round nucleus with deep chromatin staining and three crystals in the cell. A less well stained syncytium with six nuclei, each having a fairly well stained chromatin network. Within the syncytium of cells are seven varied sized crystals (*vide* E, Plate III). These were not seen in the pathological condition of the testis.

Section of testis No. 5, Table I, shows a syncytium of cells with fibrous tissue between and two nuclei of fibroblasts. The nuclei are pale and, excepting the nucleolus which is not observable in the normal interstitial cells, hardly take the stain. The cytoplasm is faintly stained in parts of the cells (*vide* F, Plate III).

Section of testis of No. 15, Table I, shows a number of cells separated by fibrous tissue and fibroblasts. The nuclei are very pale excepting the nucleolus: the cytoplasm is unstained (*vide* G, Plate III).

PLATE II.

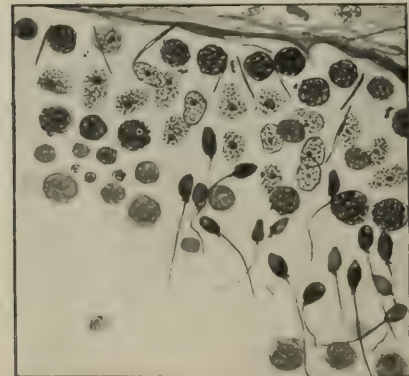


FIG. 1.

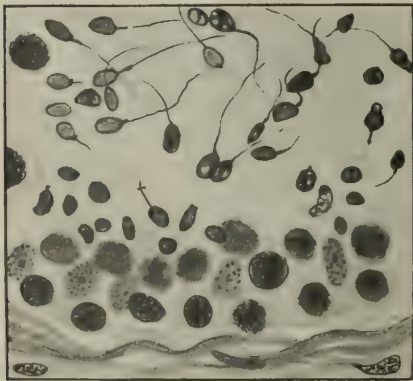


FIG. 2.

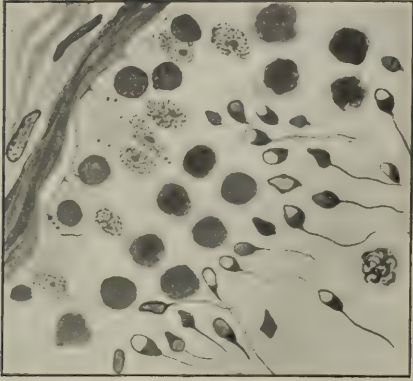


FIG. 3.

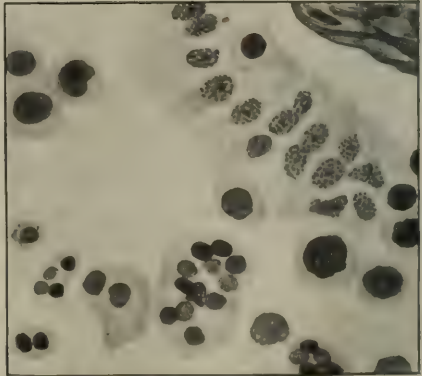


FIG. 4.

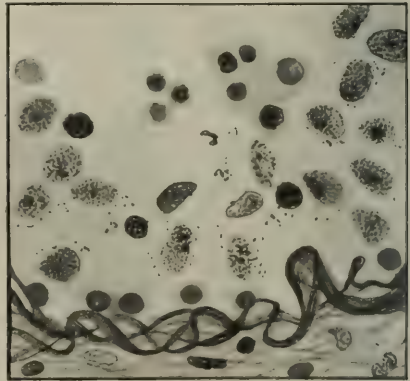


FIG. 5.

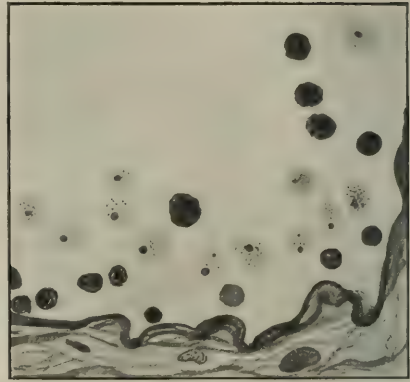
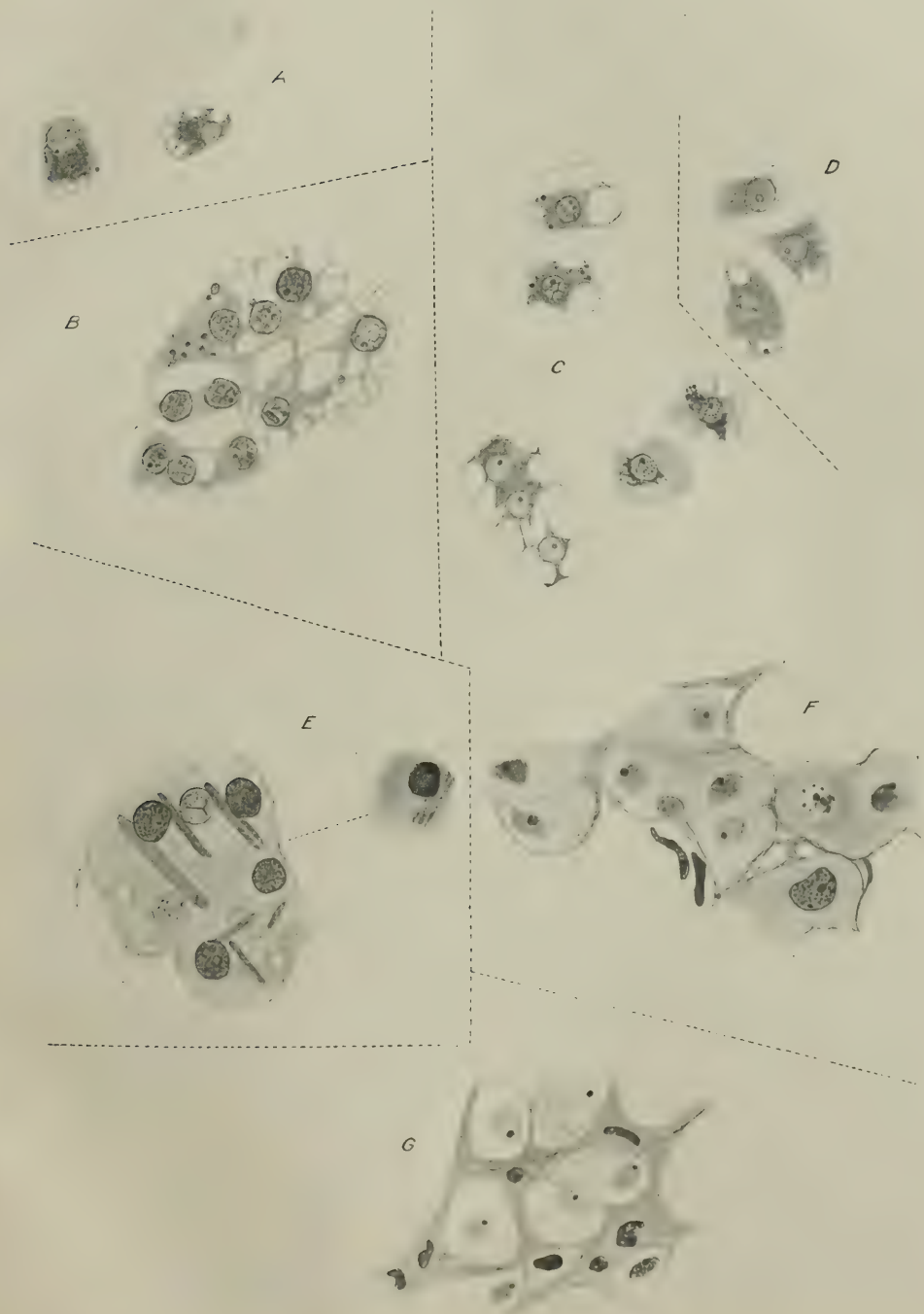


FIG. 6.

PLATE III.



INTERSTITIAL CELLS STAINED BY DEL RIO-HORTEGA METHOD (PLATE III).

Two pigmented cells: A, Plate III, showing a mass of pigment occupying the greater part of the vacuolated cells. The nucleus is very pale and contains very little chromatin (Case 7, Table I).

A group of vacuolated interstitial cells, B, Plate III, showing nearly normal staining nuclei, some pigment granules (Case 14, Table I).

Three sets of vacuolated cells, one forming a syncytium by the vacuolation: C, Plate II, nuclei round but paler than normal; the other pairs of cells show pigment granules in the cytoplasm (Case 9, Table I).

Three cells from a surgical case, D, Plate III, showing vacuoles at the periphery of the cell, no pigmentation although obtained from a man in the prime of life.

Magnification oil immersion lens and No. 4 ocular.

INTERSTITIAL CELLS.

Examination of Hæmatoxylin-eosin Preparations.

(1) Section of testes of a man, aged 81, suffering with senile dementia referred to in Part I. On the left a portion of tubule is shown exhibiting spermatogenesis with normal stained heads of spermatozoa, the basement membrane of this tubule and the adjacent one, which shows normal staining spermatogonia and spermatoocytes, is thickened by an increase of fibroblasts. Above is an isolated small interstitial cell and above this a group forming a syncytium by vacuolation, the nuclei of normal shape and staining; one cell contains pigment granules (*vide* fig. 1, Plate IV).

(2) Section of testis of normal case dying of shock described in Part I showing abundance of normal interstitial cells lying between the tubules. They are most of them mature and contain a good amount of eosin staining substance although the vacuolation would show that for the most part they contained lipid. There are three young cells lying together showing no vacuolation (*vide* fig. 2, Plate IV).

(3) Section of testis of a case of juvenile tabo-paralysis. Spermatozoa were found in the vesiculæ living eight hours after death. Most of the tubules showed normal active spermatogenesis. There was some thickening of basement membrane of tubules; this is seen in the tubule on the right by the increase of fibroblast nuclei. There are abundant normal interstitial cells which are visible as eosin-stained clumps with a low power. The nuclei are well stained and there are no fibroblast nuclei intervening (*vide* fig. 3, Plate IV). This should be compared with the next figure (4).

(4) Section of testis from Case 19, Table I. Observe the great fibroblast nuclear proliferation of the basement membrane upon which lies only a syncytium of Sertoli cells. The interstitial tissue consists of a very vacuolated syncytium of cells with pale nuclei of irregular form and size. The elongated more deeply stained nuclei are the nuclei of intervening fibroblasts (*vide* fig. 4, Plate IV).

SUMMARY OF THE LITERATURE OF THE INTERSTITIAL CELL.

The Interstitial Cell.

The interstitial cell consists of a more or less eccentrically placed mass of condensed granular cytoplasm containing a nucleus; the peripheral portion of the cell may however be extensively vacuolated. Von Lenhossek has in con-

PLATE IV.

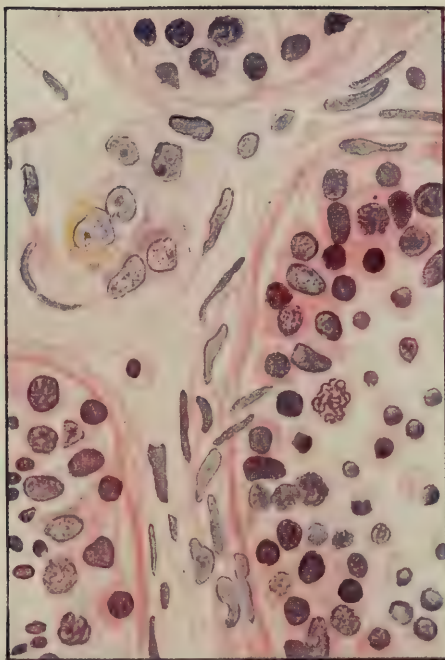


FIG. 1.

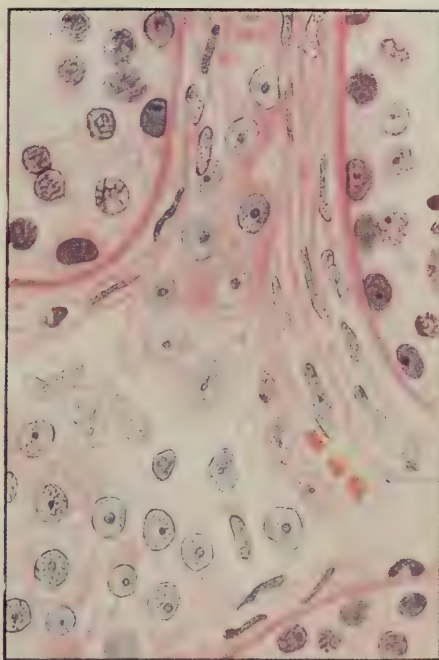


FIG. 3.

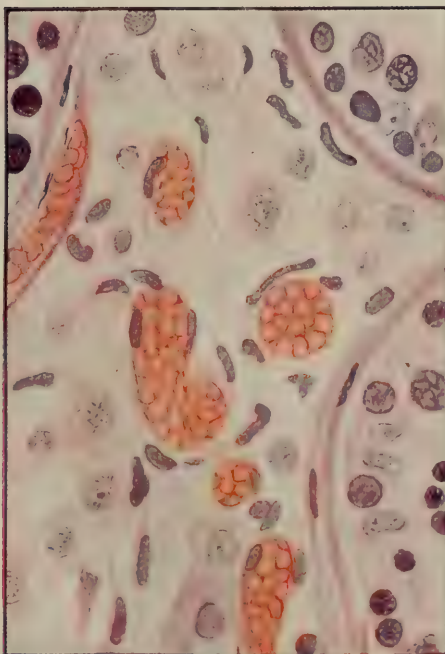


FIG. 2.

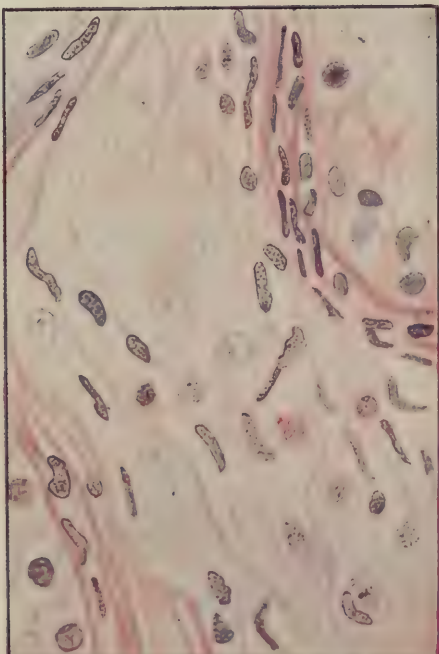
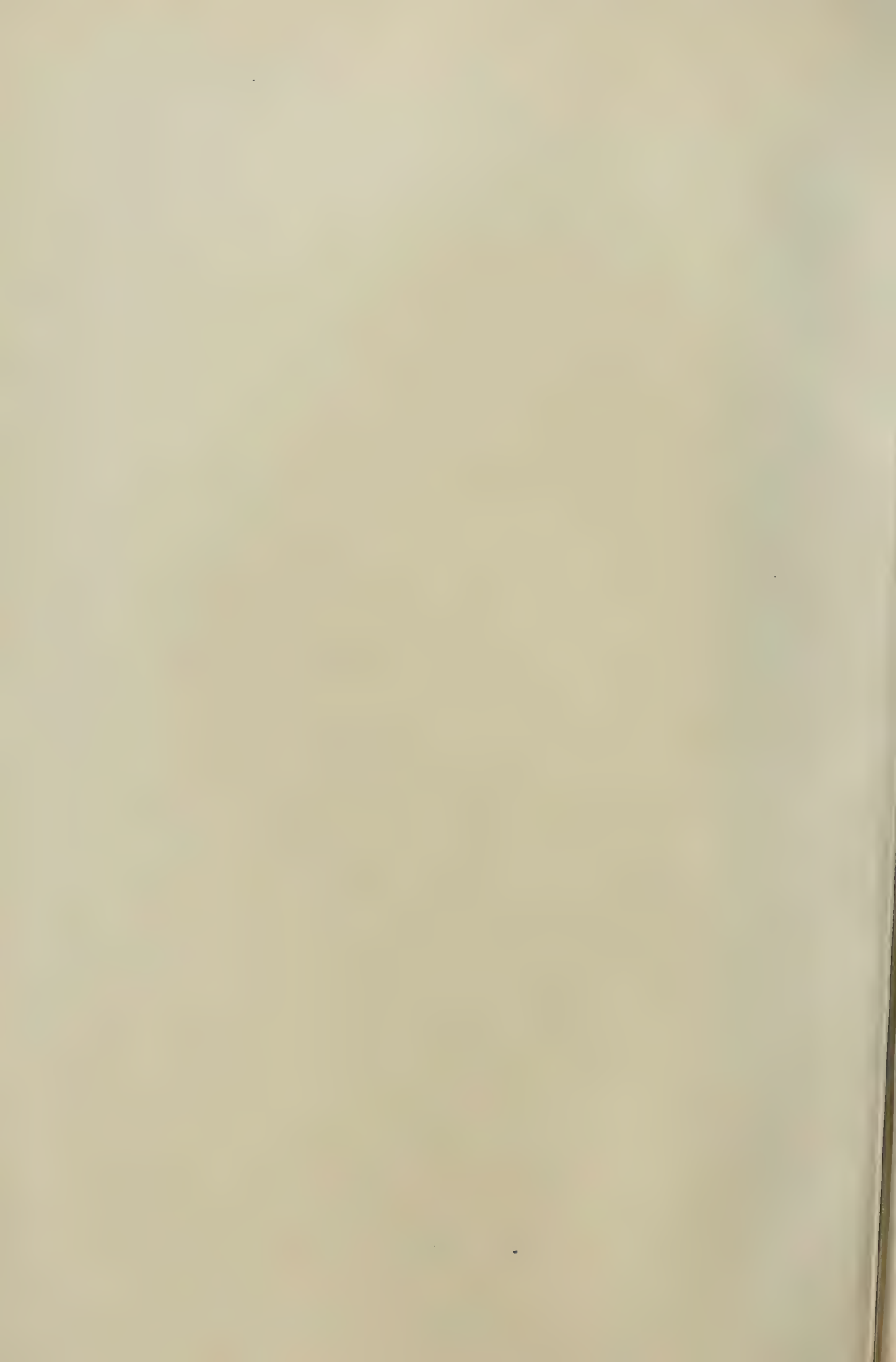


FIG. 4.



sequence applied the terms "endoplasm" and "ectoplasm." This typical structure is not found in all the cells but all gradations may be observed from cells whose bodies are composed entirely of "endoplasm" to those in which it is reduced to a remnant in the immediate vicinity of the nucleus. Plato regards these vacuolated forms as old cells, the opposite extreme being youthful forms; however their presence is restricted to a very few animals, if indeed it is not limited to man. They were found by Ganfini only in man. Whitehead, in a long series of mammals, also found them only in man. Nothing is known about their chemical composition and their inconstancy gives them little importance. But it has been pointed out in Part I that there is a correspondence of vacuolation and lipid in the cells.

Specific Granules.—Regand describes certain secretory vesicles in the rats' testicles fixed in Tellyesniczky's fluid (equal parts of 3 per cent. sol. potassium bichromate and 5 per cent. acetic acid). With many methyl blue-eosins they stain red; they are brought up well by iron hæmatoxylin, although the best is the Reinke's neutral gentian as modified by Dersley. In such preparations the majority of the interstitial cells contain definite granules, one or two microns in diameter, often in clusters. They lie for the most part in the peripheral portion of the cells but may be found anywhere in the cytoplasm. Each granule is contained in a distinct vacuole (thin sections). In the case of cells which contain many fat globules they and their granules must lie in the same vacuoles.

The staining reactions of these granules and their resistancy to acetic acid call to mind the zymogen granules of the pancreas. On the other hand, the reaction for prozymogen, which usually can be obtained in cells which produce zymogen, could not be obtained. But, without any reference to their chemical nature, we may regard the granules as an internal secretion of the interstitial cells.

The interstitial cells appear long before the epithelium of the tubules have become active.

Addison Thornton states that vital staining with trypan blue reveals two types of cells in the internal structure of the testis. One is elongated in the form of fibroblast, while the other is rounded or polyhedral in shape. Both types, according to the definition of Evans, are to be considered as macrophages and are not, as Goldmann interpreted them to be, identical with the interstitial cells of Leydig which represent the testicular organ of internal secretion. The vital staining with counterstain make it possible to differentiate between the interstitial cells and the macrophages.

Pigment.—"This is not present in any of my material (man, cat, opossum, pig, rabbit, dog, sheep, bull, grey squirrel and rat) and accordingly is not a constant content of the interstitial cells." Sehrt, who has made the latest study of its nature, finds that it is fatty, staining well with Sudan III in frozen sections and faintly even in material that has been treated with alcohol. He considers it to be a waste pigment.

Crystalloids.—The discovery by Reinke in 1896 of crystalloids in the interstitial cells of an executed criminal aroused considerable interest at first and his findings were confirmed by Lenhossek and others. It soon became evident, however, that their presence is restricted to a very few animals, if, indeed it is not limited to man; they were found by Ganfini and by myself only in man. I shall content myself with simply noting their inconstancy.

Function.—Plato held that it was the function of the interstitial cells to act as nurse cells, passing their fat and pigment through minute canals in the

walls of the tubules to be received by the Sertoli cells and there used as pabulum in the formation of spermatozoa. His theory was supported in some measure by Friedman and von Lenhossek but no one has been able to confirm his statements as to the existence of canals in the walls of the tubules and the passage of fat through them. Indeed the presence of fat in the lymphatics of the testicle would indicate that the flow of fat is away from the tubules, while Ganfini thinks that this appearance is a secretory phenomenon unconnected with the production of fat. The analogy with what has been observed in various gland cells is certainly very suggestive of secretory function, but probably the activity of the cells is not limited to the formation of fat. It should be stated that the vacuoles are not always so smooth and regularly circular; frequently they are large, irregularly shaped cavities with more or less ragged margins, doubtless the result of the breaking down of the partitions between adjacent vacuoles. It is hardly necessary to add that the demonstration of the structure of the interstitial cells require fresh tissue and good fixation. Moreover the study of the development of the interstitial cells in different animals has shown us that frequently fat is present in the tubular epithelium before it appears in the interstitial cells and that in the pigmented cells it is never present in any but the minutest amount. Finally, as Ganfini has pointed out, in undescended testes, where the Leydig cells are usually typically developed and numerous, the tubular epithelium is undeveloped or atrophied and subsequently no spermatozoa are formed.

Ganfini believes that the fat itself is the internal secretion of the cells and is poured into the general circulation through the lymphatics. He bases his opinion upon the fact that fat is found in the lymphatics leaving the testis and that the fat in the cells is in the form of more or less discrete droplets rather than in large drops as in the ordinary adipose cells and consequently is no ordinary fat.

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SUMMARY OF INVESTIGATION OF VITAMINES AND THEIR RELATION TO INTERSTITIAL CELLS.

Ezra Allen: "Degeneration in the Albino Rat Testis due to a Diet deficient in the Water-soluble Vitamine, with a Comparison of Similar Degeneration in Rats differently treated, and a Consideration of the Sertoli Cells," *Anat. Record*, 1919, xvi, p. 93.

(1) Reduction in the quantity of water-soluble vitamine in the diet of rats results in total degeneration of all the germ cells, but does not interfere with growth and development in other respects; the Sertoli cells persist.

(2) In the male this atrophy of germ cells is accompanied by hypertrophy of the interstitial tissue.

(3) The type of degeneration in the male germ cells is similar to that produced by X-ray treatment of the testis directly.

(4) A similar degeneration of the germ cells has been observed in a group of rats, part of which were subjected to prolonged alcoholization. The degeneration was found to a less extent in all but one of their five brothers not alcoholized. In this group hypertrophy of the interstitial tissue was not observed.

(5) Examination of this degenerated tissue and more careful study of normal, well-fixed tissue, confirms Regand's conclusions that the Sertoli cells form a syncytium.

(6) The nucleolus of the Sertoli cells under these degenerated conditions appears to be an equally bipartite instead of, as normally, an unequally bipartite body.

(7) The interstitial tissue is much increased in quantity in the rats put upon a reduced water-soluble vitaminé."

McCarrison, "Studies in Deficiency Disease," 1921, p. 139 :—

"One of the most remarkable results of foods deficient in vitamins is the constant and very pronounced atrophy of the testicles. It occurs in extreme degree, whether the dietary is exclusively composed of autoclaved rice or whether butter and onions are added; in the latter circumstances the atrophy is slightly less extreme. It appears then to be one of the most specific of the effects of avitaminosis in pigeons. . . . Histological examination shows a complete cessation of the function of spermatogenesis. The capsule of the organ and the intertubular trabeculæ are greatly thickened; the diameter of the tubules is lessened; spermatozoa, spermatids, and spermatocytes are wholly absent. The tubules are lined by a single but often incomplete layer of cells which still preserve, in a considerable proportion of their numbers, nuclei which, from their appearance and staining reactions, seem capable of regeneration."

Houlbert, *Paris Médical*, December, 1913 :—

"*Vitamine and Growth.*

"*Experiments in chickens* showed that when they were deprived of vitamins in their food the birds showed an arrest of growth and of the development of the secondary sexual characters (spurs, comb and tail feathers), and progressive anæmia. One bird killed on the fourteenth day, and on post-mortem examination was found to be in a state of extreme inanition. All organs appeared normal except the testes, which were very small, and on histological examination showed an arrest of the cellular divisions and metamorphoses which normally occurs in the seminal tubules. The interstitial cells of the testes showed a very pronounced infiltration of pigment, which as Bouin and Ancel have shown, occurs in the interstitial cells of glands whose endocrine glands are in decline. Sections of the suprarenals show an arrest of development of the chromatin cells."

SUMMARY BY SIR F. W. MOTT AND DR. PRADOS Y SUCH.

The general conclusions arrived at from the investigations contained in Part I and Part II are as follow:—

(1) The interstitial cells prior to birth act as sexual determinants, and at birth form the greater part of the interstitial tissue which constitutes the major part of the testes (*vide* fig. 1). They contain lipid granules. Moreover fine lipid granules are seen between the embryonic epithelial cells of the tubules.

(2) The interstitial cells after birth undergo a regressive atrophy and disappear; inasmuch as the seminiferous tubules at four months are twice the size of those at birth and are approximated, it follows that the fine lipid granules which are found between the epithelial cells, have, in all probability, served as a pabulum for their formative activity. But since there is still lipid in the residual interstitial cells (*vide* fig. 2), this correlation of function of the interstitial cells and the epithelial formative activity has not ceased, and it is reasonable to assume that had the child been 6 months old at death, it would have ceased and no lipid would have been found anywhere and the following facts support this conclusion. At 10 years the tubuli seminiferi are for the most part approximated and as a rule are only a little larger than those at 4 months; there is no lipid in the interstitial tissue, or in the tubules. There are occasionally to be seen mitotic figures as if spermatocytes were commencing to be formed, but no Sertoli cells are observable. In the interstitial tissue are seen numbers of oval, round and polymorphic nuclei, not nuclei of fibroblasts, and occasionally a definite small polygonal cell, eosin stained, can be seen, indicating that *pari passu* with the tubular epithelial formative activity there is a reappearance of the interstitial cells.

(3) The appearances of these immature interstitial cells resemble in many respects the appearances presented by the interstitial cells in advanced cases of dementia præcox (*vide* fig. 3, Plate I).

(4) At puberty and adolescence the tubules have increased in size owing to active proliferation and spermatogenesis. Abundance of mature interstitial cells are present (*vide* fig. 1, Plate I), which are undergoing active functional change; they contain lipid, and lie upon a lymphatic space which surrounds the tubule. Reasons are given why it may be assumed that this lipid substance passes through the basement membrane to the Sertoli cells which contain fine lipid granules and serve as nurse cells to the spermatozoa.

(5) Microscopic appearances indicate the continuous development of new interstitial cells which mature, actively function and decay. They are present in extreme old age (*vide* fig. 1, Plate IV) and sometimes when spermatogenesis has ceased.

(6) The microscopic examination of the testes of twenty-seven cases of dementia præcox, all commencing in prepubertal, pubertal, or adolescent stages, are described together with the age of admission and duration of asylum treatment, and age at death with cause of death and principal mental diagnostic conditions are given in Table I.

(7) It may be noted that a number of cases died of pulmonary tuberculosis, but a number died of acute disease, e.g., pneumonia and dysentery and after a few days or a week or two of illness. The microscopic conditions did not differ essentially from those dying of pulmonary tuberculosis. In some there was no history of masturbation, whereas in others there was definite information, but the microscopic examination did not reveal any difference.

(8) The regressive atrophy found microscopically corresponded, generally speaking but not always, with the loss of weight of the testes and the naked eye appearances. As a rule the longer the duration of the mental symptoms the more pronounced was the atrophy, but duration of asylum treatment does not strictly connote the length of duration of symptoms.

(9) The regressive atrophy, as determined by microscopic examination, has led me (F. W. M.) to divide the cases into three groups. *The first stage*, in which the changes indicate the formation of normal and degenerate spermatozoa (*vide* fig. 4 and figs. 2 and 3, Plate II) and commencing failure in the formation of normal interstitial cells and by special staining an increase of interstitial fibroblasts. In *the second stage* there is, in addition, an obvious shrinkage of many of the tubules, increase of fibroblasts, thickening of basement membrane and failure of spermatogenesis. The mature interstitial cells are fewer in number and there are numbers of immature cells with pale nuclei deficient in chromatin (*vide* fig. 3, Plate I, figs. F and G, Plate III, and fig. 4, Plate IV). In *the third stage* the tubules either show no spermatogenesis, or only a few tubules relatively show some spermatozoa, some being degenerate; there is a failure of formative nuclear activity and many or (in advanced cases) all the tubules consist only of a very thickened basement membrane lined by Sertoli cells. These cells usually contain lipoid granules in the syncytium, and when this occurs there is lipoid in the interstitial tissue and cells. This indicates that the essential feature of the atrophy is a primary germinal defect.

(10) In seven of the cases of dementia præcox a pigmentary deposit was found in the interstitial cells (*vide* fig. 4, Plate I) which is not seen in normal conditions except in old age, and therefore may be regarded as evidence of pre-senile change.

(11) Table III gives a summary of results obtained in nine cases of psychoses other than dementia præcox, death occurring in post-adolescence. It will be observed that similar appearances of regressive atrophy of the testes occur in many of these as are found in dementia præcox. Three cases of manic-depressive insanity without symptoms of dementia showed normal active spermatogenesis, but apparently a diminution of normal interstitial cells, although death occurred in two of them from pulmonary tuberculosis. In a case of alcoholic dementia, aged 65, the interstitial cells were fairly normal although there was absence of spermatogenesis; the cause of death may account for this.

(12) There are four cases in Table IV in which symptoms of dementia præcox came on in post-adolescence and all of these showed marked regressive atrophic changes of the tubules and the interstitial cells similar to those observed in cases commencing in early life (*vide* fig. 4, Plate I).

A recurrent manic-depressive insanity may terminate in dementia, e.g., No. 6, Table III, and then regressive atrophic changes are found exactly similar to those met with in dementia præcox. Otherwise manic-depressive insanity does not show these regressive atrophic changes in the testes. It will be interesting to see whether there are changes in the brain corresponding to those I have described in dementia præcox in these cases.

(13) As a contrast to these regressive atrophic changes occurring in the biogenetic psychoses are the changes in the testes of cases of general paralysis—an acquired disease. Whereas in the former the atrophy is primary and affects more or less the whole organ, in the latter it is secondary to inflammatory changes in the epididymis, either gonorrhœal or syphilitic, and causing a complete disappearance of the epithelium of the tubules by obstruction of the

vasa efferentia. The result is local patches of dense fibrous tissue affecting especially one testis, sometimes both. In the immediate neighbourhood are tubules showing normal active spermatogenesis and Leydig's cells. Not infrequently amidst the atrophied tubules consisting only of thickened basement membrane are seen nodules and groups of fairly normal interstitial cells.

In spite of this secondary atrophy which affects the testes of so many paralytics, the average weight of the pair after removal of the tunica vaginalis and epididymis is 8 gm. heavier than the testes of cases of dementia præcox. Whereas in the great majority of cases of dementia præcox an emulsion of the testes showed no spermatozoa, the converse was found in general paralysis.

(14) Previous studies show that the changes in the reproductive organs is a part of a generalized germinal defect of durability and vital energy of the whole body most manifest in the brain, especially the cortex, and the reproductive organs.

TABLE II.—PERSONAL CASES OF MR. KENNETH WALKER, F.R.C.S.

Name	Age	Spermatogenesis	Leydig cells
(1) G. C.	59	Active spermatogenesis	Scanty
(2) A. (malignant prostate)	70	Some mitosis and a few spermatids seen	Increase of Leydig cells
(3) T.	57	Spermatogenesis	Scanty
(4) C.	46	Spermatogenesis	Fair number
(5) K.	70	Spermatogenesis	Fair number
(6) H.	80	Absent	Very scanty (this patient had marked mental symptoms)
(7) T.	60	Present	Scanty
(8) C.	60	Present	Fair number
(9) B. (malignant) ...	66	Absent (some mitosis, no spermatozoa)	Scanty
(10) T. (malignant) ...	63	Active	Fair number
(11) K. (malignant) ...	86	Absent	A few degenerating cells only
(12) T.	73	Present (becoming malignant) ...	Scanty
(13) T.	68	Spermatids seen, but no spermatozoa	Very scanty
(14) Cancer Hospital (case of pancreas)	?	Spermatogenesis present ...	Leydig scanty

TABLE I.—DEMENTIA PRAECOX, TWENTY-SEVEN CASES.

Number of card and name	Age at death	Duration of time in asylum	Diagnosis	Cause of death	Weight of testes in grammes	Microscopic examination
(1) S. A. L.	25	6 months	Dementia præcox; condition remained with slight change; symptoms of a stuporose, hebephrenic form of dementia præcox; has hallucinations and delusions	Acute pulmonary tuberculosis	10-10	Second stage; no normal Leydig cells with low power, pale syncytium in which are numbers of pale chromatin deficient nuclei of varied size and form, a few small cells with eosin staining; interstitial lipid much diminished
(2) W. R. M.	19	6 months	Adolescent insanity; made a violent attack upon his mother; irrational, deluded, sullen and depressed; masturbator	Wasting and exhaustion	8-8	Advanced third stage; no normal Leydig cells with low power, pale syncytium with pale chromatin deficient nuclei of varied form and size, fibroblastic overgrowth; interstitial lipid much diminished
(3) A.	21	9 months	Dementia præcox; threatened to kill his sister, suffered from insomnia, delusions, blind, the result of trying to gouge out his eyes; religious mania	Pulmonary tubercle, aortic hypoplasia	10-11	Advanced third stage; no normal Leydig cells with low power, pale syncytium with pale chromatin deficient or diffuse purple nuclei; excess of fibroblasts; early history of symptoms; but inasmuch as the secondary sexual characters were well developed it follows that the Leydig cells have degenerated since puberty
(4) M. G.	27	18 months (probably duration of symptoms much longer)	Dementia præcox; history of manic-depressive insanity in father; hallucinations, delusions, katatonie; attempted suicide; admits excessive masturbation	Lobar pneumonia	—	Second stage; not much increase of interstitial tissue, groups of eosin-stained cells seen with low power, but fewer than normal
(5) H. W.	21	18 months (for 3 years previously history in the Army of delinquencies)	Recent melancholia; had hallucinations; morose and confused, refuses to speak, with bursts of aggressiveness	Lobar pneumonia	13-8-15-8	Second stage; no Leydig cells seen with low power; isolated small eosin stained cells, pale vacuolated syncytium with oval, irregular pale nuclei deficient in chromatin (<i>vide</i> fig. 3, Plate I)
(6) F. A. E.	28	18 months	Primary dementia, fixed, stupid expression, never speaks unless addressed and then makes silly replies	Dysentery	19-19	Early first stage of regressive spermatogenic atrophy; active spermatogenesis in many tubules, but degeneration of many of the spermatozoa (<i>vide</i> photomicrographs 1 and 2); diminution of Leydig cells as compared with normal, fewer mature cells (<i>vide</i> fig. 2, Plate I)

TABLE I.—*DEMENTIA PRÆCOX. TWENTY-SEVEN CASES (continued).*

Number of card and name	Age at death	Duration of time in asylum	Diagnosis	Cause of death	Weight of testes in grammes	Microscopic examination
(7) L. W.m.	26	20 months	Dementia præcox; dull and apathetic with occasional outbursts of excitement, masturbation	Dysentery and pneumonia commencing (died after a few days' illness)	11-10	Second stage; no normal Leydig cells seen with low power; irregular and oval pale nuclei in vacuolated syncytium pale pink or pigmented; excess of fibroblasts
(8) T.	27	22 months	Dementia præcox; "makes grimaces and laughs without cause . . . wanders about gesticulating and doing strange things," has delusions; dull, stupid, no initiative; no note of masturbation	Broncho-pneumonia	19-16	First stage; no normal Leydig cells seen with low power; numbers of oval, pale, round or irregular nuclei in a pale unstained syncytium
(9) C. U.	24	2 years (about 2 yrs. duration of definite symptoms)	Dementia præcox; auditory hallucinations; violent at times, otherwise stuporose; no mention of masturbation	Broncho-pneumonia	15-16	Third stage; no Leydig cells low power, pale or pigmented syncytium with oval, round imperfectly stained nuclei, fibroblastic overgrowth
(10) H.P.	20	2 years (6 months before admission character changed)	Dementia præcox; duration one year. "attributes present state to a row he had with his brother"; was suspicious	Pulmonary tuberculosis	9-9	Third stage; no Leydig cells, high power, occasionally a pinkish vacuolated syncytium of cells with pale oval and irregular-shaped nuclei, fibrous tissue overgrowth
(11) S.E.	25	2 years	Dementia præcox; "confused, restless, reacts slowly to questions, deluded, dull, apathetic"	Acute lobar pneumonia (death after a few days' illness)	21-2; 15-1	Early second stage; only a few small imperfectly stained groups of Leydig cells seen deficiently stained with eosin; there are numbers of oval, round and irregular-shaped pale nuclei, many fibroblasts; fair amount of interstitial lipoid
(12) A.M.	25	2 years	Dementia præcox; dull and confused, indifferent to self and surroundings	Pulmonary tuberculosis	10-10	Advanced second stage; generally diminished interstitial lipoid; dense interstitial tissue fibroblast overgrowth; ill defined pale syncytium with pale nuclei oval or varied in shape and size, deficient chromatin or diffuse pale purple
(13) G.R.D.	22	2½ years	Dementia præcox; dull, stupid, very rarely speaks to anyone, untidy, habits faulty	Acute pulmonary tuberculosis	15-16	Third stage; greatly diminished interstitial lipoid; no normal Leydig cells, vacuolated eosin-stained cells with pale nuclei, deficient in chromatin
(14) S.T.	27	3 years	Dementia præcox; sullen and very taciturn, laughs silly manner, no cause, very impulsive and at times very violent and destructive	Dysentery	12-5; 46	Second stage; atrophy of Leydig cells, many cells pigmented

(15) C.	29	4 years	Dementia praecox; "he takes no notice of his surroundings"; manerisms, mutism and periods of impulsive excitement at one time associated with cataleptoid state or katonnia; family history <i>nil</i>	Phthisis	10-10	Third stage; no normal Leydig cells, occasional small isolated cells seen with low power otherwise vacuolated syncytium with pale oval and irregular nuclei; pigment in vacuolated cells
(16) M.J.	29	5 years (commenced before 25 for certain; how long before this not known)	Primary dementia of adolescence; hallucinations, delusions, attitude-mising, grimacing and other signs noted	Acute pulmonary tuberculosis	8-5-9	Third stage; no normal Leydig cells; numerous pigmented cells, pale syncytium with numerous pale nuclei of irregular shape
(17) C. F. G.	30	5 years	Paranoital form of dementia praecox with masturbation, manerisms, stereotypism; periods of katonnia and excitement	Exhaustion (blue hands and feet at death)	13-5-12	Third stage of regressive atrophy of tubules, no fibroblastic overgrowth; no normal Leydig cells; here and there an islet of Leydig cells seen with oil immersion, cytoplasm pale pink or pigmented; nuclei deficient chromatin, oval or irregular in size and shape
(18) B.	26	5 years	Primary dementia of adolescence; no history of masturbation in the notes	Tuberculous broncho-pneumonia, ulceration of infestines	13-5-11-5	Second stage; very little interstitial tissue and lipid; no normal Leydig cells, pale syncytium with nuclei of varied size and deficient chromatin, a few isolated Leydig cells
(19) S.H.	28	7 years (11 months prior to admission gradually became dull, apathetic and anergic)	Dementia praecox; did not brighten up at all; sat or stood for hours in one position, movements grotesque, showed some stereotypy; much addicted to masturbation before and after admission	Pulmonary tuberculosis	—	Third stage; interstitial tissue increased, islands and islets of Leydig cells containing lipid; Sertoli cells contain abundance of lipid; no normal Leydig cells but vacuolated syncytium containing nuclei of varied form and size, deficient in chromatin, excess of fibroblasts
(20) U.T.	33	7 years	Dementia praecox; dull, listless—only speaks in whispers, occasionally faulty in habits, some katonnia; sits in one place gazing as long as allowed; no masturbation during residence	Chronic dysentery, broncho-pneumonia	8-5-6-5	Third stage; interstitial tissue increased; no normal Leydig cells, vacuolated pale syncytium with here and there groups of pigmented cells; nuclei deficient in chromatin, variable in size and shape; excess of fibroblasts
(21) C. W. J.	35	8 years (about 8 yrs. duration, commenced at 27)	Dementia praecox; history of hallucinations, delusions, masturbation and terminal dementia	Broncho-pneumonia, pulmonary tuberculosis (probably 3 months' duration)	16-15	Early third stage; abundance of interstitial lipid and in Sertoli cells minute lipid granules can be seen passing through basement membrane; lipid granules visible in Leydig cells accounting for vacuolation in haematoxylin-eosin preparation; no normal clumps of cells; with oil immersion pale vacuolated syncytium with pale nuclei of varied form and size; here and there pigmentation

TABLE I.—DEMENTIA PRÆCOX, TWENTY-SEVEN CASES (continued).

Number of case and name	Age at death	Duration of time in asylum	Diagnosis	Cause of death	Weight of testes in grammes	Microscopic examination
(22) M. W. m.	35	10 years	Dementia præcox; two years after admission notes state that he is suffering with secondary dementia; stands in various attitudes in corners of grounds and wards with bowed head; cannot converse rationally, poor idea of time and place At age of 17 certified as mania; year before as suffering with delusions of persecution, possessed by devil, hopelessly lost; attempted to commit suicide; progress of case shows typical dementia præcox	Dysentery	17.5-14.5	Third stage; all the tubules are deficient in epithelial cells, many are only lined by Sertoli cells; a striking feature is the unequal nuclear staining of the remaining cells in the tubules; the interstitial tissue consists of an overgrowth of fibroblasts and a number of pale nuclei of varied size and shape, around which in places are little collections of pigment indicative of degenerated Leydig cells; no normal cells were observed Advanced second stage; the most obvious change is an increase of interstitial tissue, excess of fibroblasts; no normal Leydig cells; with oil immersion isolated pale syncytium cells with nuclei deficient in chromatin of varied size and shape, no pigmentation observed
(23) M. A. G.	27	10 years	Typical dementia præcox of ten years' duration, commenced at 26	Broncho-pneumonia	20.5-17	Third stage; increase of interstitial tissue, a few scattered nuclei of Leydig cells and vacuolated syncytium with pale nuclei of varied form and size, excess of fibroblasts; the spermatogenic cells more profoundly affected than the interstitial cells
(24) G. A.	36	11 years	Dementia præcox; "earned his own living until six years ago," four times sentenced to prison; masturbator; had delusions, hallucinations; was irritable, excited, incoherent, and had innumerable mannerisms; verberation; no signs of congenital syphilis	Apical tubercle and cholelithiasis	12.0 Left testis absent; left supra-renal 6.5 gram., right 12 gram. 5.5	Third stage; capsule of testes greatly thickened; seminiferous tubes, extreme regressive atrophy; no lipid in Sertoli cells; nodules of Leydig cells, many containing lipid granules; these nodules stained with hæmatoxylin eosin show a vacuolated syncytium of cells with abundant nuclei of varied form and size containing a fair amount of chromatin, but there is no eosin staining of cytoplasm; the specimen is not unlike that of a cryptorchid (<i>vide</i> photomicrograph, fig. 5)
(25) W. H.	35	14 years	Dementia præcox; dull, morose and taciturn; tried to cut his throat; sometimes violent and excited; confirmed masturbator	Exhaustion	7.5-9	Early third stage; very little interstitial or intertubular lipid; no normal Leydig cells seen
(26) D. F. W. m.	33	15 years	Katatonie dementia præcox; disturbance and adolescence are given as causes; he suffered with emotional indifference, mutism, katatonie and acrocyanosis; did not obey calls of nature, and required constant supervision; destructive tendencies	Adherent lungs, probably tubercular	10-10	Advanced second stage of spermatogenic atrophy; no normal Leydig cells seen; syncytium with <i>very</i> marked pigmentation everywhere; nuclei varied in size and shape with deficient chromatin excess of fibroblasts; suprarenals very small, deficiency of medullary substance
(27) B. F.	35	15 years, (15 years' duration at least)		Acute broncho-pneumonia		

TABLE III.—SUMMARY OF RESULTS OBTAINED IN NINE CASES OF PSYCHOSES OTHER THAN DEMENTIA PRECOX OCCURRING IN POST-ADOLESCENCE.

Number of case and name	Age at death	Duration of case in asylum	Diagnosis and symptoms	Cause of death	Weight of testes in grammes	Microscopic examination
(1) G. J. A.	50	3 years	Subacute melancholia, epilepsy	Broncho-pneumonia; gangrene of lung; epilepsy	9.9	Advanced second stage of regressive atrophy of tubules and Leydig's cells; pigmentation
(2) B. G.	—	18 months	Persecutory insanity	Œdema of glottis; fatty heart	9.7	Second stage of regressive atrophy of tubes, islets and islands of Leydig cells seen with low power eosin stained, some pigmented
(3) D. E.	48	20 months	? Confusional insanity; serous meningitis; operation for tumour	Lobar pneumonia	18-13	First stage advanced of regressive atrophy, atrophous pigmentary degeneration of Leydig's cells
(4) M. T. G.	58	4 months	Melancholia, hypochondriasis	Exhaustion; bronchitis;	12-12	Tubules active, spermatogenesis in most of the tubules; nothing abnormal except diminution in numbers of Leydig cells
(5) A. T.	65	22 years	Korsakoff psychosis	emphysema Cirrhosis of liver; carcinomatous; ascites	14-14	Complete arrest of spermatogenesis, thickened basement membrane; Leydig cells considering age fairly normal in numbers and staining reaction; not pigmented
(6) N. P.	53	9 years	Recurrent manic-depressive insanity, dementia	Paracetesis	9-11	Third stage of regressive atrophy of tubules and Leydig cells; pigment granules in syncytium
(7) H.	36	First attack at puberty; four times in Claybury; subsequent 14-40 days the last time, then died	Recurrent manic-depressive insanity	Pulmonary tubercle Acute broncho-pneumonia	?	Many normal tubules showing active normal spermatogenesis and spermatozoa; Leydig cells for the most part pale syncytium with faint eosin staining; some normal groups of eosin-stained polygonal cells seen, some pigmentation, no overgrowth of fibroblasts
(8) H. T. L.	41	22 years	History points to alternate periods of depression and excitement. (Clinical notes: melancholia, restless, miserable and depressed, a furtive expression; answers questions readily, but becomes incoherent; has hallucinations of sight and hearing; memory fair for remote events, poor for recent events; poor health and condition)	Pulmonary tubercle	15-20	Spermatozoa from vesicula alive eight hours after death; many tubules show all stages of spermatogenesis; normal staining and shape of spermatozoa; no failure of nuclear staining; Leydig cells in small clumps seen with low power; oil immersion examination; no pigmentary degeneration; no excess of fibroblasts; occasional normal nucleated polygonal cells well stained; majority vacuolated syncytium with fairly normal nuclei; fair amount of interstitial lipid and in Sertoli cells
(9) L. C.	50	History of attack 4 yrs. previously; present attack 40 days	Manic-depressive insanity	Pulmonary tubercle	10.2-12.2	Vesicula seminalis abundant spermatozoa; spermatic tubules fairly normal for the age; quite one-half show all stages of spermatogenesis; basement membrane not thickened; interstitial tissue much diminished; no excess of fibroblasts; low power only slight evidence of interstitial cells; with oil immersion: a vacuolated faintly pink stained syncytium; nuclei pale, irregular, small crumpled as if they had undergone atrophy or were immature; pigmentation of the cytoplasm. Conclusion: interstitial cells affected out of proportion to the spermatogenic

TABLE IV.—PRIMARY DEMENTIA IN POST ADOLESCENCE.

Number of case and name	Age at death	Duration of time in asylum	Diagnosis and symptoms	Cause of death	Weight of testes in grammes	Microscopic examination
(10) V. W.	46	1 week in asylum; history of symptoms 3 years prior to admission to asylum	General paralysis, as he had delusions of grandeur, but blood and cerebro-spinal fluid gave negative reaction, and examination of brain showed no signs of this disease; he made grimaces and had a constant habit of wiping his mouth, and impulsive outbursts like a dementia præcox case; he had no loss of orientation in time and space	Sub-acute dysentery; granular contracted kidneys	11.5-10.5	Vesicula seminalis abundant degenerated spermatozoa; testes: less than normal amount of interstitial tissue; basement membrane of tubules thickened, little evidence of heterotypal mitosis; absence of spermatozoa; the interstitial cells are shrunken, deficient in eosinophil substance; oil immersion examination: pale vacuolated cytoplasm containing pigment granules; nuclei are pale and deficient in chromatin; a few small eosin-stained cells seen (<i>vide</i> fig. 4, Plate I); the abundance of spermatozoa in vesiculae can be explained by the fact that this is not unusual in dementia præcox; the testes might well pass for a case of dementia præcox in early third stage
(11) B. W. M. G.	52	13 years	Primary dementia; demented, confused, irrational, suicidal, incoherent, hallucinations, grimacing and other symptoms like dementia præcox	Broncho-pneumonia	12-12	Vesiculae small contracted thickened walls, very few degenerated spermatozoa; tubes thickened, basement membrane many only lined by Sertoli cells, no spermatozoa seen; interstitial cells not visible with a low power. With oil immersion: isolated small groups of cells pigmented or vacuolated, nuclei small, pale, irregular and crenated; fibroblasts increased; complete primary regressive atrophy of testis
(12) G. W. M.	56	3 years	Katatonik ecstasy of primary dementia	Chronic renal disease, bronchitis and emphysema; internal hydrocephalus	10.3-10.7	Vesiculae spermatozoa but degenerated. Many of the tubules show heterotypal mitosis and spermatids, some spermatozoa, but these tubules show a degeneration of the spermatocytes and spermatogonia as if their productive energy had come to an end; there is a disintegration of the cytoplasm and the nucleus; the basement membrane is thickened, and in many tubules there are only spermatogonia and Sertoli cells; the interstitial tissue consists of fibroblasts, and no normal Leydig cells are seen; with oil immersion there are occasional islets of small irregular nuclei; the thyroid weighed only 6.6 grm., probably the increased weight of the pituitary 0.61 may be correlated with this; the suprarenals were only 3.81 and 3.51 grm.
(13) N. E.	44	25 years	Insanity began at the age of 19; he was certified and recertified as suffering with mania, and not until three years prior to his death does dementia occur in notes; throughout the twenty-five years he was in asylum there are periodic notes referring to masturbation, and not until 1913 is he certified as suffering with dementia; the notes state that he speaks in a falsetto voice; attempts were made to cure him of masturbation by blistering the penis	Chronic nephritis and congestion of the lungs	18.5-25 (weight increased by dropsy)	September 20, 1921: oil immersion examination of sections; a few of the tubules show some of all the stages in the formation of spermatozoa, but the great majority exhibit a regressive atrophied condition in all degrees to the final where the whole of the epithelial spermatogenic cells have disappeared leaving only the Sertoli cells. As a general rule the spermatozoa which are found in a good number of the tubules show normal karyokinetic figures. In the loose connective tissue isolated Leydig cells of fairly normal appearance are seen, seldom are groups of normal cells observed, and then never more than two or three; more numerous are cells which have appearance of degeneration or immaturity. There are obvious signs of œdema of the tissue, the lymph spaces in the interstitial tissue and the basement membrane can be clearly seen; doubtless this was caused by the kidney disease, and the œdema of the organs accounted for the weights; the appearances indicate that the weight was more than doubled by the œdema. This specimen shows that there are delicate lymph plexes in the basement membrane. Unlike most advanced cases of dementia præcox the histological changes rather suggest excessive masturbation as the cause, coupled with the effects of the chronic nephritis and œdema.

Section of Psychiatry.

President — Dr. BEDFORD PIERCE.

Some Points about Repression.

By T. A. ROSS, M.D.

ACCORDING to Freud the mind may be divided into a conscious, a fore-conscious and an unconscious part. The conscious contains that which is in consciousness for the moment; the fore-conscious that which can be fairly easily recalled, and which, when it is recalled, appears without distortion; the unconscious that which cannot be recalled by voluntary effort, and which appears in consciousness only in a state of distortion. The doctrine goes on to say that when a constellation of ideas has been repressed into the unconscious, psychical or emotional energy is repressed with it, and that unless this energy can be utilized by sublimation it may become a cause of symptoms, that, indeed, psycho-neurotic symptoms arise only from the damming up of this energy so that it can have no outlet save through them; in other words, that every neurosis depends on repression.

Before the war I obtained the histories of nervous patients, in the same way, so far as I can judge, as I do now. In those days histories with marked sexual trouble were not very commonly obtained. They did, of course, occur, but the difficulties which had baffled the patient were more often connected with other anxieties such as business worries, discontent with position in life, and so forth. I discussed these situations, and endeavoured to adjust the patient's attitude towards them; and also made liberal promises of cure. Undoubtedly the impression was obtained that a considerable amount of success was being achieved; this impression has not been lessened by the way in which these patients continue to send their friends for similar treatment. In those days I disbelieved the doctrines of Freud, for it seemed to me that if by psycho-analysis the Freudians were always discovering sexual complexes, they were doing something for the most part unnecessary, and very painful to the patient, for they were digging into matters that were dead and buried, and of no causal interest.

Then came the war cases, and at once I was face to face with the fact of repression in the Freudian sense of the word, and there seemed to be no escape from its overwhelming causal importance. I am not now referring to dissociations in which the dissociated material is removed from consciousness *en bloc* and has no distorted representation in consciousness, and in which, if it be restored to consciousness, it is restored in its original form. I refer to anxiety states in which the symptoms seemed to appear as the distorted form of a repressed complex. For example, a patient would feel uncomfortable when he heard an electric tram; he could give no reason for this until it transpired by the method of free association that the sound of the car resembled that of a distant shell. This carried him back to some experience where he had been

either ashamed or terrified, and which he had repressed, so it seemed, because his behaviour in it was in conflict with the high ideals of his personality. So a grudging admission was given to the doctrines of Freud—grudging because although Freud was clearly right about the importance of repression, he was not right in his statement that the neuroses were mainly sexual in origin. This attitude was grossly unfair, for I should have reflected that I was working on material where self-preservation was threatened, whereas Freud had been working on material where personal security seemed absolutely certain.

The attitude, however, was shared by many others; and the followers of Freud in this country are partly responsible for it. They persisted in trying to force the war neuroses into a sexual category in a way calculated to arouse the strongest opposition; what could one think of a teacher who said that bombs, aeroplanes and other engines of death were phallic symbols?

Now comes the post-war period. Before the war I had not often found a history of sexual trouble, and had been told that this was because it was repressed, and required the technique of psycho-analysis to bring it out. Now it pours out. Without being sought there comes a stream of material in the gravest conflict with the ideals of the personality—material which the patients clearly want to discuss, though doing so overwhelms them with shame, and which in former days we were led to believe was repressed. What is the meaning of this discrepancy? I do not think that it is because I have become better skilled in history taking; others have found the same thing, and it is not likely that we have all simultaneously acquired a skill which previously we had lacked. It is not that I spend more time over my cases, for I have, as a matter of fact, been unduly hurried of late. There is no doubt that people are more ready to discuss questions of sex than they were; I cannot now enter into the causes of this readiness, but I think it is the most important factor in the matter. Looking back to the pre-war period, I have no doubt that certain pauses and hesitations were due not to blankness of mind, but to the fact that the patient wished to make a sexual statement and was shy of beginning; it is not likely that the type of patient has changed.

Whatever the explanation, more light is needed on the question of repression—that is on the question of the pushing of constellations of ideas by a repressing agent into an unconscious, and the holding of them there by a censor which allows them into consciousness only if they are distorted. Dr. Rivers has already questioned the necessity for postulating this censoring agent in dreams in a paper on the "Affect in Dreams" [1] a paper well worthy of study. I had not read it till the bulk of this paper had already been written, but it encouraged me to deliver this.

(1) Does repression occur at all? The answer is undoubtedly, yes, but many of what have been described as instances of repression are nothing of the kind.

In "The Psycho-pathology of Everyday Life" [2] Freud gives an example which has achieved considerable notoriety. A young man with whom he was conversing attempted to make a Latin quotation, and was unable to complete it. The missing word was "aliquis." By the method of free association he came ultimately to a thought which was a source of distress to him, viz., that a certain lady might have become pregnant.

The steps of the association included these thoughts: aliquis, not liquid, St. Chrysostom, some other saints, St. Januarius, all the saints of the Calendar, the 'miracle of the liquefaction of St. Januarius' blood, finally the thought that the lady's blood should liquefy on the date of the Calendar on which it

should—i.e., the wish that she should menstruate. Now one of the canons of free association is that it *should* be free, free from self-criticism on the part of the subject, but free also from suggestions on the part of the observer. The word "Calendar" was not without significance in the above chain, and yet, incredible as it sounds, it was Freud and not the young man who introduced it into the conversation. We shall return to this. Again, the important thing was the possibility of pregnancy. There is no word in the story that this was buried in an inaccessible unconscious; and common sense—a thing not really to be despised though it is at present under a cloud—tells us that it must have been a thought that was frequently in the young man's consciousness, all the more since the story says that at any moment he expected the unpleasant news. An occurrence of such magnitude in a man's life is bound to be associated with a very large number of things; it is bound to come into consciousness quite frequently in any case whilst the anxiety is pressing. The teleological value of repression is to save pain and distress to the personality; repression would be a functionless thing but for this and the conception of the idea would never have occurred without it, but it seems incredible that the forgetting of the word "aliquis" could have saved him stumbling on the painful idea for any length of time: and why was there no intense resistance about forgetting St. Januarius who was in closer connexion? All that the story serves to show, is that if a person will go on talking freely about whatever comes into his mind, he will soon come to thoughts that are a source of present anxiety; and from this point of view free association is an excellent instrument to discover what are a man's present troubles, but no proof is afforded that it is an instrument with which we may probe the unconscious.

A second quite bad example of repression may be taken from Jung. It is given by Dr. Constance Long [3]. A nurse had stolen a purse the day before. Her responses to the word-association test were delayed on such words as "purse," "money," &c. The test is described as one to elicit repressed ideas, but that it could possibly have done so in this instance is ridiculous. The nurse was not a habitual thief; she knew she was suspect, and it is impossible to suppose that the affair should have become relegated to her unconscious so soon. The method is another example of a good way to get a person to confess what is in consciousness.

Much of the belief in repression rests on belief in the validity of free association; all the belief in Freudian symbols rests on it, and before we go further, a few remarks may be made on the subject. In Freud's story just quoted the word "Calendar" was of importance, and it is obvious that as it was interpolated by Freud himself, the validity of that instance of free association was destroyed. This is not the only example in Freud's writings in which he made suggestions which the patient accepted; and it is probable that the practice is common. Jelliffe says that at first he finds the associations "sticky" and that the patients must be pressed and pressed until they give them. I venture to suggest that this is nearly as illegitimate as is interpolation. If the symbol under discussion is a chimney emitting heat and power, and the associations are "sticky" and the patient is pressed hard, he will sooner or later come to mention the phallus; he cannot escape from it, but the association may not have been in his mind before, either consciously or unconsciously. If we say that it must have been present because it came out in association, that is equivalent to saying that it is impossible ever to have a fresh thought. Probably there are those who would say that we cannot have a fresh thought without a fresh experience; to this the answer is that the act of

free association is a fresh experience each time it is made. Therefore, it cannot be granted that if an idea comes to the surface by free association it has been necessarily repressed, even if the patient is certain that it has not been present in consciousness before.

It is further possible that when the idea of the phallus has been reached, the observer, having obtained his goal, may rest content, and not go on pressing the patient further to see if there be no other symbolism. The patient is in this way educated to see what the observer is seeking. As he is sure to be in a state of positive transference towards him—he will not give out his free thoughts otherwise—he will see that his master gets what he wants. We are told by the analysts that suggestion and positive transference are the same thing. That means that the patient is in a suggestible state if he is in an analysable condition. To a patient in such a condition the merest hint will carry weight, and it is not only hints that are commonly employed. I have lately treated several patients who had already been analysed by competent persons. They have explained to me that the Freudian symbols must be true because the interpretations had come from themselves. I have asked them if the first of all did so, and they have replied that they did not; for, of course, the doctor had to show them the working of the method before they could do anything for themselves, but that all the subsequent ones were their own. Now we know that all good analysts have been analysed themselves either by Freud or by one in apostolic succession to him. We know that Freud sometimes makes suggestions; and that the lesser analysts often do so, and it is therefore not only possible but even likely that no independent interpretation of symbols ever takes place, all the symbols being merely Freud's. The matter is now probably incapable of independent investigation. So many patients have studied the literature that it would be hard to find a subject not already tainted; and where is the observer who does not interfere? I have said that it is possible that when the observer reaches *his* goal he may stop. A study of analytical literature and the questioning of patients who have been analysed show that this is the usual practice. Once the sexual interpretation has been reached, further interest in the matter seems to wane: the patient is no longer "pressed and pressed."

I thought that I had discovered a genuine symbol during the war, and only of late did I begin to suspect it. It was in connexion with those soldiers who were unable to ride in an electric tram although they were able to do so in an omnibus. Free association appeared to show that the noise of a tram resembled that of a distant shell. I suppose I obtained this interpretation in hundreds of cases. I was surprised that I never met a colleague who had worked this out, but was gratified to find when I met again any of those to whom I had mentioned the matter that they agreed that it was a true finding. I was, however, puzzled and annoyed because several healthy officers denied that the sounds of shells and trams were very similar. I believe I found the explanation to all these things last August. There is a collection of battle stories, called "The Green Curve," which I read before the war. In one story entitled "The Kite" this sentence occurs: "A heavy shell rumbles up with the noise of an electric tram." I re-read this story last August and the phrase made me think. It was now certain that I knew already of the possible similarity at the time I thought I was investigating the soldiers. It is probable that I suggested this reason to them to account for their objection to trams. It did not require more than a hint to get them to say they believed anything. Afterwards they could ride comfortably in trams, not

because anything had been worked out, but because they had undoubtedly been led to believe that after the explanation was found they would be able to do so. They were further benefited because, after shells had been brought into the conversation, we could easily proceed to discuss their coward complex, which certainly was never far from their consciousness, and get them to see that they had after all been quite brave soldiers.

We repeat that free association is an instrument very useful for discovering what is in a patient's conscious mind, but doubtful for discovering unconscious thoughts, and difficult to use.

When we consider the above arguments we are driven to conclude that the existence of repression cannot be deduced from observations made on other people. I have read a good deal of the Freudian literature, but I have not found clear proof there. Freud's own dreams form perhaps the most valuable part of that literature; they are courageous and honest, but I do not think the associations he has given were necessarily from the unconscious. I should like to offer an absolute proof of repression afforded by a dream of my own, a proof which came not by way of free association but by an accident. It may seem waste of time for me to begin proving what probably none of you doubt, but the terms "repression" and the "unconscious" have been employed so loosely that it is probably worth while to do so.

In the winter of 1917-18 I dreamt that I was in a railway station; there was a half-empty train, on to which we were not allowed because there had been an accident in the tunnel. I offered two shillings to a fat-faced boy in the crowd to get what I wanted, but it was no use.

At the time I was stationed at Tooting, and on the afternoon before the dream, had gone over to Golder's Green to see a friend. On my way back, after I had taken my ticket I found I had only two shillings in my pocket, and thought how foolish it was to be so far from home with so little money; at the Elephant and Castle Station where I changed trains there was a half-empty train on which we were not allowed; a day or two before I had failed to keep an appointment because of a stoppage on the Tubes. There were most of the ingredients of the dream, the two shillings, the half empty train, the accident in the tunnel; but not the fat-faced boy. He was quite the clearest thing in the dream; I recognized his face as very familiar but I could not place him. The day after the dream was Sunday, and I had a good deal of time on my hands, and again and again I tried to make associations from the boy, but found nothing. On the Monday I was busy and thought no more about him. On the Tuesday I met him in the Wards, and immediately the meaning of the dream became plain. He was a patient with whom I had had a lot to do. On the Saturday before I went out, I had had an interview with him in which I had lost my temper and been very rude. He turned to me and said with a very sweet smile, "You know, Sir, I am little more than a child." This had made me ashamed of myself and I had solved the problem promptly by extremely thorough repression. There are some other interesting points in this dream. I had given him two shillings, which was all I possessed, to get things right; and was it not like a Scotsman to say that he was offering his total possessions at a time when two shillings was temporarily his all, and to expect salvation at that price? No wonder that the dream said it was no use! But these considerations are not immediately important: the point to be emphasized now is the total suppression of the recognition of the very excellent photograph which the dream showed to me. This example indicates also that active forgetting of this kind has nothing to do with interest.

Once I had remembered I knew that the matter had interested me very much. It shows also the speed with which repression may take place. It may have happened almost at once, for I do not think that I remembered anything about it after leaving the hospital—a thing I did immediately after the interview. Now, if I confess that had this story been told to me by a patient I should have doubted whether there had been true repression, I am probably giving myself over to the enemy. It will be said that scepticism of this kind is beyond reason, and that no one could hope to convince me of anything, and that I am a mere solipsist. And yet I do think that such a position is justifiable. First, because the instances of repression given above by Freud and Jung are such striking examples of loose thought that each of us is compelled to seek that rigidity of proof which we can obtain only in ourselves; secondly, because I believe that patients would prefer to think that any given distressing thought had been dwelling in their unconscious rather than their conscious mind. And many of their thoughts are so exceedingly distressing that it would not take long for them to believe what they wanted to believe. They would rather think that they were in their unconscious mind because there is a feeling that we are less responsible for thoughts there than for thoughts in consciousness, and so there would be less loss of self respect if the former were true. I should therefore suspect them of agreeing too readily that their unpleasant thoughts had been unconscious. We constantly see what a very convenient arrangement in Adler's sense the unconscious is.

(2) Is repression the important thing in psychotherapy? We may note first that repression is commonly a very beneficial thing. It would, as Rivers has shown, be very inconvenient to go about with infantile memories; it would be useless and even harmful for the butterfly to remember the actions useful to the caterpillar. It was certainly inconvenient for me to continue remembering my shame about that boy, and it was very bad for him. He was not an innocent child at all, but when I recalled that incident I spoiled him very much for weeks, and then had to subject him to severe discipline. We should not therefore jump at the idea that it is necessarily beneficial to revive repressed memories. It may be a bad thing. I do not say it is always so, but it certainly may be. If these sexual symbols which are constantly being found in uncured analysed patients are true, their appearance in consciousness has done nothing but harm; the patients are more miserable knowing them consciously than they were before; and although patients are often improved by treatment which rests on the method of free association, that is not necessarily because some hitherto unconscious material has been brought into consciousness, but because something quite different has happened. There are many sets of facts which patients have in consciousness, or at least in the preconscious, which they have never correlated, never had in the focus of attention at the same time, which they have looked at from an unhelpful angle, which have therefore been troublesome, and which, by readjustment, can be made unhelpful. The patients, to use Dr. Rivers' word, have been "unwitting" about them but never unconscious of them. It will, I think, be found that these, and not unconscious thoughts, are the commonly important factors in psychotherapy. As the method of free association is a very good one for getting present troubles talked about, and as their readjustment is always helpful, there is no objection to its use provided the observer never at any time gives the slightest assistance, except to ask the patient to proceed; provided in short that he never by word or hint gives a single suggestion. Thus in a patient whom I subjected to an analysis lately I found a

parent complex, the discussion of which led to considerable benefit. He had always had trouble with superiors and was not getting on with his wife because she was not religious enough. He was religious and she did not sympathize with his religion; but he had had assuring experience; for example he had more than once prayed that certain sums of money which were due to him, but which he could not get sent to him, should be sent, and they were sent very promptly. After many sittings, in which the method of free association was used, he saw that this God in whom he was believing was only the surrogate of the father who used to give him pocket money, and one quite unworthy to be worshipped. He saw that his wife was right in not joining him in this worship, and he saw also that his objection to her on this particular ground was a partial rationalization; that while he did resent her not joining him here, he also found her lacking in another direction, viz., that she did not give him enough maternal petting when he was depressed after some unfortunate episode with his superiors. He saw that his difficulties with his superiors were the boy's revolt against the father who did not give him enough good things. In a sense this is the *Œdipus* position. But we never reached that position in the sense of his ever having been sexually in love with his mother. Had he been supplied with sexual symbols, and his dreams were quite full of guns, pistols, and knives in the back, he might in time have arrived at that idea; and the revolt against the father, as well as the constant need to remind him of his pocket money obligations, might have been attributed to sex jealousy. But as we never came to that position he was content with the idea that what had happened was really that he had not fully grown up, and that it is common for the young to wish to put their fathers right. It was interesting that we did not get sexual interpretations to these symbols; he had about seventy hours of analysis and we actually did not get one! It is the more interesting because he was fully prepared for a sexual explanation of his neurosis. His history was full of sex, and at the first interview he expressed the belief that he was ill because he had masturbated. He told me a great many of his irregularities, and so there was no difficulty in getting on to the subject; it was not one which we shirked. When we had cleared the history and got on to analysis it faded from the case.

Now about these findings; he accepted them all with pain. They were new ways of looking at things, but none of them could be said to have come from an unconscious below a barrier. The fact that his wife did not sympathize enough was a thing of which he was perfectly aware, but he preferred to focus on her want of religion; that put him in the right, and her in the wrong; but he had never lost the sense of her imperfect understanding of him. Having got hold of her irreligion he could put her incompatibility down to that deficiency rather than to his own childishness. He had only been unwitting of the real significance of her lack of sympathy, never unconscious of it, if we are to keep to the meaning of the unconscious as something below a barrier. With regard to the new ways of looking at things in general, it might be said that he was certainly unconscious of them; quite so, just as he was unconscious of the Chinese language. They had never been in him at all, and therefore they had never been in his unconscious mind. They were new ideas which arose from the discussion of facts which had been in consciousness, but which were now faced from another angle. It is necessary to say this rather obvious thing, because there is a tendency at present to suggest that a person's being unconscious of a thing is the same thing as to say that that thing is in the person's unconscious; whereas of course it may or may not be the same.

Before the war my great objection to repression hunting was that it consumed much time and was painful; in addition to the above objections I must add that the spread of the practice leads to the overlooking of obvious matters of importance, the investigation of which would help the patient.

Lately I have seen one patient who had been analysed for two years when his trouble was duodenal ulcer, and another, one of whose main symptoms was asthenopia, who had spent £600 on analysis but had never had her eyes examined. If we are to countenance the lay analyst we must expect plenty of that sort of thing; but it was strange to meet an officer, who had been analysed for some years, and be the first person to elicit from him in our earliest conversation the statement that he feared he had been a coward in the war; the discussion of this statement led to his being able to resume work very quickly.

An immense amount of this sort of thing is going on. The lady with asthenopia to whom I have referred lives in her father's house with her step-mother, who was their housekeeper, and whom she cannot bear. Of what use is it to her to know that a bicycle is an obvious sexual symbol if she is going to continue living in that house? And yet in return for the £600 no practical steps were taken to get her to live elsewhere.

A young lady with well-to-do inebriate parents was trying to pursue a certain course of study and naturally with the parental interruptions found it difficult. A short course of psycho-analysis was followed by severe illness, but removal from home has permitted her to resume her studies and continue at them easily.

It is needless to multiply instances; but it would be well if patients were helped in a multitude of minor ways before this very serious business of Freudian analysis were embarked on; the light-hearted fashion in which it is employed at present makes one anxious for the whole future of psycho-therapy.

Repression then undoubtedly exists, but it is not the most important thing in the investigation of psycho-neurotic persons. A great deal that passes for repressed material never was repressed but was either merely out of the focus of attention, or put into the patient's mind by the analyst. The great stress laid on the unconscious has tended to do harm in two directions: (1) In making people conscious of many images which they would be better without; (2) in causing many things to be overlooked which might effect cure. The old rule that treatment should be safe, pleasant, and speedy has fallen on evil days, and I think if we would turn our minds to it a little more earnestly our patients would not be the losers.

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Section of Psychiatry.

President — Dr. BEDFORD PIERCE.

The Ideal Clinic for the Treatment of Nervous and Borderland Cases.

By HELEN BOYLE, M.D.

MY title, "The Ideal Clinic," is itself, perhaps, not ideal, for it needs a word of explanation. Our knowledge is now increasing very rapidly in all matters concerning mental and nervous health, and I use ideal in the sense only that this paper gives my own idea of what is best in the present state of our knowledge.

Ideals, except among the insane with fixed delusions, are modifiable pictures. This ideal, I trust, will be altered and enriched by the discussion to follow if all the minds present work on it and give us the result.

I have been asked to read this paper because since 1905 I have been senior physician to the first venture of the kind in this country. It is known as the Lady Chichester Hospital, and was started in 1905; in that year I read a paper on it before the Medico-Psychological Association, and received much encouragement from those who heard it.

I saw some of the best work done on these lines before the war, on the Continent, and, since the war, in America. I was also a member of the sub-Committee of the Medico-Psychological Association appointed in January, 1918, to "consider the amendment of the existing Lunacy Laws." This committee went into the whole matter of early treatment. It is the foregoing experience that has led me to formulate my idea of what a clinic should be.

It is a pleasure to acknowledge that in 1904, the year before the Lady Chichester Hospital began its work, that true pioneer, Dr. Carswell, of Glasgow, started wards in connexion with the workhouse for the same class of case. It will, however, be admitted that splendid as is the work which these wards have done, this kind of work should no more be carried out under the Poor Law with the inevitable stigma attaching to it, than should ordinary hospital work.

Two main questions face us: First, what are the aims of the clinic? Secondly, how are these to be carried out?

(1) The aims, put briefly, are:—

(a) To provide facilities for efficient treatment for early mental and early nervous disorders. These terms are used as more comprehensive and more easily understood than such terms as neuroses, psychoneuroses and early psychoses, and by treatment is meant all the steps necessary to enable those coming to the clinic to be restored to perfect health or to as perfect a state as they can attain.

(b) To assist in maintaining such health by instructing both the patients and their families in mental hygiene as far as possible.

(c) To encourage and carry out research into the origin, course and results of these diseases and their treatment and also their bearing upon and relation to other sides of life.

(d) To afford opportunities for study to students of medicine, of social service, of nursing and other kindred subjects, and to any workers who desire them.

The aims summarized merely mean to apply to these illnesses the same intelligent effort for their care, consideration and understanding, which are given as a matter of course to other diseases. They mean that for students and others to learn about such conditions, these conditions must be there to be studied with all the equipment and environment necessary for the study. It is obvious that the neuroses, psychoneuroses and psychoses are amongst the most difficult and important of all the ills of the flesh. Are they not indeed the most important of all? Is it not the nerves and mental possibilities of a race which determine inevitably its power, its quality, its survival?

(2) How are these aims to be met?

Is it necessary for the clinic to have beds, or will an out-patient department suffice?

I have heard it said and have heard the statement warmly defended that beds are either not needed at all or only needed in very small numbers; on the other hand, the necessity for beds is recognized in all the long-established clinics, both in Germany and in America.

It will probably be found that those who advocate an out-patient department only, have gained their experience mainly during the war, and while dealing with the results of the war. The nervous troubles of war, though not in their nature radically different from those of peace, are peculiar in that they have usually arisen more or less apart from the ordinary life and home surroundings of the patient and are therefore more readily treated in the sufferer's home. Even keen supporters of the out-patient method admit that there are patients who live far away, for whom travelling is undesirable or impossible, and who will need accommodation.

Most observers will agree that the surroundings in which some patients live are destructive of even the best psychotherapy, in which, indeed, psychotherapy to be successful, would have to be applied to a whole neighbourhood as well as to the individual. Moreover, no one can doubt that where the physical surroundings are harmful, where, for example, there is excessive quiet or lack of quiet, or lack of air or food, it is well to have the power to remove the patient into hospital. This removal is necessary for the same type of reason that exists with regard to other diseases. In pneumonia, for instance, some cases are sent into hospital and others are treated at home. The environment in which the patient fell ill is often not the most suitable for recovery, and the mental environment is often less easily modifiable than the physical. It is easy to open windows and disinfect a room. It is harder to open windows in the minds of the family and to disinfect their thoughts of pernicious ideas.

Beds are also necessary for the various contributory physical treatments needed, such as massage, rest cures, electricity, colon lavage, steam baths, spinal douches. These are all better carried out in hospital. Surely it is not proposed to "scrap" all these forms of treatment! They have proved of real worth and cannot, so far at least, be discredited except when advocated as wholesale nostrums. Besides, it is possible that illnesses which are now

regarded as purely physical may come to be looked upon more and more as of psychic origin, or, at least, as having a strong psychic element. Among such illnesses are those connected with the endocrine glands. These, too, may need psychotherapy and to be transferred at times to this department. It is apparent, too, that for perfect study it is often necessary to have the patient under observation. Psychotherapy involving two or three years of work would be conceivably shortened if the patient were under supervision.

In no other disease is observation so valuable and helpful, for in no other are there so few physical signs to guide the observer, and in no other is so much indicated by the behaviour of the patient; take, for instance, the various forms of fits. For these and other reasons I urge a liberal allowance of beds as well as an out-patient department.

Where should the clinic be? Should it be in the town or in the country?

In the town it can be very closely associated with, or preferably become a part of, one of the big teaching hospitals. Both can benefit so enormously by this association that on this count alone it would be possible to decide upon the town. The clinic can share the advantages of the laboratories of the hospital, and of all its special departments. It can easily obtain and afford special consultations. It can share all the best brains and experience of a general hospital. I said a *general* hospital, but indeed I cannot imagine how the authorities dare arrogate the term "general" to such a hospital when they omit entirely the provision for treatment of the diseases of the chief part of man, namely, his mind. The urgent necessity for having a clinic on the spot for teaching purposes and the stimulus to the clinic that the presence of the students brings are both weighty reasons for its establishment in the town.

On the other side, it may be urged that the soothing quiet of the country, the healing effect of nature, the availability of country pursuits, and the fine air are very important. True, but these advantages are, I believe, more imaginary than real, and are as easily dispensed with by nervous patients as by others, possibly even more easily, for the physical drawbacks of the nervous are less marked than those of other patients. Nervous patients, too, often stand the country badly; it bores and irritates them. As one said to me: "The same old tree in the same old place every day is exasperating." The streets and the shops, on the other hand, how various they are; never two days alike! Again, how arresting are the demands of the traffic! The most introverted patient must to some extent attend to the external stimuli. Is it not true that you must take something to the country to get value back? And these bankrupt souls have nothing to give and are thrown inwards on themselves by the self-sufficiency of undiluted nature.

Also with maladjusted people, is it not usually the herd to which they are maladjusted? Would it not therefore be likely that they would learn their readjustment better in contact with the herd? The consciousness of their being one of the many, which is forced upon them in the town, is beneficial. It compels attention, where a sunset would not.

It has been suggested that the out-patient department should be connected with a general hospital, while the in-patient department should be in the country. This appears quite undesirable. There would be lack of unity in the working and a want of elasticity in the organization. For instance, some out-patients should be allowed to sleep at home and be at the hospital all day. Others should attend for massage or hydrotherapy, re-education and so forth. This valuable interaction of the two departments would be impossible if they were widely separated.

What kind of building would be best ?

It should consist of an administrative block with observation wards on each side for men and women respectively, and association rooms for recreation, smoking, dining, gymnastics, handwork, &c. As well as these there should be small wards for four, five or six patients. I think there should not be less than four patients in a ward, for two may fight or be unwisely sympathetic; three are company, and two may side against the third; but four, five or six are satisfactory, and this companionship is preferable to the solitude of single rooms. Where advisable, screens should be available to make a little cubicle for any patient, but we have found that they usually disappear in a few days. Single rooms should be used for treatment purposes only; provided there are enough bath-rooms and screens, they will hardly ever be found necessary, any more than they are in treating other diseases.

Where possible, the villa system, using for this purpose several ordinary small houses in conjunction with the administrative block, would probably work the best, with a "mother" at the head of each house, as at Dr. Barnardo's Cottage Homes. This would allow of grading, and of useful change of environment and personnel for treatment purposes. I do not know if this has been tried. The patients would meet for recreation, work, and some treatment. Where this is impracticable, small wards answer very well.

What class of patient would be eligible ?

Men, women and children, especially children, who are suffering from early recoverable or improvable mental and nervous disorders, and who are willing to co-operate in the treatment. As Dr. Henderson, of the Phipps Clinic, says: "The line of division lies chiefly between co-operative and non-co-operative states, and not between any supposed standards of sanity or insanity."

By "nervous" are meant neurological as well as psychotic cases. These nervous and psychotic patients are inextricably mixed in practice, and the present attempts to disentangle them are demonstrably absurd. The admission of neurological cases will be immensely useful in getting the early cases of the psychoses to go to the clinic.

Where there is anything like an adequate provision of such clinics, the patient will in course of time be admitted in the same way as are patients suffering from other illnesses. They will present themselves in the out-patient department and be sent in according to their needs, or, on the recommendation of their doctor, be admitted directly to the wards.

Another useful purpose which the clinics serve in America and Canada is as a place to which children and other persons who get into the hands of the law can be referred in suitable cases for an opinion as to their mental and nervous state. This is very sorely needed here.

The chief question for debate with regard to the eligibility of the patient is as to whether any should be compulsorily detained or not. *Shall there be any locked doors and interference with personal liberty?* There are many excellent arguments on both sides, but I may direct your attention to the findings of the Medico-Psychological Association Committee, which after much thought and discussion came to the conclusion that to seek legal powers for compulsory detention would be harmful to the best interests of the clinic. The same view is held in America.

The chief reasons against compulsory detention are:—

(1) It imports fear into the minds of the patients: (a) Fear that the clinic is in the nature of an asylum, and that they will not be able to get out—a real

terror to the early nervous patient. (b) Fear of the other patients, for looks suggest danger, untrustworthiness and irresponsibility, and the possibility that the patients may harm themselves or others if they leave. (c) Fear that the clinic is run on such lines that sensible people would not wish to stay in it. All fear is hampering to successful psychotherapy, and psychotherapy will form the chief basis of the treatment.

(2) It introduces an element foreign to the treatment of other diseases, which is undesirable. It suggests force and mystery. It prevents the profession and the public from regarding these illnesses in their early stages as no more occult than others, no more to be hidden and spoken of with bated breath than aneurysm or tuberculosis, attaching to both of which diseases there is a possible stigma. If there be one acute case of mania admitted and detained, you may be sure that it will be known throughout the building, and also throughout the neighbourhood whence it comes. This knowledge will deter the early case from coming.

(3) It would necessitate some degree of outside supervision to which it would be difficult to get many general hospitals to agree.

(4) It is damaging to the tone both of staff and patients, and to their attitudes towards each other. Without it co-operation is more rapidly attained. It becomes an interest, almost a sport, for the staff to see which one of them can reassure and placate a newly arrived nervous and difficult patient; and the most helpful remark to make to the patient is this: "we do not wish you to stay if you want to go. Write and ask your people to come for you to-morrow." I have often been asked what happens if a patient who is depressed or a drug-taker says that she wishes to leave at once. It rarely happens, but in that case a nurse, or even two if required, would go with the patient to her home.

(5) It soon leads to confusion of the clinic with a mental hospital, and then the very early most hopeful cases of obsessions, insomnias, maladjustments, mental difficulties, phobias and so on, will not come to the clinic at all. These are the cases which, treated at once, will reduce the amount of mental and nervous ill-health in the country, diminish the need for mental hospitals, restore the workers, and be of incalculable use to some of the best types in the community. I believed that compulsory detention would lead to this confusion, and I found it proved so in America. There I was informed by doctors that the practice of locked doors and the admission to the psychopathic clinics of acute manias—or rather let us call them "disturbed patients" in the kindly American way—as well as other certifiable cases, was militating seriously against their usefulness for very early cases. This is the most powerful reason of all, for it is now certain that the treatment of these cases, to be used rightly, should begin long before certification is possible. Do not let us sacrifice these many early cases to a few excited and certifiable patients, more especially now, when with wise economy in view, it is cure of the early case which will keep the worker as a worker, and prevent his becoming a burden on the country.

(6) It is better for the student to be *obliged* to visit the mental hospital and to see acute, chronic, and plenty of well-marked cases. I know that on the other side it is urged, not without truth, that for teaching purposes it is of value to have every stage of the disease readily available. But it will surely not be possible to keep all the cases a student should see in sufficient numbers in a clinic. If this is so, why not make the mental hospital as perfect as possible so that there shall be no reluctance in sending to it all the cases requiring detention?

(7) For the sake of mental hospitals and the treatment of the patients in them, it is desirable that there shall be no compulsory detention in the clinic.

I know it is said that some detained cases may be needed for exhaustive research, but why refuse to the mental hospital the power, equipment, and personnel necessary for this? Moreover, it would tend, as the clinics multiply, to remove from the mental hospitals all recoverable cases, which surely would be a mistake. These hospitals should be, and in many cases are, capable of dealing with all patients who need to be compulsorily detained, no matter how early and recoverable they may be, nor how modern the methods of treatment needed. It would lead to a heartless and hopeless state of things in mental hospitals if all supposed recoverable cases were taken from them and they were then depleted of the best members of their staff who would naturally not care to stay. Besides this we have not yet sufficient knowledge to recognize curability with any degree of certainty. I cannot imagine anything more deadening to the personnel than to have the supply of recoverable patients cut off or anything which would damage more effectually the morale of the mental hospital, or which would lead to a more than ever complete misunderstanding of its chief function on the part of the public. It would greatly enhance the much-to-be-deprecated stigma of the asylum. All admit there must be places for detention—why not let them be the mental hospitals with the necessary supervision of the Board of Control? It is much to be desired that in the future all mental hospitals should be allowed to admit readily voluntary boarders, increase greatly the parole system and be enabled to board out suitable patients. This latter method I saw efficiently and successfully used in Germany. In America a large percentage of patients are voluntary. There are not a few who feel protected and comforted by locked doors. There are others who, knowing the personnel of a mental hospital, may desire to place themselves in it, and others again who, having already been treated there successfully, desire to return.

What equipment would be necessary?

This question is closely bound up with that of treatment. The clinic should be able to have every known and approved method of cure. It is most important that it should not be narrowed in its scope for treatment, and by treatment I mean a very wide range of modification of environment as well as modification of the personality by psychotherapy. Perhaps the tendency today, in the light of the many triumphs of psychotherapy, is to lay stress upon this type of personality treatment to the underestimating of that through environment; and yet the wonderful effect exerted by environment is brought out more than ever by that same psychotherapy in tracing environmental effect upon the personality from the very earliest life. By environment I refer not only to physical but to mental and moral influences—to books and people as well as to light and warmth—to extrospection and external stimuli in contradistinction to introspection and analytic work. It is therefore in keeping with the most modern finding to utilize to the full all that surroundings, including personnel, can do to aid the nervous and mental patient. Environment has been shown to exert so much productive effect in the causation of mental disability that surely it can be used, too, in the reduction of the condition. It is in this needed power over environment that it will be found that the equipment and discipline and routine of the clinic will differ most from that of the ordinary hospital.

In addition to the well-known hydrotherapy, steam baths, alternating

spinal douches, prolonged baths, massage, electricity, Nauheim and Swedish treatments, there should be occupational therapy both for neurological cases and for purposes of sublimation in the psychotic. Sublimation is used in the psycho-analytic sense of a safety valve for emotional strain, a method whereby expression is allowed to emotion which is otherwise redundant, but which, when harnessed to some activity produces a desirable result. This is a very important department, for without it some forms of psychotherapy are accompanied by more risk than there need be. It is possible that in some cases it should precede the more purely mental treatment. This occupational therapy should be limited only by possibility. The Walter Reid Hospital, in America, a splendid war hospital, offered an extreme example of this by even going so far as to enable a man to learn embalming as he wished to do so! All kinds of handwork, more especially those allowing scope for originality, also carpentering, gardening, if possible, painting and writing—should be encouraged. A magazine to which any one who wishes to do so may contribute is produced at the Lady Chichester Hospital, and a very useful means of expression it is. Recently there was an amusing poem in it on "Washing up," a hated handwork. Later "washing up" done to its strains became a joke! It should be possible, if required, to allow a patient to start work for whole or part time whilst still in hospital. Games are invaluable, and even in a town room can be found for tennis or badminton on the roof or elsewhere. Various clinics could play against each other. The Lady Chichester Hospital cricket eleven played my house and beat us. We challenged them at tennis and beat them. I have known a patient with functional chorea be amazingly improved by the mental, moral and physical warmth engendered by making runs for her side. Again, patients should be able, when it is good for them, to go out either alone or with others, with friends or with a nurse, to attend concerts, cinemas, shops, to take omnibus rides, have tea out, and so on. The joy of the penny bazaar has weaned many patients from grisly memories and has aided others of the undecided type to screw themselves up to a decision.

Re-education requires all these helps as well as the more stereotyped training, and the one thing that can be spared altogether is red tape. No particle of red tape will be needed. The curative treatment of the human mind must be as various and subtle as the human mind itself, and the hospital in which those responsible maintain a real pliability and alertness to all needs will have the most successful clinic. There is a homely adage that "a man's stomach likes to be surprised," and it is not only of his stomach that this is true. The unexpected is a potent force with many nervous patients. The clinic should aim at supplying as far as possible every form of mental, moral and physical environment as treatment, or, as Adolf Meyer terms it, "melioristic handling." Amongst these influences religion is most important. There should be a chapel, simple, but beautiful, and non-sectarian, with a very few carefully selected books, or, if required, the doctor may prescribe a book for any individual patient. Provision should be made for ministers of different views to help members of their own flocks.

Another important activity, called "club night," should be part of the work of the clinic. It means that once a week any old patient who wishes can come to the clinic, have tea and a bun and share in or contribute to some form of entertainment. Among our entertainments are dancing, whist drives, lectures, concerts, sometimes entirely provided by the patients themselves. This club night has solid value. It helps those who have left and who may feel rather forlorn at starting on their life work. It enables them to keep in touch with

those who can give them a hand if they need encouragement and who can urge them to hold on to their readjustment. It makes them feel that they can help those who are struggling out of the slough in which they themselves were recently wallowing. Often the remark is heard: "Oh, don't you worry, I was worse than you; and look at me now." It cheers and encourages recently-arrived patients. Sometimes, too, the families of patients are asked to hospital entertainments. It is a useful way of getting into touch with them. To help with all these activities it is good to enlist some voluntary workers.

What sort of staff is needed and how should it be chosen?

To take the medical staff first. When the clinic is in connexion with the general hospital, it would be chosen in the same way as the rest of the medical staff. All the reasons which can be urged in favour of part-time work in general hospitals can with equal justice apply to the staff of the clinic. There is a tendency in whole-time posts for the holders to get into ruts. A rut is occasionally useful and steadying, but in our particular line it has exceptionally dangerous qualities. There is no branch of medicine in which a wider outlook, a more pliable, responsive, and intellectually agile mind is needed, or in which a tolerant knowledge of the world of men and things is more essential. This is not best obtained in a rut, however fine and straight it may be. Therefore I would urge that the clinic be served by part-time medical men and women aided by house physicians.

What qualifications should we hope to get in these part-time officers?

(1) They should have real working experience of the insane and of a mental hospital. There is no other disease of which those engaged in its early treatment would say: "I need not have any experience of the illness in its later stages. I propose only to deal with its beginnings."

(2) They must have put in some time at work on neurological lines. Here again would it not be an anomaly if in undertaking to specialize in the diseases of any systems, circulatory, respiratory or digestive, some of the diseases of that system were to be omitted? Moreover, in practice, it will be found impossible to exclude neurological cases even if desired. The whole attitude towards the problem of mental disease will be usefully modified by this requirement. It may be that with further knowledge all mental diseases may be found to be organic in the sense that a physical basis for each of them may be demonstrated. Indeed there are indications of this in the very definite relation between environment from birth—and before birth—and the forms of mental troubles and their origins. It may be argued that it will be difficult to find suitable people to staff these clinics, but is it not probable that with the intense interest aroused by psychotherapy, more young practitioners will turn their attention to this line of medicine? The course and diploma now provided by various universities are a hopeful sign. It is probable that both out-patients and in-patients will have to be fewer in number in relation to the medical staff than is the case in other diseases. Also that, as in surgery, irregularity in the time spent will be the rule. The psychiatrist may have a long and serious mental operation on one day and may have to visit the case next day.

What considerations should guide the choice of the nursing staff?

One of the main points is to have as varied a staff as possible. Some should hold the Medico-Psychological certificate, others should be masseuses,

some trained general nurses, others teachers of handwork, and there should be some young probationers.

I look forward to the time when we shall prescribe people as we now do drugs—an arresting, vital personality for one hour say, and a placid one to soothe and rest for two, so providing for that adaptability of mental climate which may be needed. As a matter of organization, too, great elasticity must be allowed in the distribution of the nursing staff, in which I include the handwork and other teachers. If a patient should need almost constant attention it would be wise not to give it at the hands of one nurse only. There are few people, except personal friends, that any one of us would like to be with all day. Think of being soothed all day long by one person, or stimulated all day by another! Even a stable, thoroughly psycho-analysed angel would find it trying!

What sort of a matron is needed?

I am tempted to call this the most important appointment of all. She should, it seems to me, as should all the nursing staff, be chosen by a committee of which at least half are women. I have known two ideal matrons, and noteworthy amongst their characteristics are a love of children, a keen sense of humour and a strong religious sense; both are unmarried.

The domestic staff is almost conspicuous by its absence. At the Lady Chichester Hospital, with thirty-five patients, we have a cook, a housemaid who is an ex-patient, who attends to the doctors' and matron's rooms, and our one chronic patient—a mildly defective girl—who does kitchenmaid's work. The rest is done by the patients according as it is good for them—helped and supervised by the nurses. We try to evoke the feeling that all would like to lend a hand where they can. The same democratic spirit should prevail in the clinic as in the general hospitals, namely, the idea that patients should be admitted quite irrespective of social rank and station, necessity and illness being the qualifications. It has been said that where the patients are up and about and have meals together, this would never succeed. But, probably largely owing to the fact and fine spirit of the matron, there has been with us no appreciable difficulty about this.

With regard to finance: The clinic is an *economy*, the very best one of all. Think of the millions of pounds that have been spent on asylums. The earlier you get the cases and the more understanding there is of the early stages by our profession and by the public, the more surely can we be certain of diminishing the supply of those institutions now containing thousands of patients. No one who practises any psychotherapy can doubt that, in the case of patients in whom all sorts of weird compensations are effected for a sense of inferiority, in whom a struggle is maintained with pluck often for years against overwhelming odds, in surroundings or in occupations which are entirely unsuitable, had there been an earlier and better adaptation the breakdown need never have occurred. It is for us to see that this wastage of good work and this extravagance of preventable illness ceases. For this the clinic at which the very earliest maladjustments can be dealt with is essential.

The late Sir Robert Morant, with his brilliant, eager brain, had the utmost faith in the true economy of this step, and had he lived was determined to prove his faith. Whether the money is obtained in the same way as that of voluntary hospitals, or by help such as is given to help venereal disease work

by Government, in either case patients should be asked to pay according to their means. This is no hardship but equitable and right, always provided that no one be excluded because he lacks money. Money is the poorest qualification possible for admission to a voluntary hospital. It is also important that the staff should not know what the patients pay, and that the treatment given should not bear any relation whatever to the payment.

I need say nothing about food and many other necessary but understood factors. There is, however, another side which is very important, to which great attention is paid in America, and that is the field work of the Social Service Worker.

To every clinic there should be attached Social Service Workers charged with the duty of collecting the data relating to the patient. In America they are definitely trained for it, and this is important. It is apparent that it is often difficult to get a true estimate from a nervous patient. It may be even necessary to check their statements as in cases of phantasy-building. Contrary to what might be supposed these workers are usually popular, and we see an indication of the same thing in this country in the success of the visitors of the Central Association for the care of the Mentally Defective. It might be possible that the C.A.M.D. workers already scattered far and wide in the country might be induced to take up this necessary work in connexion with the clinics, some of which they are already doing.

In conclusion: You may feel that some of these suggestions are vague and impracticable, that to ask for nervous clinics in connexion with any general hospitals is Utopian, and that to feel optimistic about largely reducing the amount of insanity, increasing the capacity of the workers and contributing to the longevity and competence of our race, is impossible. I would like here to quote Dr. T. Hadfield, who in an article on the "Psychology of Power" says: "Want of belief in its possibility is always the main obstacle to the performance of any mighty work."

DISCUSSION.

Dr. POTTS said he fully agreed with most of the views expressed in Dr. Boyle's paper, but he differed from her as regards the proposal to place the ideal clinic in a general hospital. That was the worst place for it, because, with a few brilliant exceptions, the medical and surgical staff of the general hospitals understood the new psychology so little that promotion of its principles would have a poor chance among them. Judging from the expressions of opinion of many such men he knew that they looked upon psychotherapy as a form of treatment which could be carried out in an ordinary out-patient department, one man dealing with several patients during an ordinary session. The atmosphere of the general hospital was charged with the materialistic or physical conception of all forms of disease. Such stress was laid upon this idea during the ordinary curriculum that it was difficult to get the student or young graduate to see there was also a psychological side. The only way to induce him to appreciate this was to divert his attention from the advanced anatomy and physiology of the central nervous system, from section cutting and staining, and to focus his attention for a time on the other point of view. Psychotherapy while comparatively undeveloped had not a fair chance if practised by the side of physical treatment.

Dr. CRICHTON MILLER said he did not think that anyone who had had experience in these matters would differ from Dr. Boyle's main recommendations. He felt, however, that she had avoided the real difficulty of the problem—namely, that psychotherapeutic work must always stand in a class by itself, because it demanded an amount of time from a skilled physician wholly out of proportion to that required in

any other form of treatment. For this reason he (Dr. Crichton Miller) felt a little doubtful about the future of the psychotherapeutic clinics at general hospitals: the boards of governors, medical committees, and so on, would tend to be critical of the very small output of "cures," as compared with the amount of physicians' time expended. Furthermore, it would be necessary to protect the psychotherapeutic clinic from the red tape and officialdom inseparable, and rightly inseparable, from a great institution. Those who had attempted psychotherapeutic work in the Army had realized the blighting effect of bureaucracy in this department.

[The Discussion was adjourned to March 14.]

Section of Psychiatry.

President — Dr. BEDFORD PIERCE.

Adjourned Discussion on Dr. Helen Boyle's Paper, "The Ideal Clinic for the Treatment of Nervous and Borderland Cases."

Dr. BEDFORD PIERCE (President) said he was deeply impressed by Dr. Helen Boyle's paper, and was reluctantly convinced by her arguments. He said "reluctantly" because there were several groups of cases for which no provision could be made under the voluntary scheme the author of the paper had outlined. She had made it clear that if there were any detention at all, the character of the clinic would inevitably be altered. It was really a case of special pleading on Dr. Boyle's part, and he did not think she quite did justice to the other side of the matter. Although he said he felt obliged to agree with her, he would like to put forward one or two difficulties not referred to in the paper.

There was, first, the question as to the use of the term "voluntary patient." Was that term used only for persons who were wishful to come under care and who desired to remain for treatment. Her definition, as he understood it, was very strict—namely, that only those patients who were willing and anxious to co-operate in treatment should be received in the clinics. That would exclude a large number of people of the kind he would like to see there. It would, for instance, necessarily exclude the confused case, many acute cases which would recover in a week or two, also practically all puerperal cases. Therefore, such a clinic would do little to help many forms of the acute psychoses which one would have liked to have seen kept from mental hospitals, and treated to recovery in these clinics. But if Dr. Boyle used the term "voluntary patient" in a loose sense, meaning any persons might be persuaded to go and had not the will to leave, including confused persons who would remain where they were put and who had not sufficient volition to wish to leave, then he thought her clinic would require a considerable alteration in the law before it could be established. The 315th Section of the Act prevented anyone receiving persons of unsound mind for profit—and if the patients paid anything, even a trifling sum for maintenance, he believed that section would be operative—and, therefore, he wondered whether Dr. Boyle was prepared, if she used the term in this loose way, to risk the possibility of prosecution. It would be a sad thing if some of the milder acute cases could not be treated to recovery in a clinic. Recently he had been associated with two homes for voluntary patients and it was extremely vexing when patients had to be removed on account of sudden exacerbation of symptoms to find they recovered in a mental hospital in a week or so. Many years ago he certified a medical brother; there was then no home or clinic, and nothing else could be done for a recent delusional case. He was therefore sent to an institution a long way from his home and when there he recovered in a week. Such a case ought not to be obliged to go to a mental hospital, and should be received temporarily in a clinic. He did not think Dr. Boyle's clinic could do it, because if the person were decidedly certifiable he was probably not a voluntary boarder in the strict sense in which he understood she used that term. There was need for a place

where the medical practitioner who was in difficulty about a case could send it for a short period of observation so as to see whether or not further steps ought to be taken in regard to it.

He also wished to ask about the depressed patients to whom Dr. Boyle did not refer. He gathered from her remarks that there were no unhappy people in her clinic, because she spoke of the cheerful character of the place, and remarked how cheerful and contented everybody was. But psychiatrists knew that the depressed cases outnumbered the other kinds. In the two homes with which he was connected he had a number of depressed patients, and he had found it very difficult to create that bright spirit and atmosphere which seemed to prevail at the Lady Chichester Hospital. What did Dr. Boyle do about those depressed people who changed, one day willingly co-operating, the next showing marked suicidal tendencies? Did she send them away? It militated somewhat against the method if immediately a person developed suicidal tendencies he had to be removed. In eighteen months at the two homes of which he spoke they had had one suicide, one serious attempt, and two or three cases who caused the officers a great deal of anxiety. He assumed there must be a somewhat similar experience at the Lady Chichester Hospital, and he thought she would admit, at any rate, that all the patients were not co-operating in their treatment all the time.

Another difficulty which the opener did not mention lay in the extraordinary kinds of people who were sent to such clinics. A medical man wrote and said a patient he was sending to one of the homes was not suicidal; but inquiry showed he had himself assisted in rescuing the patient from an attempt at self-destruction. The doctor and the relatives appeared to have combined to keep the officers at the home ignorant of the facts of the case. It might be that the doctor thought such tendency had passed, but certainly there was a lack of candour on the point, with the sequel that the matron had great anxiety on account of a particularly determined suicidal attempt.

Dr. Boyle commented on the immense number of people whom these clinics would prevent from becoming insane. On that point he (the speaker) would like to have more information; if she would produce a clinical record of a number of cases it would be more convincing than a general statement. If she could convince psychiatrists that a large number of persons would have developed insanity but for the timely treatment at these clinics, she would have done a great deal towards justifying the great public expense which this system would involve. Neurasthenics and cases of psychoneurosis, in his judgment, did not frequently become insane. Was he now to think that in reassuring patients on this point, as he had done for twenty or more years, he had been quite wrong? He thought not. He fancied that a hospital conducted on the strict lines suggested by Dr. Boyle would not contain many persons suffering from undeveloped psychoses, but, rather, patients of a neurotic type who would not become insane.

There was also the question of expense. In his own view, this would be a very serious matter, and if these clinics contained chiefly psychoneurotic people who were not likely to become a burden on the ratepayers, there would be great difficulty in convincing the public as to their necessity. He hoped it would be possible to record temporary acute cases for observation without of necessity being certified. He remembered the case of a gentleman who was worried over his business affairs, and who, when in great stress of mind, tried to get out of a carriage window. He was certified and brought to the mental hospital. There he recovered in a week, but was persuaded to remain a month properly to establish the recovery. When he left he found he was no longer managing director of his business, as the articles of association stated if a director became insane he, *ipso facto*, ceased to hold the position. A self-seeking colleague had taken full advantage of the clause in the brief time of residence of the patient in the mental hospital. Such hardship might well be prevented if clinics were available for recent acute cases.

Though all he had said had been critical, he wished to repeat that Dr. Boyle had convinced him that in the present state of public opinion, these homes would have, in the first place, to be conducted on a voluntary basis. But he hoped that, in the future, a more enlightened public would make much less fuss about detention; that more importance would be attached to the medical needs of the patient, and that legal

difficulties would recede more into the background. He considered Dr. Boyle's paper a most valuable contribution on the subject.

Dr. GOOD (Oxford) said that on this question it was important to decide as to what a certifiable patient was; how many of those present would agree as to that in any given case? He agreed with the President that one of the great stumbling blocks in the way of instituting an in-patient clinic for mental cases was Section 315 of the Lunacy Act, because the question was, not whether a patient was certified, but whether he was certifiable, and by this last word he confessed he did not know what was meant; it was a matter of personal view as to whether the patient was likely to hurt himself or be a menace or a danger to anybody else. For twenty-seven years he had been engaged in a mental hospital; from 1914 until 1918 he was dealing with war cases, and since 1918 he had dealt with war and civilian cases side by side. At both in-patients and out-patients since 1918 he had treated 355 pensioners at the Oxford Clinic, and only six of those had had to go to a neurasthenic hospital. All the six had some organic trouble. Some few of the 355 had been certified. Every one of the 355 was asked on admission whether he wished to go into hospital, but only those six agreed to do so; 47 per cent. of the 355 were now at full work. He had treated 200 civilian cases, and he had had to recommend three for certification, though he could have certainly certified a good number of them. He had also been "running" a neurasthenic hospital, where he had many cases. He had had many patients who were suicidal, but yet he had no case of attempted suicide at that institution. There was only one locked block, and that was largely in order to show that this block would be resorted to if there were infractions of discipline. His commencement was an out-patient clinic, at first held on one day a week, and single-handed; there were now twenty-two patients, and three medical officers were kept hard at work. Moreover, patients treated at the general hospital did not mind coming to see him later at the mental hospital. This was a matter of the Law, and until the Law was altered he did not think clinics for the in-patient treatment of early mental disease would be established, though he did not see why those interested should not continue on these lines with out-patients.

Dr. MARJORIE FRANKLIN, in the course of an account of methods of dealing with borderland cases in the United States of America, described some of the psychopathic hospitals; institutions which co-operated on the one hand with general hospitals and universities, and on the other with social welfare and State organizations. As an example of a private clinic, she cited that founded and partly endowed by Henry Phipps which formed the most recent of the constituent buildings of the Johns Hopkins Hospital, Baltimore. It was organized on the lines of a unit, directed by a professor (Dr. Adolf Meyer), who was appointed for part-time, and was assisted by a large staff of men and women. The clinic was thoroughly equipped for dealing with the subject by all modern methods from the therapeutic, research, teaching, and sociological aspects. There were beds for about sixty-five patients who paid (up to a maximum) according to their means, treatment and accommodation being arranged regardless of fees, each ward on a different floor, and that for disturbed patients being on the ground floor. Dr. Franklin did not find that patients objected to the admission to the clinic of persons more seriously deranged than themselves, or that their convalescent companions had passed through such a stage, but she considered that some annoyance was caused to the milder type by the locking of the outer door of the ward. This seemed an unnecessary restriction in the upper wards, especially as parole was liberally granted. The majority of patients admitted and on the extensive waiting list were psychoneurotic and borderland, but some cases of major psychosis were included. This combination was, perhaps, fundamental to the school of thought and teaching of Dr. Meyer. The patients were all uncertified, and could leave after three days' notice, which might be verbal. The out-patient department was directed by the Assistant Professor (Dr. Macfie Campbell), assisted by visiting physicians, interns, post-graduate and senior students. The large number of workers helped to overcome the difficulty of giving adequate time to individual cases. The teaching was many-sided, and spread over the three hospital years. Dr. Franklin described, as a type of public clinic, the

State Psychopathic Hospital, Boston, as it was in 1919, but she stated that it had enlarged its scope since then. It was a hospital of 110 beds with out-patient department, laboratories, and library, maintained by the State for the diagnosis and temporary observation of mental disorders and for research, and was then under the direction of the late Professor Southard. Admission was free, and patients usually remained ten days. The admission rate was 11,289 in-patients in six years. Most were under some form of legal commitment—such as the Boston Police Act or various temporary care certificates. A considerable number were sent by State agencies, courts, or industrial schools. These were often “borderland,” or were, perhaps, found to be not mentally disordered or defective. Persons voluntarily seeking admission formed only 17.6 per cent. of the total. Practically all eligible cases who applied were admitted immediately by day or night. Like Baltimore, this hospital trained and employed social service workers, whose assistance was of great value. The institution was so organized that the administrative and clinical departments were managed separately, although both were under medical control. A large staff was employed, and the medical men and women attended patients of both sexes. A special branch treated syphilis.

The State Psychopathic Hospital (under Professor Barnett), which was part of the General Hospital of the State University of Michigan, at Ann Arbor, was considered as intermediary between the two previously described. It was managed by a Board of Trustees drawn in equal numbers from the Board of Regents of the University, and the Trustees of the State Hospital for the Insane. The patients stayed on an average, three months, and the medical officers were not obliged to admit all applicants; 53 per cent. were voluntary; 22 per cent. of all admissions were transferred to State hospitals for the insane, and a few from State hospitals were sent to the clinic for special intensive treatment. The clinic conducted out-patients department locally, and at the larger town of Detroit. The pathological departments (under Professor Adela Gurd) served the whole State.

Dr. Franklin mentioned that some general hospitals, without establishing psychopathic departments, had set aside wards or beds for mental patients. She did not consider these very satisfactory.

In conclusion, she alluded to private sanatoria for functional nervous disorders, work with ex-service patients, voluntary boarders at State hospitals (legalized in about twenty-nine States); mental hygiene clinics in connexion with hospitals, clubs, settlements, industrial organizations, schools, &c.; work with juvenile and adult delinquents. The National Committee for Mental Hygiene she considered invaluable in initiating and co-ordinating work with mental disease and defect in all stages, and for the preservation of mental health.

Dr. EDWARD MAPOTHER gave a brief account of a number of the clinics of Germany and Holland which he had recently visited, especially as regards medical and nursing staff and their legal aspect. He emphasized the lack of formalities attending admission, the absence of the suspicion prevalent in England (though powers of detention were exercised), and the fact that these clinics were resorted to by every class of the community. They all dealt with severe psychoses, neuroses, and organic nervous disease in the same hospital. This was a great advantage not only to their work as the research and teaching centres of universities, but to the development of a reasonable attitude towards the psychoses and their treatment both in doctors and the public.

Dr. HELEN BOYLE (in reply) said it would be impossible at that late hour to analyse the various contributions to the discussion. She did not think a strong case against the voluntary attitude in the clinics had been made out. She expected to hear more on the advantages of compulsory detention, which, of course, would include supervision by a Government Department of a nature different from that of a general hospital. The only case at all which had been made out in opposition was by the President, who said he agreed with her. But the points which Dr. Pierce had made were all practical, and of the kind she had herself come up against.

With regard to whether suicidal patients were encountered at the Lady Chichester Hospital, it was always said that the Hospital did not take suicidal patients of an acute

kind; but she never regarded a patient as non-suicidal on the strength of any letters she received from anybody. Her experience had been that one could not accept the general practitioner's view of a nervous patient. Organic cases were sent as hysterics, and hysterical cases sent as profoundly suicidal patients; indeed, there was much confusion in the reports one received. Almost all cases received were taken into an observation ward, and were at first for a short time under constant supervision. That conveyed a sort of general knowledge as to the nature of the cases. She did not say that none of her patients were ever suicidal, but she did not feel sure that evidence that a person wished to leave this world invariably meant that he had a certifiable form of insanity. A large number of presumably ordinary people had such a wish from time to time, and they would not be certified unless they had the tendency in an acute form.

The President said a place for observation was an advantage. That was what a clinic might be. A number of patients were sent in regard to whom it was not certain whether or not they were insane. In early stages of Bright's disease a patient was sometimes certifiable, but at other times not. She had now a private patient whom she regarded as certifiable, but she felt sure she could not get a colleague to agree as to that; no one who had not seen a good deal of her would have the necessary information; an interview would be of no use.

The President also said he did not think these clinics would make much difference to the number of cases admitted into asylums; but she disagreed with him on that point. Latest findings had shown that various forms of insanity began in a small way before the certifiable stage was reached; it was the case with paranoiacs. She had had cases which would ordinarily be considered likely to become paranoiacs, who had early symptoms, such as projection and so on, and she was sure that, if attacked young, and in their early stages, they could have a more serious condition either alleviated or prevented altogether, and if they were given a method of sublimation which suited them, they got on very well. She had kept in subsequent touch with some, and knew what they were now doing.

There were innumerable forms of psychoneurosis, and she found that people used that term for almost any form of mental trouble which they did not consider certifiable. Obsession cases sometimes went into asylums. There were hosts of cases not certifiable which could be treated in a clinic, and which if not so treated would inevitably swell the asylum population.

At the Lady Chichester Hospital there were wards for children, too—a very important branch of the work. In many cases children, if treated early enough, could be prevented from becoming insane later on. There was as yet but little experience on that side, as it was only recently that children had been treated in this way. She believed that attention, both physical and mental, to young cases of severe insomnia, obsessions, and neuroses of all kinds would make an enormous difference to the general mentality of the country.

Section of Psychiatry.

President — Dr. BEDFORD PIERCE.

Recovery from Mental Disorder.

By BEDFORD PIERCE, M.D. (President).

THIS subject was selected in the hope that its discussion might throw some light on the causation of mental illness. The term "recovery," however, is one not easy to define. It hardly means the eradication of morbid tendencies, but rather the restoration of the patient to a previous condition of harmony with his surroundings. Such recovery may take place when the physical condition is far from satisfactory. Many years ago I sanctioned the discharge of a gentleman as recovered, who was actually dying of bodily disease. My justification was that he had recovered completely from an attack of delirium and could fully realize his position.

Recovery from mental disease is so wonderful an event that it is surprising so little attention is paid to it. Physicians in mental hospitals become so familiar with the restoration of mental powers that it ceases to impress them. We enter the ward some morning and the sister tells us that "Mary Jones is better." We go to the bedside and find a woman we have never seen before. Her features, her expression, the eyes, the complexion have altered. Even the hair is changed, it has recovered its lustre and elasticity. But we accept this as a matter of course. It seems no more remarkable than holing out a putt from the edge of the green—not an everyday incident, but one which is flattering to our skill! Yet the recovery of sound mind after weeks of alienation is a momentous event which deserves the most searching study. If we could only understand the exact process involved, we might be able to reproduce it; we might even prevent mental disorder altogether.

The causes and the beginnings of mental illness are seldom known to us, and we have little opportunity of making personal inquiries into them: but the recoveries take place under our own eyes, and convalescent patients are generally willing subjects for investigation. And yet, only too frequently we cannot ascertain the cause of the recovery and the patient can give us little help.

I have asked many patients if they could explain the change, but such inquiry was usually fruitless. Only the other day a highly intelligent young lady suddenly improved. She had been acutely hallucinated and very violent, trying to strangle the nurses and injure herself. In the course of twelve hours all this disturbance disappeared. She could remember her symptoms, what she had thought and feared, but she could not account for her restoration. In this case, probably, prolonged warm baths had been of service, but it would be difficult to believe that immersion in warm water had any specific effect.

Another lady, who had a long and distressing attack of melancholia, stated

that she recovered whilst listening to a lecture on America. It is probable that lecture had awakened happy memories of relatives she had visited in the States, but this was obviously a culminating episode and gave little help in understanding why she recovered.

In this way we are often spectators of an amazing process, which we cannot explain, and which to some may be incapable of explanation without calling in transcendental or supernatural agencies.

The modern revival of the doctrine of demoniacal possession, with the belief that insanity can be cured by "casting out devils," which is encouraged in certain high quarters, is deplorably retrograde and calculated to do grievous injury to the insane. It re-establishes the notion that a stigma is attached to mental disease, and unless it is discredited it will certainly put the clock back as regards the treatment and care of the insane. For this reason above all it is important to investigate the physiological and psychological factors involved in recovery from mental disorder, since superstition thrives whenever science halts or hesitates.

As a contribution to the discussion of this subject my colleagues at the Retreat, Dr. H. J. Mackenzie and Dr. Marguerite Wilson, assisted me in reviewing all the recoveries that had occurred from 1905 to 1920. Only new cases, without any history of previous attack, were considered. All of them recovered so that they were able to resume their normal places in life. The series consisted of 120 cases. The patients had little in common, except that they were all under certificates in a mental hospital. They suffered from all kinds of psychoses, but all so severe that they could not be admitted on a voluntary basis, so that it may be inferred that the bulk of them did not co-operate in treatment. I have not attempted to classify the forms of mental derangement, since in the present state of our knowledge any classification is unsatisfactory. Moreover, I incline to the view that the same injurious agent will produce diverse mental symptoms in different individuals.

These 120 patients I have divided for convenience into four groups. The first, twenty-one cases which relapsed; the second, forty-six cases, in which the cause of recovery was obscure; the third, thirty-four cases, in which recovery appeared to depend on removal of physical disability; and the fourth, nineteen cases in which it was attributed to the relief of mental stress. It may be noted that in the whole series there was a history of mental disease in the patients' family in fifty-nine cases, viz., 49 per cent. An attempt was made to ascertain the average duration of illness before admission and the average length of residence before recovery occurred. But it was found necessary to exclude certain cases on account of the history being doubtful and some others who had been ill so long before admission that they disturbed the result. Consequently all cases with more than eighteen months' history of illness before admission were omitted. The result obtained after these exclusions is as follows: On an average the patients referred to in this paper had been ill rather more than two and a half months before admission, and they recovered after residence in the hospital in just twice this time, i.e., with a little more than five months' treatment.

The first group contains the twenty-one cases which eventually did badly. Most of them never recovered from the second attack. Two may perhaps be mentioned as illustrating how little we can foresee the course of mental disease.

A schoolmaster with a good record, aged 30, broke down suddenly and made a public disturbance. Five days later he was admitted in an acutely maniacal condition. Within fourteen days he was apparently well and able to take an active interest in

games, and within a month he was well enough to leave; but unfortunately a fortnight later he relapsed. Since then he has never shown any signs of improvement, he became dull, inaccessible, almost stuporose and after ten years he appears to be a chronic patient. Why he broke down and why he recovered for a short time was never elicited. A fact of importance in the history of the case is that his only brother is insane.

Another case is that of a highly attractive and competent girl, aged 22, with many interests in life, but with a poor family history, who broke down for no assignable reason and did so badly that premature dementia was feared. She presented many of its characteristic symptoms; was unapproachable, dull and degraded in her habits. As the position was becoming grave I welcomed the opportunity of a consultation with the late Sir George Savage who gave a bad prognosis but suggested a change to some other institution. Before this could be effected the patient began to improve and in three weeks was apparently quite well. After this for five years she took her old position in society and was an extremely active V.A.D. worker during the war. When happily engaged as chief cook in a small military hospital, she suddenly broke down again. This time she has not recovered, in spite of treatment in three separate hospitals. One fact in the clinical record may be noted; when she relapsed the circulation became obviously sluggish, with capillary stasis and at the same time acne spots appeared on her face. No doubt there were important psychological factors not fully elicited, yet the illness, clearly, was associated with, if not directly due, to some physical changes.

The next group of cases, forty-six in number, are much more satisfactory. None of them have relapsed so far as it is known and most of them have been followed up and are known to remain well. In all of these we were unable to assign any definite reason for the recovery. No doubt it was largely due to change of scene, careful nursing, the good effect of the routine life in the hospital, together with medical care and attention to the general health; but unfortunately we have to admit that many cases, which appear similar, fail to recover under these conditions.

If we exclude twenty cases in which there was a clear history of hereditary weakness, there remain twenty-six patients (twelve males and fourteen females) in whom the cause of the malady and the reason for the recovery is obscure. By this I do not wish to imply that treatment was of no avail, but rather that the connexion between the treatment adopted and the recovery was not made out. Thus, a lady who had delusions for three months that her food did not get into her stomach, recovered directly her stomach was washed out. This could not be the whole explanation, for practical demonstration of the falsity of delusional ideas is usually useless. A comparison between the duration of the illness before admission and the length of treatment in the hospital previous to recovery gives very confused results. It is quite unsafe to assume that a case of recent development will recover quickly, or that one with a long history will recover slowly. There is reason to think that in some of these cases the illness will continue almost indefinitely until some change is made.

A young man of good social position was under the care of a well-known experienced alienist. He was suffering from mania and he was nursed in his father's house, with every care, and with ample space for exercise in the private gardens. But there was constant trouble in connexion with the male nurses. He bullied them, took delight in aggravating and attacking them and twice escaped from control and made a serious disturbance. When sent to the Retreat he at once settled down, complaining bitterly, however, that he had not been placed under care before. He speedily recovered and now, sixteen years later, he holds a responsible position. We cannot suppose that the atmosphere of the mental hospital had any specific effect; rather it was his removal from irritating surroundings that gave nature's recuperative powers fair play. I remember a lady patient under my own care for many months, who was degraded, violent, abusive and unmanageable. Her relatives at my suggestion, arranged a

transfer, and she at once began to improve. The morbid routine was broken, and recovery became possible. I have for many years urged the transfer of patients likely to become chronic cases, and though frequently little is gained, except by way of relief to the staff from duties which are both monotonous and depressing, in other cases there is decided benefit. This can hardly be due to change in climate, food or bodily conditions, so much as to the change in personal relationships, new doctors, new nurses, a new angle from which to view life, and perhaps the sense of a new chance to make a fresh start.

One other patient in this group was a young railway servant, with a history of excitement lasting nine months. On admission he was found to be full of impossible schemes for the reorganization of the whole railway system. In seventeen days he was well enough to leave. Probably the mere fact of being certified and placed under care compelled him to take stock of his position. In hospitals there is a frankness, an opportunity for plain speaking, not always attainable in private life; and when this is associated with a sympathetic and unbiased discussion of their personal difficulties patients often adjust themselves speedily.

The second subdivision of this group contains twenty patients and in these also little is known of the reasons for their recovery. In all these cases there was history of insane inheritance. The average duration of treatment in the hospital was longer than usual, viz., six months, and was about three and a half times the duration of illness before admission.

One of the ladies in this group recovered suddenly, in circumstances that will not be readily forgotten by the staff. She had been ill four months before admission and rapidly passed into a state of stupor. For six months she required tube-feeding and never spoke or manifested the slightest interest in anything. But one morning she startled my colleague, Dr. Norah Kemp, by suddenly saying: "You have your reward, Dr. Kemp." It was almost as if the dead had come to life! The remark had reference to a conversation which took place a few days before in the patient's hearing. The ward-sister had mentioned the wearisome monotony of the tube-feeding and my colleague had said casually, "I suppose we shall have our reward some day!" Here it seems probable that to some extent auto-suggestion was at work. Obviously, even while stuporose, the patient continued to receive impressions, one of which appeared to have some influence. And yet we do not understand either why she regressed into a vegetative state, or why she awoke from it. It seems unlikely that my colleague's words, although superficially associated with her recovery, had much to do with it.

It is extraordinary how soon convalescence begins in some cases with a long history.

A lady, aged 52, with a bad family history, had been mentally ill for two years. When admitted she was delirious, she refused food. There was retention of urine and the issue seemed doubtful. But in two and a half months she was well and now, eleven years later, she is reported as being bright, happy, intelligent and interested in life. In such a case we may imagine that the exacerbation of symptoms which led to her being placed under care cleared the air and broke a vicious circle. Or it may be said that the illness would have run its course in any case, and the recovery was similar to the recovery from a specific fever and depended upon the development of powers of resistance. But though this may happen in the toxic insanities, in a case with so long a history it seems an improbable explanation. One thing seems clear, the recovery would not have occurred if the patient had remained at home, and that skilled nursing in a special hospital was the chief factor in leading to the successful issue.

On the other hand, continued residence in a mental hospital is not always beneficial.

A lady, aged 42, who had suffered from melancholia for ten weeks was admitted and improved a good deal during seven months' residence, but did not recover. One day she went home without leave: she remained there and convalesced rapidly. Eight years have elapsed and she is reported to be perfectly well.

The third group representing more than a quarter of the whole number contains cases presenting definite physical disorder, in which some kind of toxæmia appears to be responsible for the physical illness. It includes eight men and twenty-six women, but the patients really have little in common. Alcohol, drugs or other sedatives were the cause of the insanity in ten cases, the puerperium in five, post-operative troubles in four, influenza in four, whilst in the remainder a variety of conditions existed, such as thyroid insufficiency, encephalitis lethargica, diabetes, and others which need not be specified.

In all these cases it may safely be assumed that the mental recovery was dependent upon the recovery of the bodily health. The majority were confusional cases of short duration, but there were also several cases of depression. As previously suggested the clinical type appears to depend on the constitution and temperament of the patient more than on the nature of the poison. If a number of individuals take opium, or are given the same anæsthetic, the mental disturbance produced will vary greatly. Similarly the mental disorder that follows influenza is by no means constant in type. In most of the cases psychogenic factors were also of importance, and in many there was definite history of hereditary weakness. It is unnecessary here to discuss methods of combating infection. I need only add that at the Retreat we have been singularly unsuccessful in our attempts to assist nature by means of vaccine therapy.

Several of the cases included in this group are of special interest:—

Miss F., aged 44, illustrates the complex nature of the problem of mental disorder. She was admitted in a state of acute confusion six weeks after the onset of mental symptoms and was apparently in good health, though she had been attacked with influenza nine days before she broke down. Her family history was poor, four uncles exhibiting various forms of nervous disorder. There was a history of self-abuse since childhood, she was in chronic financial difficulty and had the care of aged parents. The illness was extremely severe, and the practical difficulties in nursing almost as serious as any case I remember. She had attacks of frenzy, and tried to strangle herself, and to break her neck, or her arms, and these attacks were followed by periods of exhaustion with profuse perspiration. Before admission she had deliberately put her hand in the fire, apparently suffering no pain, illustrating the loss of common sensation occurring in acute confusional states. She was found to have albuminuria and there was throughout scanty excretion of urine. For a month she was treated without drugs and then sulphonal was given regularly for two weeks, and from that time she slowly improved; though for a good while she was dazed, disoriented and resentful. After five months' residence she was well enough to go away on trial. She remained well for ten years when she was again attacked by influenza, again developed acute maniacal symptoms and died in four days. In this case there were at least five causal factors—heredity, bad habits, financial stress, wear and tear of nursing, and finally influenza. The last was probably the most important, a view confirmed when we remember the disastrous result of a second attack.

The point of practical interest is the part played by the sedative drug. As a general rule I do not give sedatives for restlessness and excitement, and consider their use a confession of failure. Only when the patient begins to show objective signs of wear and tear do I give them a trial. It is probable they are then more effective. One thing is quite certain, little good will be done with a new case until the drugs given before admission have been eliminated. I consider these drugs of comparatively little use in treatment; indeed, I incline to the view that they frequently delay recovery by hindering natural processes of repair. Yet I have to admit in a few cases, perhaps ten or twelve all told, I have seen striking benefit from sulphonal given regularly for several successive days. But this is exceptional and in most cases the exhibition of sulphonal or other sedatives has been of little benefit.

Miss G., aged 34, was another case of mixed origin. There was bad heredity, a love affair, marked anæmia and an enlarged thyroid. Before admission she made two suicidal attempts, and one of these was especially determined. She succeeded in getting an ounce of paraldehyde, and escaping from control, she took it just before throwing herself into the river. Some time after admission she planned a serious suicidal attempt. She contrived to get out of her room in the early morning, broke into a pavilion in the grounds, turned on the gas used for boiling kettles at tea parties, and then lay down full length on the bench. When found she was on the floor, unconscious, hardly breathing at all, with minute petechial hæmorrhages over her face. It was evident that in going under from the gas inhalation she had rolled off the bench. With great difficulty she was restored and her life was undoubtedly saved by the vigilance and resourcefulness of the nurse who found her. After a residence of fourteen months she eventually made a good recovery. This was principally due to improvement in bodily health, but no doubt during her long course of treatment she became able to make a fresh adaptation to the realities of life.

Mrs. C. was operated upon for suppurating appendix, and became depressed, confused, suicidal and apathetic. In addition to the septic infection there was history of alcoholism. In twelve months she recovered and has remained well.

This was one of several toxic and post-febrile cases, and the strange feature was that the mental disturbance lasted long after the original cause had disappeared. Evidently some factor was present other than infection or poisoning. Lewis Bruce has demonstrated that the increase in the number of leucocytes precedes convalescence and continues long after it is established. Very probably the original toxic agent initiates secondary changes of a more resistant nature.

In this group the average duration of residence in the hospital was about four months, and was rather more than twice the duration of illness before admission.

The last group contains the patients whose recovery appeared to depend on the removal of mental strain or a readjustment to the circumstances of life. The causal factors were in the main psychogenic. The numbers are not large, seven men and twelve women. The average length of residence was about the same as in the preceding group but recovery was relatively quicker, as the duration of illness before admission was less than in the toxic cases. The psychosis was maniacal in character in many of the cases, in some it was almost a delirium. The nature of the mental strain was varied. In one instance a severe attack of mania followed the bombardment of West Hartlepool. The patient, a lady, had rendered first-aid calmly and efficiently in a number of cases under most distressing circumstances. Other causes of mental stress were loss of wife, loss of daughter under tragic conditions, the carrying on of a business with the handicap of a drunken husband, the suicide of a sister, an unhappy marriage, insanity of a wife, war strain as well as sexual difficulties in other cases.

I should have liked to give details of a remarkable instance of *folie à deux* which comes under this heading, but must be content with a brief outline.

A young lady, aged 31, became depressed after a singular love affair, during which there was some sexual impropriety. She suddenly became convinced that she had sold this country to the Germans, and suffered from hallucinations and was acutely suicidal. A week later her elder sister was admitted with very similar symptoms, but in her case the mental strain arose largely out of sympathy with a sister to whom she was intensely devoted. Both illnesses were protracted, lasting over twelve months.

It is impossible to describe cases of this kind adequately or to do justice to the psycho-therapeutic treatment adopted without giving particulars at greater

length than time permits. Moreover, if the cases are faithfully reported, it may involve a breach of professional confidence. There was, however, no formal treatment by analysis, indeed most of them recovered long before much headway could have been made by psycho-analysis. But in every case, there were numberless discussions of an intimate nature upon the difficulties of the situation, which assisted in preparing the way for a reconstruction of outlook. The removal from home, the introduction to a totally new social life, and the regular routine, together with attention to the general health, are all important. In nearly all these cases, however, a considerable interval of time is needed before the patient can build afresh on shattered hopes, or resume his former duties and interests.

Two cases may be mentioned which can be placed in either the fourth or fifth group, in both of which advancing years, with pre-senile changes, rendered it difficult to continue the ordinary duties of life.

One patient, a lady aged 68, developed acute mental confusion and was gravely ill on admission. She had arteriosclerosis with high blood-pressure, yet recovered her mental balance in three months. She has now remained well for three years although she found it necessary to give up all household duties.

The other, a gentleman, also 68 years of age, was ill three months before admission and suffered from mental confusion and depression. In a state of frenzy he made several suicidal attempts and was under treatment for two years. His recovery was largely due to the adjustment of some business worries. He has now been well for more than two years.

The question arises why these two elderly patients should recover when so many others suffering from involuntional changes become chronic invalids. We can only assume that the degenerative changes did not go so far, and that readjustment was possible as soon as external conditions had been suitably modified.

In reviewing the question of recovery from mental illness, I incline to think that in the great majority of cases it is due to the removal of a toxæmia by the recuperative processes inherent in the organism. The methods of removal are no doubt various; but it may be concluded that the cellular elements automatically liberate defensive products which destroy and lead to the excretion of the noxious substances. It has been stated that alcoholic delirium is not due directly to the alcohol but to an excess of certain unidentified bodies produced by the tissues in self-defence. Mental confusion, similarly, may depend on the action of secondary products and not immediately on the effects of influenza, sepsis, or other poison. Recovery, therefore, may not be merely a question of disposing of a micro-organism or some poison, but of combating secondary effects. Even when the cause of the mental disorder is psychogenic, such as bereavement, repressed emotion, or thwarted instinct, there is possibly an induced toxæmia, or a disturbance of the balance of internal secretion.

In treatment no aspect of this subject can be ignored. We have reason to believe that mental disease is generally due to more than one cause and that its incidence arises from several contributing factors. Similarly, recovery is probably not a simple process and it is most likely to occur when the disease is approached and attacked on several sides. In psychiatry we must be eclectic and keep an open mind. Above all we must not identify ourselves with any exclusive method of treatment.

I will not attempt to give even the barest outline of the principles of treatment, nor have my observations afforded any indications of the resources of a mental hospital. If it be thought that the absence of any definite conclusions

amounts to a confession of ignorance, I will merely point out that the actual process of recovery from measles, or shingles, or even lumbago, is just as obscure as that of recovery from delirium or melancholia. In spite of the inherent difficulty of psychological problems, psychiatry obtains very striking results, nor can it truly be accused of lagging behind general medical practice.

The subject of recovery is pleasant to discuss, and a review of successful cases is encouraging and even gratifying. We may be excused if sometimes we take credit without justification since much of our work is discouraging. But I suppose it falls to the lot of all engaged in medicine to receive from time to time letters of thanks which we know are undeserved. It is embarrassing to be congratulated on the wonderful results of our treatment when the patient really recovered without our aid or interference! Still, on the whole, things work out fairly well, since we are often criticized and misunderstood when we have done our utmost.

Happily, genuine satisfaction lies, not in what others say, but in the work itself. It is ample reward for all the care expended to see patients recover and take their place in society. Even if sometimes we are not sure exactly how it happened, it is a joy to have been able to assist, even a little, in bringing about this happy issue.

DISCUSSION.

Sir ROBERT ARMSTRONG-JONES said he differed from the psycho-analytic definition of insanity which the President had given—namely, the withdrawal or retreat from an intolerable situation. He said that it was impossible in every mental case for the person to know what he was escaping from or was trying to escape from. As for the percentage of recovery, in his own experience at Claybury, which extended to nearly a quarter of a century, the recoveries were 33 per cent. Some cases recovered after fifteen or twenty years or longer. The proportion of recoveries was noteworthy among cases which had been transferred to Claybury from a private asylum; this implied no reflection upon the private asylum, because the factor at work might simply have been a fresh social adjustment. He thought that cases with heredity were really more recoverable than cases with no hereditary history at all. Many people were discharged from asylums as recovered, and yet for many months, or even years, were quite unfit to manage their own affairs. One of the greatest factors in recovery was undoubtedly auto-suggestion, which was the same thing as had been described by Chalmers, the great divine, as “the expulsive power of a new affection.”

Dr. HUBERT BOND said that the President's address was not one that lent itself to adequate discussion on first hearing. But it had encouraged him (Dr. Bond) and no doubt his colleagues—in the belief that they were right in the decision they came to some months ago, though with some diffidence—that the statistics as to recoveries should be analysed, studied, and reported on, at the Board of Control, as had been done for many years as to the admissions. They were now, accordingly, going to the superintendents of, at any rate, the Borough and County mental hospitals to furnish a copy of the recovery registers, in the same way as they had long done in the case of the admission registers. That was because they considered valuable results could be obtained from a tabulation of the contents of those registers. He wondered whether the President's thoughts had gone in the direction they had taken because of the hard things, the calumnies and vilifications which had been, and were still being, uttered in relation to County mental hospitals, and the almost triumphant statements as to the alleged lack of adequate recoveries at those institutions. The President's analysis dated from the year 1908, the year when the new tables of the Medico-Psychological Association, and the registers adopted by the Commissioners to harmonize with the tables, came into force. [The PRESIDENT: That is why the date was chosen.] What had the tables yielded? He had brought some figures. In the period under review there were about 22,000 admissions annually into the institutions for the insane, and

the recovery-rate was 35 per cent. But that rate was arrived at by including every case admitted; whereas a fairer gauge would be that obtained by eliminating the manifestly irrecoverable groups of cases. These would include at least (1) the pronounced organic cases; (2) the senile dementias (a different condition from a psychosis in a senile patient); (3) the congenital cases of idiocy or imbecility, but not the milder forms of congenital mental defect, because upon such a condition there often supervened an acute psychosis from which the patient recovered, so that he could leave the hospital and resume the sort of life he had been able to follow before, within his feeble-minded limitations; and, in our present state of knowledge it would be fair to eliminate (4) general paralysis. He felt doubtful as to the elimination of epilepsy. If the forms of mental disorder he had named were eliminated, the recovery-rate rose to 49 per cent. There were also the deaths to consider. But, when all these points had been reviewed, there remained, he thought, about 37 per cent. of admissions who, for anything that was known to the contrary, might just as well have recovered, but did not. One could not say why the recoveries took place, nor why the 37 per cent. did not recover. This 37 per cent. threw upon the specialty a grave responsibility in regard to future inquiry. Dr. Pierce divided his cases into five groups; there were two to which he (Dr. Bond) would refer—the psychogenetic and those cases in which the mental illness was associated with bodily diseases and toxic conditions. But he was sure the author would not say that the psychogenic cases were not toxic, nor that, if the cases were systematically explored, the so-called toxic cases were not also distinctly psychogenetic. Therefore, it became necessary, so far as money would permit—and in all these matters expense was a very important consideration—that there should be closer, more routine methods of inquiry—inquiry along the psychogenetic line, to be effected by employing a much larger number of medical officers, well-trained, and, he hoped, well or at least adequately paid, and by carrying out a much more thorough laboratory investigation. He did not think the laboratory investigation need involve so large an expenditure as increase in medical staff; the amount of money necessary to secure thorough routine laboratory examination of every case would not be very high, at any rate, in comparison with the present cost of maintenance.

Dr. HAYNES asked whether the President drew a distinction between recovery and relief. [The PRESIDENT: The relieved are not included.]

Dr. STANFORD READ said that before one could decide what constituted recovery it was necessary to define the meaning of a mental illness. Much depended on whether one looked on the mental illness from the scientific side, or that of society, for the word "insanity" was purely a social term. An individual might be insane from the standpoint of society, he might not be well enough balanced to return to society, and when he had become so balanced he might be considered to have recovered, though from the scientific point of view he might not be normal, the former idea having been arrived at from a superficial examination, and a greater degree of which might show defects in attention, memory, &c. Much also depended on whether one was going to look upon the causation of disease from the physiogenic or the psychogenic point of view. There was perfect integration between the two, and many of the symptoms which arose might be physiogenic in origin. Through toxic agencies and consequent loss of self-control, psychogenic factors were allowed to come into view. Or a case might be purely psychogenic from the first, and endocrine and other bodily disturbance went hand in hand. In the first case one could understand that the mental balance would be brought about by a readjustment of the physical factors once more causing an altered nutrition and an increased self-control, so that the psychogenic factors retreated once more into the background, whereas in the other case it was quite different. Though Dr. Bedford Pierce suggested that even in the psychogenic cases recovery tended to come about from an alteration in some toxæmic condition, which might be set up secondarily, there was no right to suppose any such thing. If it was psychogenic in origin, though the principles involved might not be exactly understood, one had a right to suppose there had been some adjustment of those factors which were causative of the condition primarily. It was impossible adequately to understand why a case recovered, unless a great deal more insight were obtained as to why a person became ill, therefore we

could only make progress in our ideas of recovery by—as Dr. Bond had said—a greater intensive study of individual cases. But, because of the limited amount of time which a medical officer in a mental institution had at the present day, any examination must, of necessity, be very superficial. The patient's adaptations, from his early life upwards, the history obtained from all who were near and dear to him and who had had relationships with him, could not be gone into; and without such histories the matter was a very difficult one, and it could not be said why the patient had broken down under stress. When this intensive study could be carried out, as in the case of Hoch's work on benign stupor, the profession would be in a better position to judge how cases of insanity arose, and why conditions were recovered from. In cases of manic-depressive insanity, even though a good prognosis had been given, one was surprised to see that many cases did not get better, but passed on to a chronic condition and tended to become demented. Why that was so was not yet known, for the very reason of the lack of that intensive study which he had just mentioned. Therefore he was inclined to think that since the mechanistic view of the toxæmic condition had been so sterile for so many years, advances would only be made along the lines of a far greater study of the psychopathogenic details of each individual case.

Dr. BEATON said that in looking on at a case and seeing it recover, one must honestly admit that one was not doing much more than playing the part of observer. It was, as yet, unfathomable why a patient should suddenly adopt a new attitude towards life, should at a certain stage readjust himself and make friends with those about him. Even if new methods of study were adopted, he doubted whether they would ever reveal the causes of either the breakdown or the recovery, as the whole process was so complex. The most one could get to know was what the patient revealed; it was impossible for another person to get inside his mind. We should bear in mind we did not understand our individual selves. In his view the course of recovery always had two very definite stages. The first stage was one of gradual diminution, and finally of cessation, of the actual morbid symptoms. That was followed by a period of what might be termed convalescence; though he did not like applying that term, as the patient was still ill. In this second stage a readjustment was going on, quite below the level of consciousness of the person concerned. It was usually marked by apathy, lack of initiative, and the presence of submissiveness. One knew, the exhausted class of case, in which the patient had mild hallucinations, with an absolute lack of insight into her condition, yet was continually pestering the medical officer to be allowed to go home to her children, adducing all sorts of arguments in furtherance of that end. But at the end of the first stage the attitude of the patient changed; she no longer applied for leave to go home, but seemed content to remain where she was, an attitude not to be understood in a normal person. This stage, he considered, corresponded with the gaining of insight; in it the patient was prepared to agree with one's explanation. Time was the important factor in mental disease, just as it was in the lessening of the effects of shock and bereavement in ordinary affairs of life; only in course of time could the trouble be seen in its true perspective. On the question as to whether the causation of the disease or the recovery from it was physical or psychic, the profession was still in the dark. He thought too much stress had been laid on the question of infection. If one were to set out to combat all the infections to which we were liable, one would be attempting the impossible. He did not think an aberrant infection could be the absolute cause. There were other factors in causation, such as perverted metabolism, which might be akin to toxæmia. Dr. Lovell, Pathologist at Bethlem Hospital, had clearly shown that in certain cases of stupor there was a very definite increase in surface tension of the blood serum, a very important point, because the behaviour of a cell immersed in a fluid of higher tension was very much altered. It was possible that an alteration in the surface tension of the nutrient fluid might exercise a considerable effect on the function of the cell bathed in it. But that was speculative, and one had to adhere to observation. There were certain types in which recovery could be expected with a moderate degree of certainty, the most notable being the case which showed a confusional phase. It would take too long to enter into the reasons for recovery after confusion, but he thought many of the benefits derived from a confusional state were

not recognized sufficiently. If a patient went on living day by day a life in which he was hallucinated, and if he was continually building up on this false experience, there was very little reasonable hope of recovery. But if the patient became confused, all that was broken up; he no longer registered conceptions, he no longer rationalized. Intelligence was inactive, and during the period in which confusion persisted, recent experience would become lost. And, whether the confusion was due to a drug or to a toxin manufactured in the body, there was the chance that when he had recovered from that, he would return to his normal attitude. He was surprised to hear the President deprecate the use of sedative drugs in this connexion. Of course everything had to be used within proper limits, but he (the speaker) regarded sedative drugs as very valuable. He remembered a very striking case which illustrated that. An officer, during the war, had an acute hallucinatory psychosis; there was no confusion, and the diagnosis was one of paraphrenia. Owing to the hallucinations, he was very excited, and his treatment was difficult, as he could not be controlled. A heavy sedative had to be given, and he had hyoscine and morphia twice a day, so that he was quite confused. Towards the end of the week arrangements had been made for his transfer to a proper institution. The sedative was stopped, and the patient came out of his confusion perfectly normal, so that he was able to discuss the symptoms and see where he was wrong. He (Dr. Beaton) had never seen so striking a case before, and such instances made one wonder whether the asylum physician was acting wisely in not using all the allies to his hand, being, perhaps, too much under the influence of the modern movement.

Dr. NORMAN said he felt, as did others who had to deal with mental cases, that the problem under discussion was a very difficult one. The difficulty would also be evident when medical superintendents had to make the returns of which Dr. Bond had spoken. He hoped these returns would not be like the statistics of which it had been said that one could prove anything by them. There was the trouble of being able to arrive at what was meant by recovery; in such a matter it was very much a question of expediency. There was the case in which the relatives insisted upon discharge, and one had to decide whether the stage of convalescence had gone so far that one could justly say the patient had recovered. If a patient was discharged too soon, although he appeared to have recovered, there was a liability to relapse. A gentleman who did much treatment by suggestion, but had not had much experience of purely mental disorders, said he used to think he had cured manic-depressive insanity; but after a time he found that these patients were being sent back to him again, probably in the next phase—mania, or depression. Then there were patients who had had an attack of melancholia, and no mania, and who did not have another attack for twenty years. Some might try to include them in the manic-depressive forms, but he did not think that should be done. In the case of such a long period of intermission, ought one to say the patient had recovered? He thought this should be stated, although it was known that an eventual breakdown was inevitable. There were most striking cases of general paralysis which were said to have recovered, but, of course, about this one had to be very sceptical. He had seen cases in which spinal injections had been made, and the course of the disease had been modified; and the beginner thought he had succeeded in bringing about a cure. He remembered a case occurring about eight years ago, when arsenical preparations were first being used, in which a well-known man wrote saying he had treated a patient, that he had recovered, and that he went to South Africa. Another case was that of a journalist, under his own care, who was sent to Scotland, and who had a partial remission of symptoms. He was treated with injections, and the relatives said he was cured, and with this the doctor agreed. But was one justified in speaking of it as cure? He thought not: especially in this case, where the patient died eighteen months later of general paralysis! A literary man had a definite attack of general paralysis, from which, apparently, he recovered, doing his mental work for a year. But he died in forty-eight hours after a seizure of general paralysis, in which he never regained consciousness. It was parallel with the case of cancer and the "cures" which were heralded periodically. Before the time when it was possible to make use of the Wassermann and other tests, there could be no certainty, as there

were other conditions which resembled general paralysis. With regard to the use of sedatives, he associated himself with what Dr. Beaton had said. Though sedatives might be abused, he had seen many cases which bore out Dr. Beaton's contention. But a sufficient dose of sulphonal or paraldehyde should be given—so many seemed afraid of the drug. If only a small dose were given, the patient might become even more irritable. Might it not be that if one produced a certain degree of stupor, one got down to areas which were the seat of hyper-function, and, these areas being put out of action for the time, the other cell-areas had an opportunity for settling down and recuperating? In some psychoses there was a toxic effect, and, as a result, a cloudy swelling; and he thought Crile's work demonstrated that in conditions of fatigue, fear, shock, &c., there were states approximating to cloudy swelling. If the involved cells were given a chance of recuperating, the recovery of the patient took place. That seemed to be the effect of sedatives. Dr. G. M. Robertson had said that an excited patient ought not to be secluded; with that he (Dr. Norman) did not agree, for a patient struggling and being held down by nurses tended to become more exhausted. One must reduce the amount of agitation and change in the brain cells, and that could best be done by rest and seclusion and the administration of drugs.

Dr. PIERCE (President), in reply, said Dr. Stanford Read asked, very properly, what was the definition of recovery. He (the speaker) had not used the term "recovery" in the full sense of the word in which it indicated that the person was freed from his disease and his morbid tendencies thoroughly eradicated; but rather in the social sense, the patient being able to resume his place in life and again take up his ordinary duties. With regard to the psychogenic origin of mental disorder, he found it difficult to imagine the cause was entirely psychogenic when the mental states were so very severe as they were often found to be. He spoke, in his paper, of a patient who had severe delirium, and he could scarcely imagine that the whole pathology in such a case could be expressed under the term "psychogenic"; but he thought there must be a toxæmic factor in addition. Had there been time, he would have liked to discuss fully the question of the use of sedatives, as it was a subject in which he was greatly interested and he had spoken on it in his Presidential Address before the Medico-Psychological Association. He could relate cases showing the opposite side to that spoken of by Dr. Beaton and Dr. Norman. If the matter was so simple as giving persons doses of sulphonal, there would be a wonderful recovery-rate in mental hospitals. He agreed that if sedatives were given properly excellent results sometimes ensued, but, in his experience, the balance of evidence was that they were hurtful. For this he thought there was an *a priori* reason; he looked upon sedatives as having the effect of slowing protoplasmic activity and delaying the process of repair, and he considered that sleep which was induced by a sedative might do no good and mean so much wasted time. It was better to use other measures, as far as possible, such as continuous baths, and open air. Yet there were some cases in which these drugs had acted in a marvelous manner.

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OF ORTHOPÆDICS and of the SUB-SECTION OF PROCTOLOGY]



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Section of Surgery.

SUB-SECTION OF ORTHOPÆDICS.

President of Sub-section — Mr. E. LAMING EVANS, C.B.E., F.R.C.S.

Resection of Shaft of Right Humerus for Fibrous Osteitis.

By W. H. OGILVIE, M.Ch.

R. W., AGED 3½. History: Admitted to Guy's Hospital, March 14, 1921, under Mr. Rowlands, for swelling of upper end of right arm. Swelling had been noticed for six months, and had been steadily increasing in size, but was not tender and had caused no pain or disability. Passive movements at elbow and shoulder-joints perfectly free; no limitation in any direction. Voluntary abduction at shoulder slightly limited.

Skiagraph report: Large cystic expansion of humerus, indicative of myeloma, or simple cyst. Not endosteal sarcoma.

Operation, March 17, 1921: Swelling exposed subperiosteally, and part of outer wall chiselled away. Interior contained reddish material, like blood clot, and bled freely. Thoroughly scraped out, cavity swabbed with 1 in 1,000 perchloride of mercury, and wound closed.

Pathologist's report on section: Fibrous osteitis with much cellular tissue; numerous giant cells around cyst, which closely resembles myeloma but is obviously due to the osteitis.

Subsequent history: Wound healed easily, and patient discharged. Swelling of arm, however, continued to increase. Patient X-rayed each month.

June 3, 1921: Following report sent: "The cystic expansion has increased in vertical depth and in circumference, indicating that disease is still active. Increased formation of trabeculae points to attempt at filling up of cavity with new bone. Margins of cystic expansion are still well defined. The X-ray appearances are still those of myeloma, or simple cyst, not of endosteal sarcoma."

Comparison of the two radiograms shows that the expansion has approached much nearer to the epiphyseal line in the ten weeks.

Readmitted June 8, 1921.

Operation, June 23, 1921: Whole length of shaft of humerus exposed along intermuscular planes, and lower end cleared subperiosteally. Then divided 1¼ in. above joint line, and periosteum dissected off whole shaft. This was very tedious, as shell of bone was extremely thin, and perforated in many places, and widely adherent to old scar. Bone again divided just above upper limit of swelling. This point was about ¼ in. below upper epiphyseal line, and line of section included cartilage at one point. Portion removed measured 4½ in. Operation so far had lasted fifty minutes and systolic blood-pressure, taken at intervals of five minutes, had fallen in this period, from 120 mm. to 76 mm. Insertion of graft therefore not proceeded with as originally planned, but periosteal tube was sewn up with catgut, and skin with silkworm gut, the intention being to cut and fix a graft a week later. The arm was simply bandaged to the chest, and child sent back to bed.

Subsequent history: Patient's condition caused grave anxiety. He had taken anæsthetic badly, and subsequently developed broncho-pneumonia, and

for ten days pulse-rate was between 140 and 170. No treatment of arm was possible, in view of chest condition. Wound healed by first intention. On sixteenth day after operation, arm found firm in its whole length, limb moving as a whole, and joints also moving freely. Skiagram taken eighteenth day after operation shows definite shadow occupying whole of gap, where shaft had been resected. Arm placed for one month in position of right angled abduction, and light massage applied. At end of six weeks there was $\frac{1}{2}$ in. shortening, and full range of voluntary movement at elbow and shoulder-joints, with exception of slight (15°) limitation of extension at elbow-joint. Extension at elbow still limited, owing to development of keloid in scar. There is, however, a good firm bone, and no recurrence of growth. The last radiogram shows, moreover, that growth at upper epiphysis has not been interfered with by propinquity of line of section.

Report on section from specimen removed: A typical fibrous osteitis, with much fibrous tissue, much development of new bone, proceeding side by side with absorption.

(With the case are shown the specimens removed and the microscopic sections cut after the first and second operations.)

Case of Bilateral Snapping Hip with Functional Varus.

By H. A. T. FAIRBANK, D.S.O., M.S.

I. D., FEMALE, aged 12. Shown before Sub-section in December, 1920.¹ She then displayed double to-and-fro "snap" in region of great trochanter on each side. Snap on right side more violent than that on left. Our President, in commenting on the case, said he had cured a similar case by rest in plaster and suggested this method should be tried.

The worse hip, the right, was operated upon in December last, the left being treated by fixation in plaster for two months. Tendinous formation on deep aspect of gluteus maximus as described by Wood Jones was seen at operation. This was not very well developed, though certainly present. Attempts at production of snap by stimulation of gluteus maximus or the tensor fasciæ femoris and rotation of thigh failed, apparently because these two muscles could not be stimulated at the same time. Snap was, probably, produced by tensor fasciæ pulling forwards tendinous portion of gluteus maximus which was already in state of moderate contraction. Flap of periosteum turned forward on posterior aspect of great trochanter, and this tendon stitched beneath it. Femoral attachment of muscle was thus prolonged upwards to top of trochanter.

When seen in July patient had had no recurrence of snap.

Case of Subluxation of the Hip-joint of Doubtful Origin.

By H. A. T. FAIRBANK, D.S.O., M.S.

G. R., GIRL, aged 17, first seen when aged 10 at Great Ormond Street in 1914, with the history that nine months before she had had a fall and soon afterwards began to complain of the right hip and to limp. Four months

¹ *Proceedings*, 1921, xiv (Sect. Surg., Sub-sect. Orth.), pp. 61, 62.

before she was seen hard swelling noticed in region of hip, said to be increasing in size. Swelling situated in front of trochanter, apparently due to enlargement of head of femur. Slight limitation of abduction, but movements of hip-joint otherwise normal. Legs equal in length. Radiogram showed head of femur slightly flattened and displaced slightly outwards on neck of femur, which was thickened on inner side; in fact, the appearances somewhat resembled one stage of pseudo-coxalgia. Upper part of acetabulum fluffy and upper lip ill-defined; slight subluxation present. Pelvis otherwise symmetrical. Arthritis of doubtful origin diagnosed, and modified double Thomas's splint applied, legs being abducted.

Patient shortly afterwards lost sight of owing to the war. In January of this year (1921) patient seen again, still limping and complaining of pain on using the limb much. Swelling noted previously, below and outside anterior superior spine, much more obvious. Hip was flexed, but there was limitation of abduction. Trendelenburg's sign present on right. Shortening now present amounting to $\frac{5}{8}$ in. No scoliosis.

Radiogram shows neck of femur apparently shortened and definitely anteverted, with enlarged flattened head, articulating with flattened upper lip of acetabulum. Condition very similar to that seen, not uncommonly, as late result of reduction of congenital dislocation of hip, and more rarely in congenitally deformed hip which has never gone beyond condition of subluxation. Radiograms of both these conditions are shown for comparison.

Case shown in hope of eliciting suggestions as to cause of condition, and as to what treatment, if any, should be adopted. When patient was seen in January last, the symptoms did not seem sufficiently severe to warrant any operative treatment.

Case of Bilateral Absence of Radius.

By G. R. GIRDLESTONE, F.R.C.S.

WHEN patient was first seen both hands were in position of right-angled radial deviation, which I have treated by gradual extension of the contracted tissues by plaster. The question arises of a stabilizing operation later on, and I suggest splitting lower end of ulna and wedging carpus into the gap, or formation of lateral bone-graft to replace absent lower end of radius.

Contracture from Burn treated by Plastic Operation.

By R. C. ELMSLIE, O.B.E., M.S.

L. T., GIRL, aged 13, had extensive scarring of forearm, resulting from burn, when $2\frac{1}{2}$ years old. Thumb was tied to wrist by web which kept wrist flexed at right angles. Plastic operation carried out February 17, 1920—web being split into two skin surfaces, posterior layer cut away from thumb and left attached to side of wrist, and anterior layer cut away from wrist and left attached to thumb. Wrist could then be extended almost to straight line position, and the two flaps were laid in place on raw area and sutured. Cock-up splint used for eighteen months and wrist and forearm treated by massage and exercises. There is now almost full movement and use in hand and wrist.

Old Dislocation of Head of Radius causing Paresis of Muscular Spiral Nerve (shown after Operation).

By PAUL BERNARD ROTH, F.R.C.S.

C. S., MALE, aged 38, plumber, attended hospital on July 15, 1921, for relief of pain in right forearm and hand. Stated that twenty years ago he was caught in a belt, and right elbow was severely wrenched: there was fracture of shaft of ulna and dislocation of head of radius. Treated for fracture; dislocation not discovered until some weeks later and not treated. Patient became apparently quite well, and was able to work efficiently. Six months ago began to feel pain in forearm and hand on moving them. Pain complained of extended down radial side into thumb and was getting worse: sometimes dull and aching, sometimes sharp and shooting. Patient said he "thought the head of the bone was getting bigger and was pressing on a nerve."

On examination: Head of radius found dislocated forwards, so as to lock against front of lower end of humerus and prevent full flexion. Head very prominent beneath skin; pressure upon it at once produced pain complained of. On testing sensitivity of skin, area size of shilling found opposite head of second metacarpal bone on its radial aspect, which was insensitive to pin-prick and to light touch. Muscular power of extensors of wrist only quarter of that of extensors of sound side.

Radiogram showed head of radius dislocated forwards, and curvature of neck of radius tending to make head more prominent. Radiogram also showed old united fracture of shaft of ulna at junction of its middle and upper thirds.

Diagnosis of pressure symptoms in branches of the musculo-spiral nerve due to the displaced bone seemed certain: it was decided to remove head of bone.

Operation, July 18, 1921: After incision had been made through skin and subcutaneous tissue, radial nerve was found lying immediately on head of bone, which was itself enclosed in an envelope of fibrous tissue. The nerve had to be lifted to one side to open this fibrous envelope, and the lifting of the nerve provoked frequent twitchings in the extensors of the wrist. Head and neck having been cleared, the latter was divided and head removed. Wound healed and patient stated that all pain had gone.

When examined on September 28, 1921, nine and a half weeks after operation, patient stated that all symptoms of pain had disappeared. I could find no area of skin insensitive to light touch or pin-prick, and the power of the extensors had distinctly increased. The patient has resumed work.

Case of Congenital Radio-ulnar Synostosis, after Operation, in a Boy, aged 10.

By PAUL BERNARD ROTH, F.R.C.S.

WHEN patient was first seen the left forearm was fixed in complete pronation so that the hand was practically useless. Radiogram showed congenital synostosis of upper ends of radius and ulna. Operation was performed during June of this year, and bony union divided and forearm put up in plaster in complete supination. After a few days, position had to be altered to one of mid-pronation on account of pain, and this position has been retained, thereby converting a useless hand into a most useful one.

The PRESIDENT said he was unable to convince himself that any movement at the superior radio-ulnar joint was now possible, and other speakers agreed that there was no movement at this joint.

Section of Surgery.

President—Mr. RAYMOND JOHNSON, O.B.E., F.R.C.S.

PRESIDENT'S ADDRESS:

Carcinoma of the Jejunum and Ileum.

By RAYMOND JOHNSON, O.B.E., F.R.C.S.

(ABSTRACT.)

(The Paper will be published in full in the *British Journal of Surgery*, January, 1922.)

FULL details were given of three personal cases, in two of which the tumour was in the jejunum, and in the third case in the lower part of the ileum. Each case was treated by resection, but in one case, on account of the presence of acute obstruction, an ileostomy was performed at the first operation, and at a second operation the growth and artificial anus were removed. In one of the cases of cancer of the jejunum the patient was known to be well six years after the operation, but in the other two cases metastases occurred early and proved fatal. The clinical history and physical signs in cases of carcinoma of the small intestine are unlikely to present any diagnostic features, but in those cases in which the condition of the patient allows delay X-ray examination of the stomach and large intestine may give valuable information by exclusion. In each of the three cases and in many others recorded visible peristalsis was a very prominent physical sign, and the character of the abdominal distension varied much, according to the situation of the obstruction in the upper part of the jejunum or in the lower part of the ileum.

Attention was called to the frequency with which the disease pursued an entirely latent course until the onset of acute obstruction, which in some instances was due to the production of an intussusception. This appeared to be more common in the polypoid form of growth than in the stenosing variety. Cases were quoted from the literature illustrating some of the complications which may occur, including perforation, fistulous communication with the colon, and metastases.

Among the various diagnoses made before operation were pyloric obstruction, duodenal ulcer, typhoid fever, carcinoma of the colon, and obstruction resulting from a previous operation for hernia.

The tumour might present itself in four forms: (1) As a polypoid growth, filling the lumen of the bowel; (2) as a ring stricture; (3) as an extensive ulceration; and (4) as a colloid cancer. In the majority of cases the tumour was a columnar-cell carcinoma, but in some instances the cells were spheroidal or polygonal.

Two water-colour drawings by Sir Robert Carswell from the museum of University College Hospital were exhibited. They were executed in Paris between 1828 and 1831, and each represented a malignant growth in the jejunum which had proved fatal from perforation.

Reference was made to certain small tumours occasionally found post mortem in the small intestine. They caused no symptoms, but on examination were found to have the structure of a small spheroidal-cell carcinoma. The investigations of Krompecher and others showed that these tumours, as well as the small growths not rarely found in the vermiform appendix, were similar in structure to certain basal-celled tumours of the skin, and like them showed little, if any, evidence of malignancy. Such tumours had been called "carcinoids" by Oberndorfer.

Section of Surgery.

SUB-SECTION OF ORTHOPÆDICS.

President of Sub-section — Mr. E. LAMING EVANS, C.B.E., F.R.C.S.

Fibro-cystic Disease of Humerus: Replacement by Beef-bone Graft.

By P. MAYNARD HEATH, M.S.

PATIENT, a boy, aged 9, admitted to hospital on June 14, 1921. In April, 1921, his left arm had been forcibly twisted behind his back by another boy. Shoulder-joint became very painful on movement, but no swelling noticed at that time. On May 30, 1921, patient fell on to the same shoulder. He then complained of extreme pain and was unable to move the joint. The part was rubbed with embrocation, and during this process something was heard to crack.

On admission there was distinct swelling of upper end of left humerus and crepitus felt on movement.

X-ray pictures showed upper end of the diaphysis of left humerus to be expanded by a central growth extending up to the epiphyseal cartilage. Compact bone surrounding tumour very thin, and was fractured. Transverse diameter of swelling 3.5 cm. Swelling did not appear to invade medulla of humerus; no evidence of sub-periosteal growth.

Diagnosis of myeloma made on X-ray appearances: it was decided to resect affected bone and replace it by a beef-bone graft, which was cut from tibia of ox of such size and shape as to replace the portion of bone it was proposed to resect as estimated from X-ray photograph. Graft provided with peg at lower end to fit into medulla of humerus, and upper end shaped to fit epiphysis. Holes bored in graft in three planes to admit in-growth of granulation tissue. Graft sterilized by boiling.

June 17: Humerus exposed between deltoid and pectoralis major muscles and periosteum incised on outer side of bicipital groove. Shell of bone and tumour removed subperiosteally and transverse section of humerus made below level of tumour. Distal surface of epiphyseal cartilage thoroughly curetted. Medullary cavity of humerus was slightly enlarged with drill and peg of graft inserted, upper end of graft then lying snugly against epiphyseal cartilage. Periosteum was sutured over graft and wound closed. Arm then put up in abduction at 45° in previously prepared plaster case. Wound healed and X-ray examination (July 2) showed that epiphysis had slipped off end of graft and was lying on the inner side. By manipulation under anaesthesia correct alignment of graft and epiphysis was obtained and arm put up in abduction at right angles to trunk.

Later radiograms show that correct position was not maintained and that upper end of graft projects under deltoid. Union quite firm and large amount of new bone has been formed round the graft.

There is fair range of movement in shoulder-joint, though abduction is limited to 60°, and movements are still improving.

Tumour was soft and vascular. Microscopic section showed it to be composed of spindle cells embedded in a myxomatous matrix. Numerous blood cysts and newly formed blood-vessels are scattered through the tumour. No giant cells are seen.

November 1, 1921.

Case of Congenital Absence of Sacrum, with Associated Dislocation of One Hip and Talipes Calcaneo-valgus.

By R. C. ELMSLIE, O.B.E., M.S.

PATIENT, a boy, now aged 14 months, with very anomalous condition in region of sacrum and buttocks. No gluteal cleft, the two buttocks running into each other and anus situated very far back. Knees slightly flexed, and feet in state of bad calcaneo-valgus.

X-ray photograph shows the two ilia in contact posteriorly, pelvis being extremely narrow. There are three lumbar vertebræ, of which the third appears to be composite, is wedge-shaped, the apex of the wedge being downwards between the two ilia. Left hip dislocated, head of femur being apparently above acetabulum.

The feet have been treated by manipulation under an anæsthetic and the application of internal splints. They have improved, but require further treatment.

Case of Cervical Rib, with Symptoms, in a Girl, aged 8.

By H. A. T. FAIRBANK, D.S.O., M.S.

B. B., GIRL, aged 8. For the last two years mother has noticed that child has not been using the right hand properly. There has been difficulty in picking things up. Right hand is said to be colder than left.

Family history: Mother operated upon fifteen years ago for left cervical rib, which had caused pain in hand and wasting of thenar eminence. Wasting still very obvious. Four years ago mother operated upon for right cervical rib which was believed to be the cause of pain on ulnar side of hand and forearm, but she states that nothing abnormal was found. The operation was, however, followed by cessation of pain.

Examination of child shows marked wasting of right thenar eminence, with complete paralysis of muscles supplied by median. All other muscles in hand, forearm and upper arm normal. Colour of hand normal, though right pulse seems rather weaker than left, and practically disappears on elevation of arm. No pain. Examination of neck negative. Faradic stimulation of thenar eminence produces no result. Sensation absolutely normal.

X-rays: Transverse process of seventh cervical vertebra definitely enlarged, and projects well beyond the limits of corresponding process of first dorsal on both sides of the neck.

The case is shown because of the early age at which signs are evident, and because of the interesting family history. Operative treatment is proposed.

Case of Charcot's Arthropathy of the Tarsus, with Normal Knee-jerks and Normal Pupil Reflexes.

By E. LAMING EVANS, C.B.E., F.R.C.S. (President).

C. H., MALE, aged 31, suffered from syphilis fourteen years ago. Five years ago was operated upon for disease of fifth right metatarsal bone at a general hospital. Three and three-quarter years ago, first noticed swelling of

right foot: swelling has remained constant in size for the last three years: no pain nor tenderness present at any time in swelling.

Present condition: Swelling present involving posterior right tarsus 13 in. in circumference: corresponding circumference of left foot 10 in. Movement at ankle-joint free: no laxity at subastragaloid and mid-tarsal joints. Knee-jerks and pupil reflexes normal. Ankle-jerks absent. Wassermann reaction positive.

X-ray examination shows extensive destructive arthritis in subastragaloid and mid-tarsal joints typical of Charcot's arthropathy.

Case of After-result of Tendon Transplantation in Foot in a Case of Infantile Paralysis.

By E. LAMING EVANS, C.B.E., F.R.C.S. (President).

S. H., AGED 7, was attacked with acute anterior poliomyelitis six years ago. Massage, electricity and mechanical support were employed until June, 1920. The right foot was then in a state of severe equino-valgus deformity. In June, 1920, the peroneus longus was transplanted into the internal cuneiform bone, by passing its tendon downwards along and within the sheath of the tibialis anticus: at the same time the tendo Achillis was lengthened. At the present time the transplanted tendon is functioning as a voluntary dorsal extensor of the ankle-joint in the act of walking.

A Patient showing the End Result of Treatment for Ununited Fracture of the Humerus and Complete Musculo-spiral Paralysis.

By W. ROWLEY BRISTOW, F.R.C.S.

AUGUST 24, 1917: Patient received a gunshot wound of the right humerus which fractured the bone about the middle of the shaft, and divided the musculo-spiral nerve.

September 26, 1917: Bone plated in a military hospital.

September 23, 1918: Musculo-spiral nerve explored; found impossible to resect damaged portion and suture end to end, even if nerve were transferred to front.

December 14, 1919: Humerus being still ununited after a year's treatment in plaster, with passive congestion, &c., operation was performed. Fractured ends cut down upon, freshened, jammed together and held by means of inlay graft taken from tibia. Healing proceeded normally and the bone united.

January 17, 1920: The following tendon transplantation performed, on account of drop-wrist and musculo-spiral paralysis: (a) Pronator radii teres was inserted into extensor carpi radialis longior and brevior: (b) flexor carpi radialis was inserted into extensor ossis metacarpi pollicis and extensor brevis pollicis: (c) flexor carpi ulnaris was inserted into extensor communis digitorum and extensor longus pollicis.

State on November 1, 1921: Right humerus firm and well developed; X-ray shows but little abnormality. Right arm as strong as left. The transplanted tendons are working well and patient can extend wrist, fingers and thumb to almost the normal limits.

Patient is employed as a telegraphist and his hand is fit for use at any ordinary occupation: but would probably not stand the strain of hard manual work as a general labourer.

Case of Loose Bodies in Left Shoulder-joint.

By D. McCRAE AITKEN, F.R.C.S.

G. S., MALE, pensioner. February, 1915: Horse bit patient by left shoulder, shook him and threw him to the ground. Skin not broken. In hospital about fourmonths. Returned to duty in lower category. Shoulder never free from



pain, and gradually became more painful and movement more limited. Discharged from the Army (on account of "hernia") in 1917.

September 27, 1921: Loose bodies removed. The largest—with glass rod in it—enveloped biceps tendon. There were about seventeen others averaging $\frac{1}{2}$ in. in diameter.

X-ray shows fracture of clavicle, now united, of occurrence of which patient has no knowledge.

Old "Abduction" Fracture of Right Ankle, with Dislocation of Astragalo-scaphoid Joint.

By PAUL BERNARD ROTH, F.R.C.S.

M. M., GIRL, aged 15 $\frac{1}{2}$, five years ago injured her right foot when running ; she thinks she fell over a log. She was able to walk home, quarter of a mile, with help, and rested the foot a few days, because it was so swollen, but it was not thought bad enough to consult a doctor.

Three and three-quarter years ago, as the foot had become deformed, it was radiographed, when it was reported that there was "an old injury to the internal malleolus and scaphoid with considerable formation of new bone." An operation was performed and some bone removed : no splint or plaster was applied afterwards. The foot has become steadily worse, turning over more and more on to its inner side, and now the internal malleolus nearly touches the ground. Patient stands with foot rotated out 60° at the ankle and astragalo-scaphoid joints, and with the foot in a position of extreme valgus. A large bony swelling masks the outline of the internal malleolus and appears to consist of a piece of displaced bone and of the scaphoid. When the foot is made to point straight forwards, the patella looks inwards and slightly forwards.

The radiographs reveal (1) old fracture with callus formation of both internal and external malleoli, with displacement forwards of a portion of the inner malleolus ; (2) a fracture-dislocation of the scaphoid bone.

Suggestions are invited as to the best way of getting a stable foot.

In the discussion which ensued Mr. AITKEN said he was opposed to astragalectomy and suggested correction of the deformity by removing a wedge. Mr. BRISTOW agreed that the results of astragalectomy were disappointing, and suggested postponing operative attack until growth was nearer completion. He advised this as although the deformity was marked the disability was very slight at the present time and gave rise to no pain.

Case of Rigidity of Spine in a Boy, aged 9 (for Diagnosis).

By PAUL BERNARD ROTH, F.R.C.S.

S. C., MALE, aged 9, was first seen on January 13, 1921, suffering from double pes cavus and a rigid rounded kyphosis in the mid-dorsal region. He was said to be delicate and easily tired. Radiograms, taken twice, revealed no abnormality of the vertebræ, and he was given a course of exercises to strengthen him and improve his position. These did not seem to do him any good, and after a month were discontinued.

In October he again presented himself for examination, there being no improvement in his condition, and radiograms by Mr. Bracken now revealed (1) the presence of a clearly defined linear shadow on the right side of the spine stretching from the eighth to the eleventh bodies. (Mr. Bracken stated "he was puzzled to account for the right hand shadow ; it was unlike muscle or abscess and was not an artefact, as it occurred in a precisely similar manner on two plates.") (2) Deficiencies anteriorly of the bodies of the seventh, eighth, ninth and tenth vertebræ. (Mr. Bracken did not think these deficiencies were indicative of disease.)

Though there are no signs of pain or discomfort, I suppose no other diagnosis but Pott's disease can at present be made.

In the discussion which followed Mr. AITKEN, Mr. BANKART and Mr. BRISTOW agreed that there was no evidence of Pott's disease in this case. Mr. AITKEN suggested that the kyphosis and rigidity of back were due to septic absorption from the throat and advised treatment on these lines. Mr. BANKART objected to the spine being termed rigid and thought that the case was in all probability one of Friedreich's ataxia, at an early stage. It was agreed that the shadow on the radiogram was probably due to soft parts and was certainly not evidence of tubercle. A shadow thrown by the aorta was suggested as a possible explanation.

Case of Arthritis of both Hips.

By B. WHITCHURCH HOWELL, F.R.C.S.

PATIENT, a male, aged 12. History: Admitted March, 1913 (aged 4), to Queen's Hospital for Children, Hackney Road, as case of rheumatism in both legs. Case later diagnosed as tubercle of the right hip, and treated with extension and tuberculin. February, 1917: Left hip was affected. August, 1917: Subtrochanteric osteotomy of right femur.

Present condition: October 25, 1921, when seen by me. (a) Right hip-joint: dorsally dislocated and apparently ankylosed; flexed 30° ; adducted 30° ; slightly internally rotated; scar of recently healed sinus in groin. (b) Right knee-joint: slight genu valgum; definite lateral mobility; range 180° to 90° . (c) Left hip-joint: Incomplete ankylosis; flexed 40° ; abducted 30° , and externally rotated. (d) Shortening: $2\frac{1}{2}$ in. (e) Knee-jerks: ? Clonus; plantar reflex, flexor. (f) Gait: Inclined to cross legs, walking on balls of toes (right), constantly catching them on the ground; much improved by high boot. (g) Back: Mid-dorsal kyphosis, with marked hump 3 in. long; onset six months ago; no pain, no psoas abscess; marked increase in deformity on stooping to touch toes. Is this compensatory for ankylosis of hips?

Points raised: (1) The exact diagnosis, especially the left hip. (2) The exact line of treatment of the hips: (a) ? arthroplasty left hip; (b) ? subtrochanteric osteotomy (right). (3) The nature of the kyphosis.

DISCUSSION.

Dr. PUGH advised waiting longer for the arthroplasty, and suggested that the kyphosis was tuberculous.

Mr. HOWELL (in reply) stated he did not think the *left* hip was tuberculous, but septic, and suggested the use of B.I.P.P. during the arthroplasty.

Section of Surgery.

President—Mr. RAYMOND JOHNSON, O.B.E., F.R.C.S.

An Operation for Inguinal Hernia.

By Sir G. LENTHAL CHEATLE, K.C.B., C.V.O., F.R.C.S.

SEVERAL cases in quick succession presenting difficulties in the efficient excision of the sac led me to devise a new method by which these and other troubles could be easily and successfully dealt with when they arise.

I approach and reach the back of the inguinal canal from a middle-line incision in the lowest part of the abdominal wall. Unless compelled by some complication I do not open the general peritoneal cavity. All the work is done in a space made in the subperitoneal tissue. I have operated in this way upon forty-one patients. In the first nine I made all the incisions longitudinal. In the remainder I have traversed the abdominal walls by Pfannenstiel's method. I will describe an uncomplicated operation. The patient is placed in the Trendelenburg position and the operator stands on the side opposite the hernia. A transverse skin incision 4 or 5 in. long is made $1\frac{1}{2}$ in. above the symphysis pubis. Its centre corresponds with the middle line. A transverse incision is made in the aponeurosis of the rectus abdominis of both sides, care being taken not to injure either linea semilunaris. The linea alba is undercut upwards and downwards, to within 1 or 2 in. of the umbilicus, and to the symphysis respectively: in doing so the sheath of each pyramidalis muscle will be opened. The opening thus made in the aponeurosis is retracted up and down and the subperitoneal tissue exposed by separating the abdominal muscles in the middle line. The peritoneum and its contents are pushed up on both sides and if necessary kept up by packing. Two retractors are inserted on the side of operation. The retractors should have long, separate and blunt prongs. The lower retractor, by far the most important instrument in the operation, should pull the abdominal wall downwards, outwards and forwards. Forwards to lift up the abdominal wall. Its prongs should reach the deep epigastric artery and vein. I should not advise anyone to proceed with the operation until he is satisfied that this retractor is in its proper position. The upper retractor pulls the structures outwards. After more completely pushing upwards the outer part of the peritoneum and thoroughly exposing the iliac fascia the neck of the sac can be seen entering the inguinal canal. The deep epigastric artery and vein are delimited and separated from the inner part of the neck of the sac. Cheyne's dissector is a very useful instrument to use for this purpose. The spermatic veins and vas deferens with its vessels are found and separated from the whole length of the exposed sac. These structures are usually on the outer and under surface of the sac. Having cleared the sac it is pulled out of the canal by gentle continuous traction in the direction in which it lies. If there are no indications of the possibility of its easy extraction the sac is cut and the canal portion replaced. (Congenital herniæ would belong to this type.) The neck of the sac including part of the parietal peritoneum is then transfixed and removed. Finally the inguinal canal of the opposite side is examined and if abnormalities exist they are treated on the same lines. I have only once had to ligature a vessel in the subperitoneal space and that was a small branch of the deep epigastric vein.

I will now describe: First, the complications and structures with which I have met. Secondly, the herniæ I would avoid. Thirdly, the herniæ I would select for this operation.

(1) *Complications and structures with which I have met.*—I have found and removed unsuspected and potential hernial sacs from the opposite side in three patients. In others I have cleared from the internal opening of the canal firmly attached dimples of the parietal peritoneum. In others I have removed fibrous cords that passed from the parietal peritoneum into the canals. In the case of one patient the urinary bladder occupied the canal and was practically a part of the neck of the sac. The peritoneum was peeled from the bladder, and the sac was radically excised. When I meet with this complication again I shall distend the bladder with fluid; this will render the separation of the peritoneum safer and easier. I may say here that it was a sequence of herniæ in which the bladder appeared that made me devise some procedure by which I could deal more adequately with this complication. I am sure the operation I have described to you allows the adoption of a complete and safe method. In seven cases I found the urachus, which bore the same relation to the sac as that occupied by the bladder. This was traced to its union with the bladder, and it contained a good deal of unstriated muscle, very tortuous patent arteries, and some fat. In a few patients I have traced adhesions between bowel and sac, and omentum and sac, into the general peritoneal cavity, which I have been compelled to open to satisfactorily clear them away. The herniæ in these instances appeared to be reducible before operation. The obliterated hypogastric artery was recognized in the sac in three cases. In one of these the lumen of the vessel was not obliterated at the point of section. From one patient I removed a small sac entering the canal and missed a much larger one that was plastered against the iliac fascia and outer part of the inguinal opening. It was this mistake that makes it so essential to expose the iliac fascia. In another patient I had the following unfortunate experience: I had removed the sac on the left side and found an unsuspected sac entering the internal ring on the right side. I congratulated myself on removing these two sacs from the same opening. In six weeks the patient returned with a direct hernia on the right side. Rightly or wrongly I associated my operation with this new hernia and determined to take Mr. Victor Bonney's advice and adopt Pfannenstiel's method of traversing the abdominal wall. As an unusual complication, I may mention that through the same opening I have removed an appendix from a patient who suffered from appendicitis and inguinal hernia.

(2) *The herniæ I would not select* for this operation are direct herniæ, irreducible enteroceles, hernia in male children under 7 or 8, and old herniæ in which the internal opening has been dragged down opposite the external.

(3) *Herniæ I would select.*—Those in females of any age, and all uncomplicated inguinal herniæ in males over 7 or 8. I would not exclude irreducible epiploceles.

I may here say that I have approached femoral herniæ by the same method. It has been quite easy. In some cases I have covered the internal opening of the crural canal by turning up a flap of periosteum from the pubes and by turning outwards and upwards a larger periosteal flap from the back of the symphysis pubis. In other cases I have blocked this opening by coiling up into a plug the internal saphena vein which I dissected from the thigh as far as the knee.

It is too soon to report upon the success of these measures of occlusion in femoral herniæ, but all is well so far.

“Ligation of the Innominate Artery for Innominate Aneurysm.” By Sir CHARLES BALLANCE, K.C.M.G., C.B., M.V.O. (With illustrations.) This paper is published in the *British Journal of Surgery*, January, 1922, p. 438.

Section of Surgery.

President—Mr. RAYMOND JOHNSON, O.B.E., F.R.C.S.

DISCUSSION ON POST-OPERATIVE EMBOLISM AND THROMBOSIS.

Mr. J. P. LOCKHART-MUMMERY

said that ever since Neolithic times, when surgeons had diligently scraped away with a sharp flint portions of the patient's skull in order to relieve tension in the brain, one of the chief studies of the craft had been how to reduce the risk attending surgical operations. That there must always be a certain element of risk in the performance of any surgical procedure was recognized, but their ambition was to reduce this to the minimum. Few people appreciated how enormously the risk had been reduced during the last twenty years. The risk of sepsis, which was the greatest of all, had now almost reached a vanishing point. Fatal results due to sepsis in clean cases were only a fraction of 1 per cent. Shock, which at one time had been a serious risk, was now, at any rate in the case of private practice, almost entirely preventable. But though these two great causes of death after operation had been minimized, deaths due to pulmonary embolism and infarction of the lungs and brain following operation had come into greater prominence.

Statistics.

It was very difficult to collect reliable statistics in connexion with post-operative embolism and infarction, partly because the condition was not always sufficiently well recognized, and partly because the notes were often incomplete, but chiefly because such an enormous number of cases had to be analysed in order to obtain any figures that were at all reliable. By far the best figures on this subject that had yet been published were those of J. B. Wilson in the *Annals of Surgery*, September, 1912, and collected from the Mayo Clinic during the years 1899-1911. This consisted of 63,573 major operations, out of which there were 47 fatal cases. Unfortunately the non-fatal cases were not mentioned. With regard to the situation of the operation the following list of cases was instructive: thyroid, 2; mouth, 1; stomach, 3; gall-bladder, 9; small intestine, 1; appendix, 4; colon and rectum, 5; hernia, 5; kidney, 1; prostate, 4; uterus and appendages, 10. The embolus was pulmonary in 36 of the cases, cerebral in 10 and coronary in 1 case.

As regards the time after operation at which the fatal embolism had occurred the following figures were interesting: In 19 cases it occurred in the first week following operation; in 21 cases in the second week following operation; in 4 cases in the third week following operation; in 1 case in the fourth week following operation; in 1 case in the fifth week following operation; and in 1 case it occurred in the ninth week following operation.

These figures, though very valuable as regards the fatal cases, gave the impression that the condition was much rarer than it really was, since in his (Mr. Lockhart-Mummery's) own experience not more than about one in ten cases were fatal.

One hospital alone had had eighteen deaths in a single year from this cause. Every surgeon had met with many cases; it was the one complication

he knew no means of preventing or anticipating. It smote his patients out of the dark and spoiled the most carefully planned and skilfully performed operations.

These fatalities might happen after almost any operation and after apparently normal childbirth. They were very rare after accidents. They were most often seen after operations upon the abdominal viscera, less commonly after operations upon the lower limbs, and very rarely after operations upon the arms and head.

Deaths from embolism were probably the worst tragedies of surgical practice at the present time, and not only was no means of preventing them known, but the number of such fatalities appeared to be on the increase, in spite of the undoubted improvements that had taken place, both in surgical technique and in the after-care of patients, during the past few years.

That post-operative lung complications were by no means rare was shown by the hospital records. Out of 3,559 operations at the Middlesex Hospital 5·6 per cent. had developed lung complications during the post-operative period. Out of 6,825 operations at the Mayo Clinic the percentage of post-operative lung complications was 1·2.

Probably the majority of post-operative lung complications, excluding bronchitis, were the result of infarction or thrombosis, and many of the cases that used to be called "ether pneumonia" were really due to this cause. Unfortunately many of the cases were not sufficiently carefully observed, and it was not possible from a study of the records to be certain of the truth in many of these cases of post-operative lung complications.

Cases of embolism and infarct might be divided into three categories:—

(1) Those in which death occurred instantaneously and without any premonitory symptoms.

(2) Cases in which death occurred in a few minutes or hours following an acute and sudden crisis, characterized by dyspnœa and pain in the chest.

(3) Cases in which the patient developed signs of consolidation of part of the lung, following a similar crisis, and recovered.

(1) *Cases of Instantaneous Death.*—This was well illustrated by the following:—

The patient was a man, aged 51, who had had his rectum excised for cancer. He made perfectly satisfactory progress after the operation, but in the early morning of the tenth day, just after waking up from a good sleep, and before he had moved at all, died instantaneously. He was talking to the nurse at the time and she said that he died so quickly that he began a sentence and never finished it. There were absolutely no premonitory symptoms and no signs of distress. A post-mortem examination on this patient showed no clots anywhere in the heart or pulmonary arteries, and no disease of the heart. The brain was not examined.

Such cases were of particular interest, as it seemed certain that they could not be due to ordinary pulmonary embolism. No normal heart would give up the struggle for life without at least an attempt to force blood past the obstruction. In his opinion such sudden death could only result from an embolism in the medulla of the brain. He believed the explanation of these cases to be that a thrombus formed in some of the pulmonary veins, or in the heart, and a portion becoming detached was swept through the left ventricle into the left carotid artery and so into the medulla.

(2) *Cases in which Death occurred in a Few Minutes or Hours following a sudden Crisis.*—In this second more usual class of case, the initial symptoms were always acute pain in some part of the chest, accompanied by dyspnœa,

Section of Orthopædics.¹

President—Mr. E. LAMING EVANS, C.B.E., F.R.C.S.

Stabilizing Operations in the Treatment of Paralytic Deformities of the Foot.

By NAUGHTON DUNN (Birmingham).

ORTHOPEdic surgery is so closely associated with function that perhaps in no other branch of surgery is the patient in a better position to judge of the practical success or failure resulting from any operative procedure. For this reason any departure from the accepted principles of treatment is fraught with considerable responsibility, and should be carefully investigated by such a Section as this.

I propose briefly to record my experience of the operations generally advised for stabilizing the foot, and frankly to state my reasons for departing from these in the treatment of selected cases.

Instability of the foot suggests deformity. This may be apparent only when the patient bears weight on the limb, or it may be a fixed deformity which does not allow passive correction. In the first case the muscles are unable to support the strain of body weight, the ligaments stretch, and abnormal laxity of certain joints of the foot results. In the second case the normal relations of the articular surfaces of the bones of the foot are altered, while there is an adaptive shortening and stretching of the ligaments and muscles controlling these.

Before advising treatment we must be clear on two points: (1) The cause of the deformity; (2) the degree of power present in the muscles controlling the foot and their potentialities of recovery.

The cause of the deformity may in the first instance be postural. Perhaps the commonest example of this is the equinus and slight cavus resulting from inefficient splintage during the acute illness and the convalescence. In other cases deformities result from unbalanced muscular control of the foot, aggravated by incidence of body weight. Here the efforts of the stronger muscles to support and propel the body cause further distortion of the joints which they control. The most severe paralytic deformities are of this type, and paradoxical as it may seem, it may be taken as a general rule that the more extreme the deformity of the foot in infantile paralysis the more successful the surgeon should be in his efforts to improve function.

This leads us to the second question, which is the degree of power present in the muscles controlling the foot and the potentialities of recovery.

Where paralysis of all muscles is complete there is usually little deformity. In these cases recovery of any muscular control is not to be expected. But, where as a result of deformity certain muscles are overstretched but still show some voluntary power, further recovery in these is to be expected by appropriate treatment.

¹ The *Proceedings* of this Section will appear with those of the Section of Surgery until the end of the current Session.

Having satisfied ourselves of the cause of the deformity and the potentialities of recovery in the muscles controlling the foot what means have we at our disposal to improve function?

The generally accepted lines of treatment I think are: (1) Splintage; (2) forcible correction of deformity; (3) tendon transplantation; (4) tenodesis; (5) operation on bone.

(1) *Splintage*.—This is always essential in the early states to prevent overstretching of the weaker muscles while they are undergoing appropriate treatment. It will also be necessary for a time whatever operative procedure is adopted. Its disadvantages are that splints require constant supervision, are often irksome to the patient, and in certain cases will not prevent the gradual increase of deformity.

(2) *Forcible Correction of Deformity*.—This usually involves stretching or division of shortened structures. In this procedure we must remember that the muscular control of the foot is already deficient, so that simple division of the tendon of an active muscle should be avoided. We cannot afford to sacrifice a muscle which by transplantation might be made effective. My chief objections to forcible correction of paralytic deformities are: (i) That the presence of deformity means alteration of articular surfaces with laxity of the ligaments—with lax ligaments relapse is probable unless strong recovery of balanced muscular control is assured. This will seldom be possible in these cases. (ii) That the stretched and torn structures will tend to contract again. (iii) That in cases in which there has been fixed deformity for a prolonged period, much of the original cartilage will have atrophied from disuse—so that an unsound union of the joints may occur when the normal relation of the bones is restored. (iv) That in many cases it is not possible to correct one deformity without giving rise to others.

(3) *Tendon Transplantation*.—Deformity having been corrected the second aid to stabilization of the foot is balanced muscular control. Where the paralysis is not severe tendon transplantation may be effective in restoring muscular balance and so increasing the stability of the foot. I have found it a sound general rule that the most effective tendon transplantations are those where the transplanted tendons are used to replace one of the group with which they are normally in action [1]. In the more severe cases of paralysis tendon transplantation by itself is not sufficient to secure stability of the foot.

(4) *Tenodesis*.—This and other operations designed to stabilize the foot by acting as additional external ligaments have not in my experience withstood the strain of body weight.

These four methods all have their place in the surgery of the foot, but I think it is now generally agreed that in many of the cases of paralytic deformity the stability of the foot obtained by them satisfies neither the patient nor the surgeon.

Splintage may be necessary always—forcible correction of deformity seldom—while tendon transplantation and tenodesis are more likely to be effective when additional stability of the foot has been secured by other means.

It will, I think, be more useful, if my remarks are limited to experience of results obtained by direct operations on bone.

Whitman's Operation [2].—Whitman's operation of astragalectomy with backward displacement of the foot is, I think, generally recognized in America and perhaps in this country as the standard operation for stabilization of the paralytic foot. It was originally devised for cases of calcaneo-valgus deformity, but has in recent years largely been employed in securing stability in other types of paralytic deformity.

Before giving my own views on this operation I will remind you of a recent article by Sever, of Boston, who reviewed the end results of 217 cases of removal of the astragalus in paralytic feet [3]. He reveals the fact that this operation has been practised at the Children's Hospital, Boston, for practically every type of paralytic deformity. His conclusion is interesting. He says:—

“As a result of this analysis of these cases I feel that an astragalectomy is not an operation to be advised for any foot showing lateral instability as a result of the paralysis of one muscle group alone. The lateral instability at the ankle may be averted but more subsequent deformity may develop. It is as good an operation as any in feet which are flail or have only one muscle group left. In the presence of toe flexors varus is likely to develop later and lead to a bad weight-bearing position. The best results I have seen are in those feet in which there was good muscle power before operation and where after operation there was good motion between the tibia and os calcis and good weight-bearing position of the foot. In the latter cases I believe it should never have been performed. It is not an operation which will cure a limp or even improve one as a rule. It is not an operation to be advised lightly or invariably for foot deformities, but should be performed in older children on selected cases.”

Royal Whitman in an article in the *Journal of Orthopædic Surgery* carefully reviews and criticizes this paper [4]. He ascribes the results of astragalectomy at the Children's Hospital, Boston, largely to improper selection of cases and failure in operative technique and after-care. These would explain failure in any branch of surgery, but probably the work at the Children's Hospital, Boston, would not compare unfavourably with that of other hospitals where this class of case is treated.

Lovett, after examination of a large number of old cases of astragalectomy, makes the following statement [5]:—

“My experience has never led me to feel that astragalectomy should be performed in young children, except in cases of severe calcaneus for which the operation was originally devised by Whitman, or in connexion with really serious and threatening deformities of other types. Performed after the age of 14 years it seems to me to be an admirable operation where operation is necessary. It gives a stable foot with little movement at the ankle-joint and in skilful hands a good ultimate result is easily obtained by proper technique, but this I believe is not generally the case in children.”

My own experience of Whitman's astragalectomy is limited, but I join issue with him in his opinion that it is “the only effective remedy” for paralytic calcaneus. The condition of extreme paralytic calcaneo-cavus, associated as it usually is with active posterior tibial and peroneal muscles, lends itself peculiarly well to an almost perfect functional result by the operation which I described at the meeting of the British Orthopædic Association in May, 1919 [6]. The operation devised by Whitman fails from its not controlling or correcting mid-tarsal deformity while it limits movement at the ankle, even in cases where this movement might be easily controlled by transplantation of the unparalysed muscle. It is my own practice to reserve the operation of astragalectomy as an alternative operation for those cases in which paralysis of all posterior tibial and peroneal muscles as well as the tendo Achillis is complete.

If you examine a paralytic deformity clinically or by the X-ray you will find that there is little alteration of the astragalus in its relation to the bones of the leg. The to-and-fro movement at the ankle is normal unless limited by a shortening of the tendo Achillis. The distortion is mainly due to an alteration of the remainder of the foot in its relation to the astragalus. This

may result in cavus, calcaneus, equinus, varus or valgus deformity, or an association of these.

We thus have a deformed foot with laxity and distortion of the joints and deficient muscular control. We can as a rule correct the deformity, we can often redistribute the muscular balance so that this is no longer a factor in the production of deformity, but the joints are still lax and only perfect bone balance would enable the patient to support body weight without the recurrence of deformity. In a large number of cases something more radical is necessary. We cannot restore power to the atrophied muscles; our efforts must therefore be directed to diminishing their task. We can do this by reducing the number of joints which they control. Our consideration of this point will be most effective if we also recognize the relative importance of the various movements of the foot.

The human foot is adapted for support and locomotion. The most important movement in propulsion of the body is controlled flexion and extension at the ankle. We should therefore retain this movement at the ankle if possible. We can do this by sacrificing the mid-tarsal and subastragaloid joints. Bony union of these ensures stability of the foot, leaving such muscle power as is present for control of the ankle movements.

Davis, of Philadelphia, appears to have been the first surgeon to recognize the importance of stabilization of these joints. He devised this operation—that of destroying the subastragaloid and astragalo-scaphoid joints with a gouge, so that ankylosis of these in the desired position might result. In this operation no bone is removed from the wound.

De Forrest Willard first demonstrated this operation to me in 1917, and I tried it in twelve cases. Although sound in principle it is not a clean surgical procedure, while correction of deformity and backward displacement of the foot are difficult. It is a method of arthrodesing the subastragaloid joint which I sometimes employ in cases where this joint is lax and allows deformity on weight-bearing in spite of almost normal muscular control.

Previous to this I had resected the subastragaloid and mid-tarsal joints in cases of calcaneo-cavus deformity, and this is the line of treatment which I would emphasize as the most satisfactory radical operation for stabilization of the foot where this is necessary for any type of paralytic deformity.

I am pleased to see from their recent articles that Ryerson and Hoke in America have been working on the same lines and have reported improved results. Both obtain ankylosis of the astragalo-scaphoid and subastragaloid joints. Ryerson also removes the opposing cartilage of the calcaneo-cuboid joint. The procedure described by Hoke is, I am sure, unnecessarily complicated [7]. He would appear to emphasize the importance of the direction of the neck of the astragalus in its relation to the foot rather than the relation of the foot to the neck of the astragalus.

The results obtained by the double wedge operation in the treatment of calcaneo-cavus were so satisfactory that in 1917 I commenced to remove bone freely in selected cases for the correction of all types of paralytic deformities—the bone removed always including the joints between the astragalus, os calcis and the fore part of the foot and the subastragaloid joint.

Since I was asked to read this paper I have examined 135 cases on which I have myself performed an operation on bone to stabilize the foot during the last five years. I have also had the opportunity of seeing the results in seventy-one cases operated on by Mitchell Smith on the same lines. The large majority of these have been cases of instability and deformity following

infantile paralysis, but a number of cases of severe deformity of the foot from other causes are included. The age of the youngest patient was 4 years, and of the oldest 58; 65 per cent. of the cases were between the ages of 8 and 14.

The deformities for which operation was undertaken were:—

Paralytic.—Calcaneus with associated deformities, 60; flail, 39; equinovarus, 28; valgus, 15; varus, 10; claw, 8; equino-valgus, 7; simple equinus, 5.

Various.—Spastic deformities, 20; relapsed club feet, 11; mid-tarsal arthritis, 2; Charcot's disease, 1.

The essential principle is to ensure bony ankylosis between the astragalus and os calcis and between these bones and the fore part of the foot. If the foot is divided into three sections—(1) the astragalus and bones of the leg; (2) the os calcis; and (3) the fore part of the foot—it is merely a question of careful carpentry to remove bone so as to correct any deformity and ensure ankylosis of these joints in a good weight-bearing position. If necessary, the foot may be displaced backwards in its relation to the astragalus. Actual shortening of the foot may also be obtained by removal of bone at the mid-tarsus if this is desired.

I will describe fully the operation which I practise in selected cases. The objects of the operation are:—

- (1) To correct deformity.
- (2) To shorten the foot by removal of bone at the mid-tarsus.
- (3) To increase the stability of the foot by producing arthrodesis of the tarsal joints.
- (4) To improve the balance and control of the foot by displacing it backwards in its relation to the astragalus and the bones of the leg. The to-and-fro movement at the ankle-joint is retained for ease in locomotion.

STEPS OF THE COMPLETE OPERATION TO SHORTEN AND STABILIZE THE FOOT.

(1) An incision is made just in front of and above the external malleolus to the dorsal surface of the base of the fifth metatarsal bone (fig. 1).

(2) The outer border of the extensor brevis digitorum muscle is exposed. Its origin from the head of the os calcis is divided. By its reflection with the tendons of the anterior tibial group of muscles towards the inner side of the foot, the entire dorsal surface of the tarsal bones is freely exposed.

(3) Note the position of the astragalus. If necessary elongate the tendo Achillis just sufficiently to allow the normal range of movement at the ankle.

(4) By means of an osteotome a portion of bone including the articular surfaces of the head of the os calcis and cuboid is removed. The amount of bone removed will determine the actual shortening of the foot which will result from the operation.

(5) The head of the astragalus is divided just behind the level of its articular cartilage. The proximal cartilage of the cuneiform bones is next removed—so that the bone removed from the inner side of the arch of the foot includes the articular cartilage of the head of the astragalus, the whole of the scaphoid bone and the adjacent cartilage of the cuneiform bones (fig. 1).

The complete removal of the scaphoid bone may present some difficulty owing to its numerous muscular and ligamentous attachments. It is important that it should be completely removed, as failure to do so will give the appearance of flat foot when the foot has been shortened.

(6) A cup-shaped depression is cut from the dorsal surface of the cuneiform bones for the reception of the under surface of the head of the astragalus.

(7) The strong interosseous ligament between the astragalus and os calcis is divided, and while the two bones are levered apart the opposing cartilaginous surfaces are removed by means of a gouge.

(8) The foot is then displaced backwards at the subastragaloid joint. This allows the head of the astragalus to rest in the depression cut for it, on the dorsal surface of the cuneiform bones—while the raw surfaces of the cuboid and os calcis are also in apposition (fig. 2).

(9) The extensor brevis digitorum is replaced and fixed by a continuous catgut suture and the wound closed.

(10) Fixation of the foot in plaster of Paris completes the operation.

At the end of fourteen days the stitches are removed and after final moulding of the foot a closely fitting plaster of Paris case is applied. Walking in this may be allowed at the end of a month, but fixation should be maintained for another four months.

The diagrams show that the calcaneo-cuboid wedge (A-B) is smaller than that removed from the inner side of the tarsus (A¹-B¹). This is due to the fact that the calcaneo-cuboid wedge determines the actual shortening of the foot while the wedge including the scaphoid bone represents the actual shortening of the foot plus its backward displacement at the subastragaloid joint.

A theoretical objection has been made to this operation, namely, the possibility of irregular growth resulting from disturbance of the centres of ossification of the bones of the foot. I have not been able to satisfy myself that this has been a factor in the production of the slight varus deformity which has occasionally resulted. I can certainly produce cases operated on three and four years ago in which the growth of the child has been normal and there has been no recurrence of deformity in the foot.

Although I think I am safe in saying that there has been an improvement in function in all cases, all are not perfect—three patients complain of pain in the sole of the foot, and in several of the earlier cases there is a tendency to varus deformity. The pain complained of is in each case due to a correction of equinus deformity by a mid-tarsal resection, so that the head of the os calcis and the cuboid bear the main pressure on standing. This could have been obviated by elongation of the tendo Achillis and restoring the normal relations of the astragalus and os calcis to the bones of the leg, before removing bone to correct deformity.

In several of the cases in which varus deformity is present, this deformity had been noted when the plaster was first removed. I believe it is due to improper setting of the foot in the post-operative plaster. The surgeon should see that the foot is set in the best weight-bearing position at the end of the operation, and should verify it when the stitches are removed. Any alteration of position by moulding can at this time be easily obtained.

Although we have obtained sound union between the heel, the fore part of the foot and the astragalus, we must realize that muscle pull may still affect the direction of growth in bone. It is therefore essential to maintain a good weight-bearing position of the foot by splintage until muscular balance has been restored by operation if necessary.

In certain old standing cases lateral instability of the foot at the ankle is also present. In these cases there are two alternatives, one is to arthrodesis the ankle as well as the mid-tarsal and subastragaloid joints, the other is to remove the astragalus. Where control of the knee is good the short stable foot which results from the former gives an excellent result.



FIG. 1.—To illustrate lines of bone section in operation to shorten and stabilize the foot. Lateral.

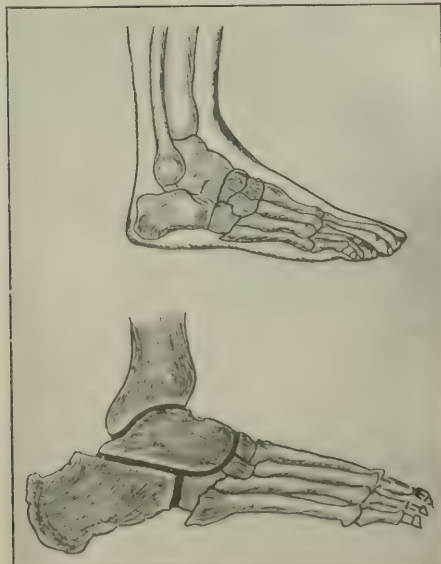


FIG. 2.—Diagram to illustrate relation of bones of the foot after complete operation to shorten and stabilize the foot.

The operations which I have described are comparable to the two types of ankle supplied by limb makers: the Hanger type, which allows easy limited movement at the ankle; and the Rowley type, where the foot is solid and there is practically no movement at the ankle.

The very large majority of those cases had been under hospital treatment for several years, and further operation to stabilize the foot by removal of bone has only been undertaken after a careful consideration of all factors affecting the individual case.

Deficient growth of the paralytic limb is a common sequela. It depends probably on two factors, first, the damage to trophic centres in the cord, and secondly to disuse. The first is beyond our control, but by improving function we can do much to combat the second. It is important, therefore, that any treatment necessary for this should not be delayed.

It is difficult to ensure bony ankylosis of the foot by means of the operations I have described before the age of 7, but I feel justified in performing the operation at any age if reasonable function without progressive deformity cannot be obtained by splintage or tendon transplantation.

SUMMARY.

- (1) The object of treatment is to improve function.
- (2) Experience and a careful consideration of all factors affecting the individual case are essential in the treatment of paralytic deformities of the foot. Operation may not be necessary even in the flail foot if bone balance is perfect.
- (3) Correction of deformity and appropriate after-treatment with or without tendon transplantation may be all that is necessary.
- (4) The site of deformity is seldom the ankle-joint, but usually the mid-tarsal and subastragaloid joints.
- (5) Arthrodesis of one or both of these joints is by itself little disability. It allows correction of deformity and stabilization of the foot, so that the unparalysed muscles may be used to control the movements of the ankle. Shortening of the foot or its displacement backwards at the sub-astragaloid joint are also possible. The tendo Achillis should be elongated if necessary to allow the astragalus to resume its normal relation to the bones of the leg.
- (6) In certain cases arthrodesis of the ankle may also be necessary to secure stability.
- (7) Whitman's operation should be reserved as an alternative operation for cases of complete paralysis with lateral instability, or cases of calcaneo-cavus, where the posterior tibial and peroneal muscles, as well as the calf muscles, are paralysed.

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When the area of lung involved was small the dyspnoea was only temporary and soon passed off. On the other hand when a large area of the arterial field in the lungs was suddenly blocked the dyspnoea was very acute and accompanied by all the symptoms of great cardiac distress. The dyspnoea was not due to the fact that part of the lung was put out of action, but to the strain put upon the heart by the sudden limitation of the area of pulmonary circulation. It was this sudden strain on the heart which caused death, and not the limitation of the ventilating surface. This could not be generally recognized, as the usual treatment was to administer oxygen or a stimulant such as strychnine when a patient first got signs of a pulmonary embolus. This was wrong, as it was useless to kick a willing horse. The heart was straining against a very sudden and serious obstruction in the arterial field from the right ventricle and was in danger of dilating from the strain; stimulants could only increase this difficulty. What was indicated was to try to relieve the pressure in front of the heart by dilating the vessels as far as possible, and for this purpose amyl nitrite was probably the most valuable drug, or in its absence atropine and morphia should be administered, but stimulants should be avoided.

(3) *Cases of Recovery.*—When a pulmonary embolus occurred, the dangerous period was during the first few minutes, and as a rule if the patient could survive for ten minutes he would recover, provided that secondary pneumonia did not supervene. In the non-fatal cases, the dyspnoea soon passed off and the pain in the chest, which was at first acute, subsided. The temperature generally rose in the course of a few hours following the onset to 100° or 101° F., and a cough developed in the course of the next day or two. During the next few days the patient might bring up blood-stained sputum. Examination of the chest as a rule revealed an area of dullness and tubular breathing and sometimes friction sounds. These sounds might be absent if the infarction was not near the surface and was small in area. The symptoms generally passed off within a period varying from a week to ten days. The distinction from ordinary pneumonia lay chiefly in the character of the onset. In embolism or infarction the first symptom was always pain, and the temperature did not go up until afterwards, nor were there any previous signs of illness. In pneumonia, on the other hand, the rise of temperature occurred first as a rule and the dyspnoea was a comparatively late symptom. Also in pneumonia the temperature was much higher and the illness much longer.

Pathology.—When a portion of loose clot passed into the right side of the heart it was driven into the pulmonary artery. If large enough it might block the main pulmonary artery at its bifurcation, or it might obstruct either the right or left pulmonary arteries, thus completely blocking off the blood from an entire lung. Such an embolism would almost certainly cause death in a few minutes, from the strain thrown upon the heart. If the clot was not very large it would pass further into the lung and block one of the smaller pulmonary arteries and would arrest the respiratory circulation in a cone-shaped portion of lung corresponding to the distribution of the artery concerned. The condition thus produced was called an infarct. It produced signs of a localized pneumonia, but was seldom fatal. It seemed certain that clots could not pass through the lung capillaries, but it must be concluded that fat emboli could do so, since fat emboli in the brain were well known to result from fractures of the bones, and the only channel by which the fat could reach the brain was by way of the lungs. Fat was a fluid and probably passed easily through the lung capillaries. Emboli might lodge in the coronary arteries and one such case was included in the figures from the Mayo Clinic. Such cases were probably very rare.

There was rarely any evidence of the source from which the clots were derived. Cases had been described in which the inside of the vena cava had been found to be lined with a layer of clot. When the thrombi were discovered they were most usually in the internal or external iliac veins. Dr. Turnbull said the commonest situation for the thrombus to be found was at the junction of these veins.

No direct connexion was as a rule traceable between the operation wound and the main thrombus.

In considering where the embolus came from it was important to remember the anatomy of the veins. Previous writers had not always done this, and one writer said that he believed ligaturing portions of the omentum by transfixion was liable to result in embolism, forgetting that the omental veins drained into the liver. In this connexion it was important to observe that stomach and gall-bladder operations accounted for a very large proportion of post-operative emboli, in spite of the fact that the veins from these organs were part of the portal system. Von Eiselsberg had suggested that hæmatemesis occurring after abdominal operations, in which portions of the omentum had been ligatured, was due to the separation of small portions of thrombus and an embolus becoming side tracked into the gastric veins. In this connexion it was interesting to speculate as to whether post-operative dilatation of the stomach might not be due to embolism in the gastric veins.

The question of the source of emboli in the brain was confronted with serious difficulties. Very occasionally emboli from the systemic veins might pass through a patent foramen ovale, and there was a specimen of this actually taking place in the London Hospital Museum. It could only account for a very small number of cases.

Cases of cerebral embolism furnished quite a large proportion of post-operative emboli. There were no less than ten cases of cerebral embolism among the forty-seven fatal cases of post-operative embolism in the Mayo Clinic figures, or more than one-fifth of the total.

Thrombi were sometimes found on the valves of the heart when there were fibrinous vegetations. Dr. Turnbull, to whom he (Mr. Lockhart Mummery) was indebted for the advantages of his great experience, described these thrombi as the valve thrombi of Ziegler.

No doubt valve thrombi would account for some of the cases of cerebral embolism, but he did not think it would explain all the cases. In order for valve thrombi to form, previous disease of the valves must have been present, and in most of the described cases no mention was made of any signs of disease in the heart, while in some it was definitely stated not to have existed. Dr. Turnbull thought another possible source of cerebral emboli might be clots formed in the left auricle as a result of post-operative dilatation of the heart when this organ was diseased.

While these sources of emboli reaching the brain were all of importance, they did not appear to explain many of the cases, and he believed that they must look to thrombosis of the pulmonary veins for a source of these cerebral emboli. There seemed no reason why thrombosis should not occur in the pulmonary veins just as it occurred in the iliac, or femoral, veins, but although he had communicated with several leading pathologists, who were most likely to have seen pulmonary thrombosis after operations, they had no knowledge of such a condition. It must occur, but probably it had been overlooked, or mistaken for infarct, which he imagined it would closely resemble, since the thrombus would be arterial in colour.

When he had started to get together the material for this paper, he had had no idea what a difficult subject it would be to tackle. It had proved like the typical detective stories of modern fiction, in which the detective hunted the criminal down from the clue furnished by a cigarette end or smudge on the wall. Each time that he had with difficulty hunted down a clue it had been generally to find that he was up against a fact that completely upset all his previous knowledge on the subject, and he had had to go back to the beginning and start afresh.

One thing this investigation had demonstrated very clearly was the modern tendency for scientific knowledge to be much too shut up in compartments. The surgeon, the physiologist, the clinician and the pathologist, each worked along their own lines, and often did not know what the others had discovered, or how the facts they themselves had found out were of importance to their colleagues. This was a problem which could not be solved without the aid of the physiologist and the pathologist, and the most recent work on blood coagulation was of the utmost importance in connexion with it.

All that he could do in opening this discussion was to lay before them the some which seemed to be of importance, and to suggest possible answers to facts of the many riddles that emerged from those facts.

The prevailing idea at the present time as to the mechanism of pulmonary embolism seemed to be that thrombosis started in one or more of the veins in the immediate neighbourhood of the wound and spread from this into one of the larger veins in the neighbourhood, and that at some subsequent period a portion of this clot became detached. Although this might be the mechanism in some cases he felt certain that it was not what usually happened.

Another theory, which was similar, was that emboli were due to interference with, and damage to, the large veins in the pelvis and that this set up intravascular clotting in these veins.

His main argument against both these theories was that they did not fit in with cases of embolism following such operations as simple laparotomy, hernia, interval operation for appendicitis, or operations for piles. Moreover they could not account for the cases of crossed thrombosis. It was not uncommon to find thrombosis occur, for instance, in the left saphena vein, or femoral vein, after an operation on the right side, or vice versa. In looking through the records of cases of embolism one was struck by the fact that in quite a number of cases thrombosis in the leg had been present, either before the occurrence of the embolus, or after, and that this thrombosis was not always on the same side as the operation, where the latter was unilateral.

When a thrombosis occurred in one of the veins of the lower limbs it was naturally detected, but obviously if thrombosis occurred in one of the pelvic or abdominal veins it was not likely that this could be detected unless it was able to cause such an arrest of the circulation as to produce swelling in one of the lower limbs, or other signs of inadequate venous circulation in the parts drained by that vein. Quite extensive thrombosis might take place within the abdomen without producing any obvious signs of its presence in a recumbent patient. It was true that it would almost certainly cause pain, as pain was always a first symptom of thrombosis, but as the pain would be referred to some part of the abdomen its real cause might easily be overlooked: in fact in looking through the notes of some of the cases of embolism he found that in several it was stated that vague pains in the abdomen had been complained of by the patient before the occurrence of the embolism.

He believed that embolism of the lung following operation was due to the

same mechanism as embolism following thrombosis in the leg, and that the thrombus did not necessarily spread from the site of operation into the larger veins. The thrombosis in fact was not due to injury to the blood-vessels produced by the operation, but to sluggish circulation in the veins of the trunk and legs resulting from lack of normal movement of the muscles. This would help to account for the fact that embolism very rarely, if ever, occurred after operations upon the upper limbs, as patients did not keep so quiet after such operations as they did after operations on the lower limbs and trunk. This also helped to account for the fact that anæmia and conditions of lowered vitality seemed to predispose patients towards a post-operative embolism. It also probably explained why children did not appear to suffer from embolism. It was never seen in children, and this was no doubt due to their more active circulation. Children did get pneumonia after operation, but it was always of the broncho-pneumonic type and had none of the characteristic symptoms of pulmonary embolism.

There was a very prevalent belief that embolism was due to sepsis—a belief he could not accept, although the figures clearly showed that pulmonary embolism was more frequently seen after operations in which there had been sepsis than after operations in which there was none. That was to say, the percentage of cases of pulmonary embolism was higher in septic than in non-septic cases; yet the majority of cases followed by pulmonary embolism were clean cases in which there was no evidence of sepsis at all. Beckman, in his review of 6,825 operations, came to the conclusion that sepsis could not be the cause of thrombosis.

To get over this difficulty some surgeons said that embolism resulted from a mild form of sepsis unaccompanied by the breaking down of the wound. This, surely, was merely begging the facts. If sepsis were the cause of the condition the secondary focus caused by the embolus becoming arrested should become septic and they would have a pyæmia. This was, however, not the case; true, a secondary pneumonia often developed, but this was not as a rule a septic pneumonia, but a pneumonia due to the ordinary pneumococcus having been able to obtain entry to the damaged lung. Moreover the time at which the embolism occurred did not fit in with its being due to sepsis. Embolism sometimes occurred within a few hours after operation, which would seem to be altogether too soon. On the other hand in not a few cases the embolism did not occur for three weeks, or even longer. He had notes of one case in which an infarct formed three weeks after a simple laparotomy which had healed by first intention, of another case in which a fatal embolism had occurred six weeks after a normal confinement; and Wilson recorded a fatal case of embolism nine weeks after operation.

They had to remember that septic wounds were usually more painful than non-septic wounds, so that the patient would probably keep much quieter. Further, the patient's general health was lowered and blood changes would probably occur which would encourage the formation of intravascular clotting. In these ways he believed sepsis was a contributory cause, but not the direct cause he felt certain.

It seemed at present impossible to determine what, if any, was the exact relationship between sepsis in the wound and post-operative embolism and infarction. There was no proof that septic organisms, or their toxins, in the blood could induce intravascular clotting, and on the other hand septic diseases which were not the result of wounds, such as whitlows and abscesses, sapræmia, scarlet fever, &c., were not complicated by emboli and infarcts.

They were, however, still very much in the dark as regarded the exact morbid physiology of phlebitis.

Summary of Evidence.

From the mass of evidence available certain salient facts emerged. Evidently a relative degree of stasis in the veins, or at least a very sluggish venous circulation, must be present before intravascular clotting could take place. This was shown by the fact that thrombosis did not occur in children, its extraordinary rarity after operations on the arms, its greater frequency after just those operations which predisposed towards lack of movement in the trunk and lower limbs. Also the interesting fact that thrombosis in the portal vein and its tributaries was very rare, although its branches were more likely to be damaged during operations upon the abdomen than were the branches of the vena cava. This, it appeared, was explained by the circulation in the portal vein not being affected by recumbency and absence of movement in the same way as the circulation in the cava.

The marked difference as regards the liability to post-operative emboli between the lower part of the trunk and the upper (for although an embolus occasionally followed operations on the neck and breast it was very rare) was, he thought, explained by the fact that the venous circulation in the upper part of the body was controlled by the brain. A free flow of blood through the brain was essential to life, and consequently anything approaching stasis in the big venous trunks of the neck and chest was impossible.

It might be concluded that a considerable degree of venous stasis was a necessary factor in post-operative thrombosis. It was not, however, the only cause of the condition, as there was no evidence that mere recumbency or lack of movement would result in thrombosis, apart from operation. Embolism did not occur after rest cures, or in paralysed persons who had lain in bed for months, or even years. Obviously were mere venous stasis the cause they would be liable to get thrombosis and embolism as the result of normal sleep. The physiological proof that mere venous stasis did not cause thrombosis was of very ancient date; over one hundred years ago Hunter had proved that intravascular clotting would not take place in an intact vein, even when both ends were ligatured. His now famous "living tube test" experiment had been made by tying at both ends 6 in. of the jugular vein of a horse. He found that the blood was still fluid at the end of six days.

Recently Professor Collingwood, of St. Mary's Hospital, had shown that intravascular clotting did not follow the intravenous injection of thrombin (a substance which activated coagulation of the blood), even when injected in relatively enormous quantities, *if the blood was in a condition of movement or circulation*, although the amount of thrombin injected was actually sufficient to clot every drop in the animal's body if removed from the vessels. The conclusion they arrived at was that while venous stasis was a necessary factor in the production of thrombosis, it could not alone cause intravascular clotting, and some other factor must be present. The physiologists had shown that in order to produce clotting of the blood a substance called thrombokinase must be present in contact with it. This substance, thrombokinase, was not present in blood, but was found in varying quantities in all the other tissues of the body, and a certain quantity was liberated whenever a wound was made. It was the contact of the escaping blood with the thrombokinase present in the wound tissues that determined the formation of the clot which sealed up the vessels after a cut had been made.

The change which took place to produce a clot when thrombokinase came into contact with blood might be expressed as followed:—

Thrombokinase + a calcium ion + thrombogen (normally present in the blood) = thrombin.
Thrombin + fibrinogen (present in the blood) produced fibrin (clot).

Thrombokinase introduced into a vein would produce intravascular clotting if the blood was stationary, but apparently failed to do so if the circulation was active.

It had been stated that increasing the calcium content of the blood would cause intravascular clotting, but more recent work had proved this not to be the case. The amount of calcium present in the blood was the maximum required and the addition of more calcium tended to reduce the clotting capacity, rather than to increase it.

It would seem, therefore, that before a thrombus could occur two factors must be present:—

(1) A certain amount of thrombokinase, derived from the tissues and liberated by the wound must be present in the blood, but the amount would appear to be very slight.

N.B.—The reason why intravascular clotting occurred when a vein was damaged, or inflamed, was that thrombokinase was liberated at the point where the intima was damaged.

(2) There must be a venous stasis.

Neither factor alone could produce a thrombus, but both must be present at the same time.

The sequence of events would seem to be somewhat as follows: as the result of the wound thrombokinase was liberated from the tissues and passed via the lymph stream into the circulation (or possibly passed direct into the vein); this enabled a clot to form in those veins in which the blood was for the time being stagnant, or nearly so.

It must therefore be concluded, if this were correct, that the clot probably formed either during the operation or very shortly afterwards, that it spread to a greater or less extent, and that at any period after this, from a few hours up to many weeks, a portion might become detached and produce an embolus, the subsequent result depending on the size of the embolus, the part of the circulation in which it had started, and where it became arrested.

Prevention and Treatment.

The condition was not one that lent itself to treatment, and the only hope of the surgeon lay in being able to prevent the occurrence of intravascular clotting after operations.

It would appear that could he prevent thrombokinase from the tissues getting into the blood as the result of the wound, there would be no possibility of intravascular clotting. But while this would seem to be the ideal, the state of our knowledge did not enable us to even guess at any means of attaining it. The only chance at present of preventing post-operative thrombosis would seem to be by taking such steps as were possible to prevent venous stasis in the branches of the vena cava.

The present methods of anaesthesia, which included a preliminary injection of morphia, probably tended to encourage thrombosis by slowing the blood stream during the dangerous period. This possibly explained the greater frequency of cases of embolism during the last ten years, as compared with the time when most patients had been given gas and ether, with consequent vomiting as soon as they left the table.

The best method of preventing patients from getting post-operative thrombosis did not consist in altering the technique of operations, but rather in altering the methods of preparation and of after-treatment. Preliminary purgation of the patient should be entirely abolished; if really necessary, it should be carried out five or six days before operation. The practice of starving the patient, both before and after the operation should be discontinued.

The supply of proper mattresses on the tops of operating tables, and the prevention of the patient assuming a constrained position, during the operation, which would cause pressure upon the veins, were points requiring greater attention. The frequency of embolism after gynæcological and gall-bladder operations was possibly attributable to the unnatural positions in which the patients were placed.

After operation the patient should be moved or be encouraged to move about, and the large muscle masses of the legs, thighs and buttocks should be made to move, or kneaded, in order to promote venous circulation in the cava. Whether assistance might be obtained by the use of drugs to minimize intravascular clotting was a point for pharmacologists to decide.

He had tried to ascertain whether the post-operative embolus was commoner in this country than in America, where it was the custom to get patients out of bed much sooner than here. Available statistics pointed to this being the case.

Trendelenburg had suggested operating in order to remove the thrombus from the pulmonary artery, and there were records of this operation having been performed five times. Although several patients had survived the immediate operation they all subsequently died in the course of a few days. This procedure did not appear to be sound; more particularly as there was no means of making an exact diagnosis between an embolus of a pulmonary artery and a cardiac embolus; moreover patients who survived long enough to undergo an operation would probably recover if left alone.

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Mr. VICTOR BONNEY

said he thought pulmonary embolism was the result of sepsis, but of a sepsis which presented peculiar features. The term sepsis could be extended to include the effect of toxins, like thrombokinase or some other clotting medium. In gynæcological operations the disaster usually occurred from the tenth to the fourteenth day after operation, and the same was true after labour. Phlebitis, as ordinarily understood, did not account for pulmonary embolism; it was comparatively rare to see a clot from an inflamed vein entering the circulation. In the cases under discussion something of which the profession was at present ignorant occurred between the tenth and twentieth days. The progress of these cases to the time of the accident was, as a rule, fairly satisfactory, but in most the temperature chart for a week previously showed an up-and-down line between 97° F. and 99° F. in the evening. This was what he

was accustomed to call "subnormal fever." Pulmonary embolism in gynæcological work was especially associated with the uterus, being seen much less often in patients with ovarian, tubal and vaginal conditions. The uterine veins were very irregular, and he believed there were no valves in this venous system. There was probably some auto-infection of the veins between the tenth and twentieth days, one not necessarily due to an organism, but to a toxin or some substance which but slightly damaged the vein wall. Such thrombi were easily detached, and in valveless veins, such as the uterine, embolism would necessarily occur. At Chelsea Hospital for Women between 1895 and 1910 1,573 hysterectomies had been performed and pulmonary embolism had occurred in eleven cases (6·9 per 1,000), but in the period 1905-18 in 1,791 hysterectomies, there had only been five cases of pulmonary embolism (or only 2·7 per 1,000). This did not support Mr. Lockhart-Mummery's view that the condition was becoming commoner. A similar reduction was seen with other pelvic operations.

Dr. ARTHUR LATHAM

said in his experience, infarction of the lung was the most common complication in the lung after operation. He had always felt doubtful about "ether pneumonia"; he had never seen such a case clearly due to the anæsthetic. The bulk of the cases of pulmonary infarction were not correctly diagnosed, even at the present day. In the majority of his cases the temperature rose to about 103° F., and required ten days in which to become normal. This rise he thought was due to the same cause as that of the hæmothorax which occurred during the war, the absorption of fibrinogen, or a derivative of it. The great necessity was complete quietude and the avoidance of meddlesome tactics. He agreed that the dyspnœa was due to the heart condition, not to the state of the lung itself; it was caused by the difficulty of the pulmonary circulation getting level with its conditions. Some even large infarctions were not fatal. Sometimes pulmonary embolism occurred suddenly in people who were apparently healthy, and subsequently the thrombosis was found in a distal vein. In clotting it was not that a thrombus occluded the vein at once; but a thrombus was formed, then the red cells and the white cells were shaken out by the rush of blood and only fibrin was left. This contracted, causing partial occlusion, and subsequent layers of fibrin formed on that. Sir Almroth Wright's researches showed that the giving of citric acid (not citrates, which were inert) diminished the tendency to thrombosis.

Sir CHARLES GORDON-WATSON

said that in his own practice during the past year, he had lost three patients from massive pulmonary embolism and had had four cases who suffered from pulmonary infarction—with recovery—following operation. In his opinion the massive emboli were usually non-septic in origin and resulted from detachment of clots in bulk from the iliac veins and their branches or from the right auricular appendix, whereas the infarctions were due to the detachments of fragments of clot broken down as the result of sepsis, usually of a mild character. Dr. Spilsbury in going through the recent post-mortem records at St. Bartholomew's found that there had been nine deaths from pulmonary embolism in 1,013 consecutive autopsies. In the Gynæcological Department of the Johns Hopkins Hospital, there had been thirty-one deaths from pulmonary embolism in 21,000 operations. In this series, there were 205 cases of thrombosis. It was important to consider what were the con-

tributary factors in this calamity. They might be classified under three headings: (1) Loss of fluid from the blood; (2) slowing of the blood stream; (3) trauma of vessels.

Under the first heading, excessive purging before operation, hæmorrhage during operation and vomiting after operation were possible, if not probable, contributory factors and were more or less preventable. It was a mistake to purge before the operation in most instances. This had practically never been done in operations at the Front, and patients were certainly none the worse for the omission and probably benefited thereby.

Under the second heading, post-operative shock, diminution of respiratory effort and of muscular contraction in the limbs, associated with a recumbent and fixed position in bed, must be important factors in a sluggish venous circulation. Here again the surgeon was able to take action. For the past year, in his own wards, he had carried out a scheme of respiratory exercises, especially after abdominal operations. Patients were not kept in a fixed position and in the absence of thrombosis were moved freely while in bed and allowed up much earlier than they used to be.

Under the third heading, bruising of vessels by violent methods, with hands or retractors, transfixion of large veins with needle as in pelvic operations or twisting of large vessels causing damage to the lining at a distance from the point of division, clamping of vessels in a mass of tissue without ligature above this point, ligature of small venous branches close up to the main trunk, were all possible factors in exciting thrombosis. It was noticeable that the great majority of these pulmonary emboli followed uterine operations in which big veins were liable to be dealt with in this way. Another factor under this heading, he believed, was posture during the operation. Gall-bladder operations were high up in the list. In these operations the spinal column was usually unduly stretched. This must have some pressure effect on the inferior vena cava and to some extent obstruct the circulation through it. And the high Trendelenburg position in pelvic operations might act in some similar way by pressure of the ventricles on the right auricle and inferior cava. These points with regard to trauma and pressure were obviously speculative but in a problem of this sort it was necessary to look round in all directions for possible factors. Damage to the epigastric veins in incisions in the lower abdomen either by stretching the parts, especially with retractors or by direct injury to the veins or by pressure from hæmatomata in the rectal sheath, might be the starting point of thrombosis occurring in such operations as removal of the appendix. It was possible, too, that similar injury to the superior epigastric vein via the superior cava might be a starting point in connexion with operations on the upper abdomen such as gall-bladder operations. It was interesting to note that in this connexion in one of his own fatal cases following a gall-bladder operation, there was thrombosis in the superior vena cava and right innominate veins.

With regard to the exciting cause of thrombosis, Mr. Lockhart-Mummery's suggestive remarks in relation to thrombokinase were attractive, and it might well be that the release of this substance during operations provided the missing link in this complex problem.

Dr. J. KINGSTON BARTON

said that, speaking as a physician, owing to the comparative frequency of minor cases of thrombosis or phlebitis in the lower limbs after operations, as well as the much more rare calamity of pulmonary embolism, he had made it

an insistent practice to give fresh lemon juice (with no sugar) to all patients before and after every operation which involved stasis of the large veins. In his opinion, too little movement of the lower limbs did seem to favour thrombosis in the veins of the legs: on the other hand he felt strongly that too early movement of the trunk was often associated with sudden death from pulmonary embolism.¹

Dr. J. BLOMFIELD

said that the discussion had been consoling to the anæsthetist. Conditions of the lung often laid to his charge under the name of "ether pneumonia" had been shown to be really due to small infarcts of the lung, for which the anæsthetic was not to be held responsible. As a matter of fact the commonly used anæsthetics, in the doses in which they were employed, had little influence on the coagulation of the blood. A more formidable influence would be the deprivation of fluid through protracted vomiting, and on that score anæsthetists to-day were less often to blame than in the past. It was difficult to associate an embolus which arose fourteen or twenty days after operation with the anæsthetic in any way at all.

The PRESIDENT

referred to some experiments in which irritation of the pulmonary artery in animals had caused most intense dyspnœa, evidently of a reflex character. It was practically certain there was a cause for the dyspnœa apart from the mere throwing out of action of parts of the lung. He would have liked to have heard whether post-operative embolism was common in France during the war. One physician with a large experience of post-mortem examinations in France had told him he never saw a case of the kind there; yet many of the conditions supposed to favour it must have been constantly present, such as extensive damage to tissue and immobilization of limbs. On the other hand, the men were of good physique and very fit, and certainly did not suffer from over-preparation for operation.

Mr. W. McADAM ECCLES

said that he had had three cases of pulmonary embolism in six weeks recently. Two of these cases were fractures, and the possibility was suggested to his mind that in many of these cases of embolism after operation the conditions were similar to those obtaining in a fracture, namely, there was injury to the vessels and injury to the fatty tissue. It might be that clot causing pulmonary embolism in some way resulted from this combination. Both these cases were comminuted fractures opening up a good deal of medullary tissue, but there was no wound, and consequently sepsis was not to be expected. There was no rise of temperature in either case. The third was an operation case—a case of hernia in a very stout man, who was getting up a fortnight after operation when he suddenly died.

¹ Attention should be drawn to the following valuable contribution on this subject by Dr. Thomas Lewis, in his "Clinical Disorders of the Heart Beat," p. 103: "In post-mortem examinations of hearts which have suffered with auricular fibrillation, the virtual paralysis of the auricles and the consequent stagnation of blood in them, definitely predisposes to thrombosis in the appendices. As long as the blood-stream is very weak and slow, the clots do not shift their position. But when the heart improves and goes slower and firmer, then is the time a pulmonary infarct is apt to come about."—J. K. B.

Section of Surgery.

SUB-SECTION OF PROCTOLOGY.

President of Sub-section — Sir CHARLES GORDON-WATSON, K.B.E.,
C.M.G., F.R.C.S.

Case for Diagnosis.

By J. P. LOCKHART-MUMMERY, F.R.C.S.

J. Y., MALE, aged 29. Admitted to St. Mark's Hospital, September, 1921. Patient had been in perfectly good health until May, 1921, when at 9 o'clock in the evening he had an acute attack of abdominal pain in the umbilical region, and vomited on and off all night. The pain disappeared next day. There was no constipation. The pain came on again at intervals and was very severe. The abdomen was distended, but he was relieved by enemata. Two days after the onset his doctor felt a lump in the left iliac region. His temperature was raised to about 100° F., but there were no rigors. History of enteric in 1917. No other history of previous disease. Patient was sent up to me at St. Mark's Hospital.

Examination: Evening temperature 99·6° F. Some nocturnal frequency of micturition. Examination of the abdomen showed a hard fixed tender mass in the left iliac fossa, passing towards the middle line. *Per rectum* 3 in. up there was a stricture which admitted the tip of the finger. It felt as if plaster of Paris surrounded the rectum. No fluctuation could be made out, and the urine was quite normal in appearance.

Operation: On the abdomen being opened the whole pelvis was found to be full of dense fibrous tissue and adhesions. The lower part of the sigmoid, the rectum and bladder were all one hard immovable mass. The mass was so hard that it was quite impossible to separate anything, even from the anterior abdominal wall. There was no free fluid. The appendix could not be found. The rest of the abdomen was quite normal. No pus discovered.

The abdomen was closed and the patient put on potassium iodide, 3 dr. a day. At the present time the condition *per rectum* is unchanged. Temperature is now normal, and the abdominal swelling appears to be going down.

Specimens were taken and examined for actinomycosis, &c., but nothing was discovered.

Cyst of the Rectum.

By J. P. LOCKHART-MUMMERY, F.R.C.S.

PATIENT, a female, aged 38, children's nurse. Admitted to St. Mark's Hospital. The history was that a year ago she began to notice aching pain in the rectum, which prevented her from sitting comfortably. Her doctor dis-

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covered a lump posterior to the rectum, and between it and the sacrum. She had lately been complaining of some difficulty in getting the bowels open, as the lower bowel appeared to be partially blocked up. The patient was otherwise in perfectly good health.

On examination *per rectum* a smooth globular cystic swelling about the size of a tangerine orange could be felt bulging into the rectum, and situated in front of the coccyx and lower part of the sacrum. It lay exactly in the middle line, and could not be moved about. It was not tender on examination. Diagnosis of dermoid cyst of the rectum was made, and the patient was admitted to hospital for operation.

Operation: The cyst was exposed by longitudinal excision from behind and the coccyx excised. The cyst turned out to be loculated; it contained sebaceous matter, and was lined with smooth epithelium. There were no hairs. It was very adherent, and was only separated from the lumen of the rectum by the mucous membrane. It was removed completely, with some difficulty, without opening the rectum. The posterior wound was stitched up, and the patient made a good recovery.

Examination of the specimen: Microscopical sections of the wall of the cyst show that it was lined with glioma cells, and therefore must have been derived from the remains of the neurenteric canal at a late period of development.

Chronic Duodenal Ileus.

By J. P. LOCKHART-MUMMERY, F.R.C.S.

THIS case is published with an illustration in the *British Journal of Surgery*, January, 1922, p. 467.

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Section of Surgery.

SUB-SECTION OF PROCTOLOGY.

President of Sub-section—Sir CHARLES GORDON-WATSON, K.B.E.,
C.M.G., F.R.C.S.

Two Specimens of Diverticulitis of Pelvic Colon successfully removed by Resection and Anastomosis.

By Sir CHARLES GORDON-WATSON, K.B.E., C.M.G., F.R.C.S.
(President).

At the discussion on diverticulitis two years ago in this Sub-section, I showed a specimen of diverticulitis of the colon which I had removed by Paul's method after delivery of the loop outside the abdomen. The colostomy was closed a year later—during the war—by Mr. Lockhart-Mummery. This was followed by stenosis and a short-circuit operation.

It was generally agreed at that discussion that primary resection was seldom justified because in active inflammatory or obstructive cases the risks of anastomosis are very great. It was agreed that resection was the ideal treatment for those cases which simulate carcinoma in the absence of active inflammation or obstruction but that these cases seldom came under the knife at a suitable time.

On these grounds I have thought it worth while to show these two specimens—one removed by primary resection, the other by secondary—and to refer briefly to some points of interest. Both patients were aged over 60, and came for treatment on account of diarrhoea and offensive discharge, and loss of weight. In neither case did the sigmoidoscope help. In the first a tumour was felt and suspected in the iliac fossa. In the second, a constriction was diagnosed by a barium enema. In the first case the effect of resection was very striking; diarrhoea stopped at once, toxic appearances rapidly disappeared and the patient put on weight quickly. The patient is here to-night. She weighs 3 st. more than at the time of operation (a year ago) and is in every respect perfectly well.

In the second case, the patient was rather feeble and I did not like to risk a primary resection, so I performed a lateral anastomosis between the two halves of the pelvic loop. Unfortunately, this did not cure his symptoms of loose offensive stool, and a month later, as I found with the sigmoidoscope that both channels of the bowel were active, I decided to resect.

Sacculitis of Colon which gave rise to a Perforation of the Ascending Colon.

By HAMILTON DRUMMOND, F.R.C.S.Ed.

SPECIMEN shows lower end of ileum and proximal half of colon with about thirty well-formed sacculi all about the same size; none at the time of hardening in formalin contained any stercoliths. Many of them are very thin-walled, evidently composed only of atrophied mucous membrane and peritoneum. All sacculi appear on the side of the bowel between the mesocolic and the lateral longitudinal bands. A well-marked group of four sacculi are

seen in the region of the ascending colon. The sacculæ which gave rise to perforation is seen transfixed with a glass rod in the ascending colon.

History: D. C., aged 46, a healthy well-nourished woman, was admitted into hospital with the following history: Three days before admission, while walking outside, she felt a sudden pain on the right side and was only with difficulty able to get home. She was obliged to remain in bed and was unable to take food, but was not sick. She was unable to sleep on account of the pain which persisted up to the time of admission. She stated she had had two previous attacks of pain on the right side of a similar nature; the last was about two years ago.

On admission she looked ill; pulse 100, temperature 98'4° F. Tongue dirty. Abdomen showed tender area slightly above McBurney's point with rigidity. Remainder of abdomen normal. Diagnosis of acute appendicitis made.

Operation showed no active pathology in appendix. An acutely inflamed area in region of ascending colon showed induration and extension towards mesentery altogether about the size of a half-crown and very deeply congested. On examination of the colon in other regions multiple sacculi were seen, some of which harboured concretions the size of shot. As far as could be seen the sacculi were just as numerous on the right side as the left. The inflamed area was covered with separate Lembert catgut sutures and the abdomen closed.

Ten days later when all acute symptoms had subsided the abdomen was opened in the middle line and a more thorough examination of the colon made. From what could be seen the sacculi, which were distributed throughout the whole colon, appeared to be more numerous on the right side (ascending and transverse colon). The pelvic colon, which was short, was not easily examined. Lower end of ileum and proximal half of colon removed. Lateral anastomosis between ileum and transverse colon. Recovery straightforward.

This specimen is of unusual interest because the lesion (an acute inflammatory process in a sacculæ causing a localized gangrenous patch in the bowel wall) occurred on the proximal half of the colon and not the distal.

Primary Carcinoma in the Blind Bowel Five Years after Excision of the Rectum.

By J. P. LOCKHART-MUMMERY, F.R.C.S.

THE patient, a male, aged 51, first consulted me in December, 1916, with an early adeno-carcinoma in the rectum. The growth was low down in the rectum and had not involved the muscular wall. I performed a preliminary colostomy and a week later removed the entire rectum. He made a good recovery and resumed a very active life. He remained in excellent health until the middle of 1921, five years later, when he began to have a profuse discharge of clear mucus from the lower colostomy opening connected with the blind end of the sigmoid. At the same time a cystic swelling made its appearance in the lower part of the abdomen and he had some frequency of micturition. I operated upon him in September, 1921, and on opening the abdomen in the mid-line I found that the whole of the blind portion of the bowel below the colostomy was enormously distended and thickened, and that there was a tight constriction round it about 3 in. below the colostomy. I tapped the distended bowel and drew off quite a pint and a half of clear gelatinous mucus.

I then found on bringing the bowel up into the wound that there was a small carcinomatous growth on the site of the stricture. It was not possible to remove the whole of the lower segment of bowel so I resected about 6 in. of it containing the growth and brought the end of the lower segment out of the lower end of the incision. There were no signs of diseased glands or secondary growth anywhere.

On examining the portion removed, it showed a small growth causing an almost complete stricture situated just below the colostomy opening and about 10 in. above the position of the original growth removed five years before.

It seems to be impossible to consider this as a recurrence of the original growth as it was quite 10 in. above the position of the original growth and there were no glands or signs of recurrence elsewhere, either in the blind portion or in the abdominal cavity. I regard it as a second primary growth occurring in the same individual.

Such cases are rare, but are occasionally met with. Some years ago I operated on an elderly lady and removed an epithelioma of the anus. Three years later she returned to see me with a duct carcinoma of the breast and her breast was removed. She is still alive, five years later, and has no signs of recurrence of either of the growths.

I have also on three occasions seen two growths present in the same patient, one in the colon and one in the rectum. In these latter cases it is probable that the upper growth has been the primary focus and the lower growth is autogenous. No such explanation, however, will fit the case here described and one can only conclude that it was a primary growth on both occasions.

Adenomyoma of the Uterus invading the Rectovaginal Septum and the Pelvic Colon.¹

By W. ERNEST MILES, F.R.C.S.

THE ordinary fibromyoma of the uterus is comparatively innocuous, and, except when giving rise to excessive bleeding or from its bulk causing pressure symptoms, occasions very little trouble. It may become adherent to neighbouring structures such as intestine or omentum but never invades them.

A peculiar tumour, however, closely resembling in external appearance the fibromyoma but differing from it histologically by being composed of epithelial glandular elements in addition to smooth muscle and fibrous tissue, has for some years past been known to occur in the uterus. This neoplasm, termed adenomyoma, derives its importance from the fact that it is apt to adhere to adjacent structures and then invade them progressively. Adenomyomata usually occur as ill-defined non-capsulated growths in the uterine wall and may attain considerable size from cystic transformation of the epithelial glandular elements. In many instances, however, the adenomyomatous deposit takes the form of a diffuse infiltration of the uterine wall, the organ becoming thereby enlarged but otherwise preserving its natural shape. Microscopically, the growth consists of a ground substance of bundles of unstriped muscular tissue intermixed with fibrous tissue in which are imbedded epithelial glandular tubules closely resembling normal uterine glands. Scattered throughout the ground substance of muscular and fibrous

¹ Two specimens, together with microscopical and lantern slides, were shown.

tissue are gland-like spaces varying considerably in diameter. These are lined by a single layer of epithelium, which in the smaller glands is cylindrical in character. The large spaces, which are dilated glands, are lined by a cuboidal or by a flattened epithelium according to the degree of tension within the cyst. Each of the glands is surrounded by a mass of cells having oval vesicular nuclei which are identical with those found in the endometrial stroma. The origin of the epithelial glandular elements, which closely resemble the uterine glands in structure, has been the subject of much controversy. Some, von Recklinghausen for instance, hold that they arise in connexion with remains of the Wolffian body. Others believe that they originate from remains of the Müllerian duct. Others, again, notably Iwanoff, trace them to ingrowing processes of epithelium derived from the serous coat of the tumour (the serosal theory). Cullen, a recognized authority on adenomyoma uteri, considers that the majority of these uterine tumours are derived from the endometrium; but owing to the fact that identical tumours have been found in various parts of the pelvic structures apart from the uterus, he does not accept the endometrial origin in every case. Archibald Leitch, however, believes that they are always derived from the uterine glands and advances a reasonable explanation of the extra-uterine tumours. He considers that when the uterine glands begin to invade the uterine wall, they continue to do so through continuity of tissue; and explains the occurrence of detached foci by the breaking of the connecting chains by muscular contraction in attempts to extrude the growth from the uterus. He has observed that the gland tubules invade the muscular wall by insinuating themselves along the clefts existing between the muscular fasciculi, and concludes that, when the distal portion of a tubule becomes broken off during muscular contraction, the part of the gland so detached continues to exist and to grow as a separate focus. The gland tubules then worm their way through adjacent tissue and may implicate structures either anatomically, pathologically or temporarily attached to the uterus (Leitch's migratory theory).

Whatever be the source of the glandular elements in these tumours, without doubt the power of invading adjacent structures is due to them. Adenomyomata have been found elsewhere than in the uterus, for example, in the tubes, the round ligament, the broad ligament, the ovarian ligament, the utero-sacral ligament, the ileum and the umbilicus. Without discussing these extra-uterine adenomyomata which belong to the domain of gynæcology, I will confine my remarks to those that may invade the rectum or the pelvic colon and are of proctological interest.

(I) ADENOMYOMA INVADING THE RECTOVAGINAL SEPTUM.

Adenomyomatous infiltration of the rectogenital space and the rectovaginal septum is not uncommon. Cuthbert Lockyer has collected details of forty-seven cases from literature, and that list no doubt can be increased. As the infiltration usually involves the anterior wall of the rectum and produces a definite tumour in it, the differential diagnosis from other rectal neoplasms, particularly carcinoma, is important.

Gross Pathology.—The growth, varying in size from a walnut to a hen's egg, is situated between the anterior wall of the rectum and the cervix uteri below the level of the peritoneal floor of Douglas's pouch. Usually hard in consistence, it may be partly cystic. The tumour has no definite outline. When large it encroaches upon the lumen of the vagina as well as upon that of the rectum.

Per vaginam.—The posterior wall of the vagina at its upper part is bulged forwards by a hard fixed mass which also obliterates the posterior fornix. The mucosa is generally adherent but is seldom ulcerated. Ecchymoses are not uncommonly found scattered over the surface of the growth.

Per rectum.—The lumen of the bowel is encroached upon by a growth in its anterior wall. When the infiltration has not extended beyond the muscular coat, the mucosa is freely movable over it; but when the submucosa has become implicated, the mucous membrane is adherent. Even in those cases in which the mucosa itself has been invaded, ulceration seldom occurs, though a papillomatous condition may be produced which closely simulates the papilliform type of carcinoma of the rectum.

Histology.—Microscopically the growth, and the structures invaded by it, display the characteristic features of adenomyomatous tissue of the uterus. The neoplastic tissue, consisting of bundles of unstriped muscle, enclosing tubular glands surrounded by endometrial stroma, is found to extend through the whole thickness of the muscular walls of both vagina and rectum, into the submucosa or even into the mucosa itself. The invading tissue appears to displace rather than to destroy that of the part invaded.

Symptomatology.—Pain, which is deeply seated in the pelvis and is more or less constant, is practically the only symptom indicative of the presence of adenomyoma of the rectovaginal septum. Dyspareunia is not uncommonly complained of and defæcation is generally painful. These painful sensations are aggravated both during and after menstruation, an evidence that adenomyomatous deposits participate in the menstrual cycle and themselves menstruate. The ecchymoses so often seen on the vaginal surface of the tumour are also probably due to menstruation. Curiously, however, bleeding from the rectum is not a symptom of the disease although the growth itself is extremely vascular and bleeds profusely when incised.

Diagnosis.—The hard indurated nature of the growth and the fact that it infiltrates the wall of the bowel creates a suspicion of malignancy. When the rectal mucosa is firmly adherent, and especially if a papillomatous condition of the surface has developed, the resemblance to a malignant growth is even more marked. The fact, however, that there is an absence of surface ulceration, that there is no bleeding apart from the co-existence of internal piles, and that the pain varies in intensity during the menstrual cycle, should serve to establish the diagnosis. In all doubtful cases a piece of the growth should be removed for microscopical examination. When this is done the piece should be taken from the vaginal portion of the tumour because adenomyomata bleed freely when incised and the bleeding is more readily controlled in this situation than in the ampulla of the rectum.

Treatment is entirely operative. All those who have operated upon these cases agree that the uterus, together with the upper third or even half of the vagina, must in all instances be removed. There can be no doubt as to the advisability of this, because the uterus is probably always the seat of adenomyomatous change and if this adenomyomatous tissue is left behind the process may invade other structures. There is much difference of opinion, however, as to the best way of dealing with the invaded portion of the rectal wall. Some state that it is unnecessary to interfere with this as it ceases to grow after removal of the rest of the growth and eventually disappears. This view has been entirely disproved by a case that has recently occurred in the practice of Dr. Herbert Williamson. An adenomyomatous tumour of the broad ligament, which was adherent to and had invaded the rectal wall, was removed by

a surgeon in India who dissected the tumour away from the rectum but left the part of the growth which involved the wall of the rectum behind. When the patient came under Dr. Williamson's observation about a year later there was a large growth as big as a walnut in the rectovaginal septum. Others excise the segment of the bowel in which the portion of its circumference is involved. Lockyer, in his case, after removing the uterus and the upper part of the vagina, excised a considerable portion of the whole circumference of the rectum and established a permanent colostomy. Yeomans, of New York, believing that the growth originated in and was confined to the rectal wall, removed the entire rectum and brought down the sigmoid and sutured it to the anus, but he left the uterus behind.

I agree that it is necessary to remove the uterus together with the upper part of the vagina in every case, because we have pathological evidence that the disease starts from the uterine glands, whence the invasion proceeds downwards through the cervix and the posterior vaginal wall to the rectum. But it never appears necessary to carry out an extensive excision of the rectum because the growth is practically confined to the anterior wall of the bowel. That excision of the portion of the wall of the rectum actually involved is all that is needed in these cases is well illustrated by the following instance.

A lady, aged 34, with one child, was found to be suffering from a growth situated in the rectovaginal septum. The diagnosis of adenomyomatous infiltration was established by microscopical examination of a portion of the vaginal growth. On rectal examination a definite swelling was found to be situated in the anterior wall of the bowel at the level of the cervix uteri. The mucosa was intact but was closely adherent to the surface of the growth.

Operation, June, 1914: After opening the abdomen I tied both internal iliac arteries so as to ensure a bloodless field. The uterus was isolated as in the operation for panhysterectomy except that the tubes and ovaries were preserved. The upper part of the vagina was then freed from its connexions anteriorly and at the sides and divided completely across, below the growth in the rectovaginal septum. The uterus and the upper part of the vagina, together with the involved area of the anterior wall of the rectum, were then removed by an elliptical incision carried through the bowel wall. The rent in the anterior wall of the rectum was closed by means of a triple row of sutures. The patient made an uninterrupted recovery and remains well after the lapse of eight years.

Examination of the uterus after removal showed that in contour it resembled a normal but enlarged uterus. On section the uterine wall was found to be considerably thickened and divisible into two parts—an outer part resembling normal uterine muscle, and an inner presenting a coarsely striated appearance. The posterior wall of the vagina and the attached portion of the rectal wall were considerably thickened, and when examined microscopically were seen to be infiltrated, as far as the submucous tissue, by typical adenomyomatous tissue.

(II) ADENOMYOMA INVADING THE PELVIC COLON.

This is apparently an extremely rare condition. So far as I have been able to ascertain only four examples of the kind have as yet been seen, one of which I have had the opportunity of observing. This case occurred in the practice of my colleague, Mr. Cecil Rowntree, who has kindly allowed me to give details of it.

I was present when Mr. Rowntree opened an abdomen and found that the pelvic colon near the middle of the loop was adherent to the posterior surface of the uterus. The part of the bowel which was adherent appeared to be constricted and puckered, thus closely resembling, in outward appearance, the whipcord type of carcinomatous

growth which is so commonly met with in this situation. The appearances were those of a primary carcinoma of the pelvic colon secondarily invading the uterus, to which it had become adherent. Subsequent examination, however, of the excised portion of the bowel revealed that the muscular as well as the submucous coats were infiltrated by adenomyomatous tissue.

This case is an example of direct invasion of the pelvic colon by an adenomyomatous area in the uterine wall to which it had become attached. Extra-uterine adenomyomata sometimes invade the rectum as is shown by the case that came under Dr. Williamson's observation. In that case an adenomyoma, situated in the broad ligament, was adherent to and had involved the wall of the rectum. Two examples of extra-uterine adenomyomata have occurred in my own practice which, though they did not actually involve the pelvic colon at the time they were removed, would in all probability have done so within a short time. In one instance when performing abdominal hysterectomy I noticed that the left utero-sacral ligament was considerably thickened. The thickening of the ligament extended backwards as far as the terminal portion of the pelvic colon but did not actually invade it. Microscopical examination of the thickened ligament revealed the presence of adenomyomatous infiltration, and without doubt, had I opened the abdomen at a later period, the bowel would then have been implicated. In another case of abdominal hysterectomy I found an adenomyoma in the left broad ligament lying in contact with, but not involving, the bowel, though no doubt eventually it would have done so.

Symptomatology.—The only symptoms of adenomyoma implicating the pelvic colon are pain in the left iliac region with menstrual exacerbations and signs of increasing obstruction in the lower bowel.

Diagnosis may be only possible after exploratory laparotomy and even then it may not be easy to decide whether the growth in the bowel is carcinomatous or not without removing a piece for microscopical examination.

Treatment.—Local excision of the infiltrated part of the bowel is all that is necessary. Mr. Rowntree, in his case, was able to extirpate the disease by removing a portion of the circumference of the gut. In more advanced cases it may be necessary to resect the involved segment and then restore the continuity of the colon either by end-to-end or by lateral anastomosis.

There is ample evidence, then, that adenomyomata both of the rectum and of the pelvic colon occur with sufficient frequency to warrant their inclusion among the tumours that are met with in these localities; and, if only the possibility of their occurrence be borne in mind, many more examples of this interesting type of tumour will be brought to light.

Case of Malignant Polypus of Rectum ; Ligation and Removal ; Subsequent Treatment with Radium ; Signs of Local Recurrence ; Perineal Excision of Rectum.

By LIONEL NORBURY, F.R.C.S.

PATIENT, S. J., male, aged 43. Admitted St. Mark's Hospital, January 10, 1920. History of piles for several years; worse one year; bleeding and severe prolapse.

Examination: Internal hæmorrhoids and a large fibrous polyp growing from right posterior wall of lower part of rectum.

44 Norbury: *Perforating Diverticulitis of the Pelvic Colon*

Treatment: Hæmorrhoids and polypus ligatured and removed, January 15, 1920. Polyp had short pedicle—base rather indurated. Polyp examined by Professor Shattock, who reported "squamous-celled carcinoma with highly malignant cells."

January 29, 1920: Radium 100 mg. shielded and applied for twelve hours. Similar application March 13, 1920.

Readmitted to hospital, April 17, 1920: History of pain on defæcation fourteen days. Marked induration at site of base of polyp, suggestive of recurrence.

April 22, 1920: Colostomy followed by perineal excision of rectum. May 6, 1920: 5 in. of bowel removed under spinal anæsthesia.

Pathological report: "Small ulcer present; base lies on muscular coat surrounded by much inflammatory change. In the region of the ulcer the mucosa has undergone metaplasia and the cells have assumed a squamous form."

Case of Perforating Diverticulitis of the Pelvic Colon, with Vesico-colic Fistula, treated by Suprapubic Cystotomy and Transverse Colostomy.

By LIONEL NORBURY, F.R.C.S.

PATIENT, J. B., male, aged 57. Admitted to hospital, June, 1920, with history of passing wind and fæces in the urine. He had been told that he had an inoperable carcinoma of the bowel involving the bladder and that nothing could be done. Mass felt, left iliac region. Cystoscopy attempted but urine too thick to obtain a proper view.

Exploratory laparotomy: Large inflammatory mass, low down in pelvis, left side, involving pelvic colon and bladder. Condition diagnosed as perforating diverticulitis. Subsequent suprapubic cystotomy and drainage of bladder, and transverse colostomy.

Admitted to hospital again May 23, 1921, for observation. Colostomy acting well. No cystitis.

Sigmoidoscopy: Pelvic colon found to be much narrowed and no attempt made to close colostomy.

Case of ? Diverticulitis of Pelvic Colon, with Vesico-colic Fistula.

By LIONEL NORBURY, F.R.C.S.

PATIENT, W. H., male, aged 62. History of constipation for many years. One week before coming to hospital he felt "something give way," as he described it. Acute epididymo-orchitis of right testicle followed and he began to pass fæcal matter and wind in the urine.

Examination: General condition good. Mass felt in hypogastric region which disappeared to a great extent after an enema. Refused surgical treatment. Condition regarded as one of perforating diverticulitis.

Points of interest: (1) History of prolonged constipation; (2) history of the sensation of "something giving way."

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Section of Surgery.

President—Mr. RAYMOND JOHNSON, O.B.E., F.R.C.S.

Some Points in the Technique of Bone-grafting, with a Special Reference to Bridge-grafts.

By C. MAX PAGE, D.S.O., M.S., and G. PERKINS.

(ABSTRACT.)

[This paper is published in full, with illustrations, in the *British Journal of Surgery*, April, 1922, ix, pp. 540-552.]

THE subject was opened by a consideration of the established position of bone-grafting in surgery. After outlining the conclusions of most recent observers it was pointed out that some parts of the life history of bone-grafts in adult man remained obscure, or at any rate, matter in dispute.

From an experience based on fifty cases operated on for ununited fracture the authors put forward certain conclusions which might help to settle the points at issue. They proposed to base their conclusions on the results of the bridge-grafts in their series: a bridge-graft being defined as a bone implant which fills in a definite gap in the original bone and ultimately reconstitutes a part of it.

Particular stress was laid on the interpretation of the radiograms of the cases which had proved a failure.

Some forty X-ray plates and diagrams were shown and the following conclusions formulated:—

(1) From the evidence of radiograms it would appear that bone implants possess and retain a vital activity for a definite period independently of the tissues of the host.

(2) Boiled beef bone cannot be used successfully as a bridge-graft, though it is of value if employed as an internal splint.

(3) For successful bridge-grafting the graft should be autogenous. The most satisfactory source of the graft for average purposes is the tibia. The graft should contain a sufficiency of compact bone to give it strength and as much cancellous bone as is convenient. It is in relation to the cancellous part of the graft that firm union with the host bone takes place, and that new bone is laid down, after implantation, to thicken the graft. The presence or absence of periosteum on the graft does not appear to affect its vitality initially or its later enlargement in response to function.

(4) Two steps are necessary in the preparation of the host fragments:

- (a) The removal of the sclerosed ends till healthy vascular bone is exposed;
- (b) mobilization of the fragments so that they can be brought into their normal alignment without tension.

(5) The most important factor governing success is the firm fixation of the graft to both extremities of the host bone.

(6) The method of fixation of the graft found most satisfactory for the average case consists in intramedullary pegging at one end and in an inlayed splice at the other.

(7) Fractures of a bridge-graft fall into two categories: (a) Apparent fracture at the junction of the host and graft: these so-called fractures occur

at an early stage and are due to the weakening of the implanted part of the graft by absorption which results when it is not in firm contact with a healthy section of host bone; (b) fractures in the free part of the graft which occur from inadequate support of the limb or its exposure to undue strain after the implanted ends of the graft have become firmly incorporated with the host bone.

(8) Post-operative treatment may be divided into three clinical stages: (a) During the first six weeks absolute immobilization of the part involved is necessary; during this stage the graft should unite firmly with the host bone. A radiogram taken at this period will show whether or not the graft is successful; if implantation of the graft has been so carried out that vital continuity between it and the host bone becomes established, no space is seen between the graft and the host bone in the picture; if fixation has not been firm a clear area is seen between the graft and the host bone, and the related part of the graft will become gradually absorbed and finally give the appearance of being fractured where it is in contact with the host bone. (b) After six weeks, partial function of the limb involved is allowed, adequate splint support being supplied to prevent fracture from undue strain. (c) When the graft in response to function has sufficiently thickened, all support is removed, and the return of full nutrition and activity in the limb is assisted by physiotherapy.

(9) The ease with which successful bridge-grafts can be carried out varies in different bones. The radius gives the largest proportion of successes. The upper third of the ulna and the lower third of the tibia have been found to be the most difficult to bridge.

(10) We have attributed most of our failures to unsatisfactory fixation of one end of the graft to the host bone.

(11) Post-operative infection of the wound is not incompatible with the survival and growth of a bridge-graft.

We should like, in conclusion, to acknowledge our indebtedness to Dr. R. J. Reynolds and Dr. W. P. Tindal-Atkinson for their invaluable help in taking the radiograms upon which this study has been based. We are also much indebted to Mr. G. W. Heckles for the careful records which he kept of the earlier cases of the series referred to.

Mr. JENNER VERRALL said that his own experience largely coincided with that of Mr. Max Page. He also had had cases in which, in the presence of mild sepsis, sequestra had been thrown off from a graft without its vitality or success being impaired. In a number of cases in which a previous graft had undergone aseptic absorption he had found a positive Wassermann reaction and he now made a routine of having this investigated. With reference to Mr. Page's remarks on the use of the fibula for grafting the humerus, he was of opinion that, wherever the humerus would not be unduly shortened thereby, it was better to bring the bone-ends together and fix with a small intramedullary graft, but when long gaps had to be covered the fibula was most useful, as it fitted naturally inside the humerus, and he had never seen any ill result to the leg from the removal of the upper half of the fibula. He called attention to the value of free-ended grafts and instanced the transplantation of the upper end of the fibula to replace the upper end of the humerus in flail shoulders with strong deltoids, a procedure he had twice carried out at the suggestion of his colleague, Mr. Elmslie. Grafts of the upper third of the ulna were notoriously unsatisfactory and he would call attention to the frequency of subluxation forward of the head of the radius in these cases, leading to increased instability. This could be obviated to a certain extent by putting up such grafts in full supination. It was not his practice to allow movements of adjacent joints as early as six weeks after operation, as he mistrusted the vitality of the graft at so early a date, but he admitted that Mr. Page's results seemed to justify such a course.

Section of Orthopædics.

President—Mr. E. LAMING EVANS, C.B.E., F.R.C.S.

Two Cases of Deformity of the Hip.

By R. C. ELMSLIE, O.B.E., M.S.

Case I.—Boy, aged 5, who has limped from the time of beginning to walk at the age of 1 year and 6 months. The left lower limb is 1 in. short, the great trochanter being raised; movements of the joint are free. The head of the femur can be felt in its normal situation, but, on flexing and adducting the hip, it appears on the dorsum ilii. Trendelenburg's sign positive. X-ray photograph shows a right-angled deformity of the neck, an absence of ossification of the head of the femur, and an enlargement upwards of the acetabulum. This is apparently a case of subluxation of the head of the femur with coxa vara and defective ossification of the head of the bone. The condition is possibly congenital, possibly due to an early arthritis, as there is a history that the boy has suffered from pneumonia at the age of 4 months, and during the illness had some trouble in the left leg. The case is of interest, in view of a recent communication, by Calot, in *La Presse Medicale*, January 14, 1922, in which he suggests that coxa plana is really congenital subluxation of the hip.

Case II.—Girl, aged 12, with coxa vara of the cervical or adducted type, showing a positive Trendelenburg sign, a diagnosis from congenital dislocation, apart from radiographic appearances, being very difficult.

Case of Osteo-chondritis of Head of Femur.

By E. LAMING EVANS, C.B.E., F.R.C.S. (President).

PATIENT, a girl, E. W., aged 11, has been treated in country surroundings by hip extension and immobilization as for tuberculous disease of the hip for four and a half years. All movements of the hip are free. Shortening $\frac{3}{4}$ in. Trochanter raised $\frac{3}{4}$ in. above Nélaton's line. X-ray shows flattening and spreading of the capital epiphysis upon the femoral neck. It is a case of osteo-chondritis. In addition, there is a clearly-defined centre for the lesser trochanter of increased density, suggestive of the radiographic appearance of the tarsal scaphoid in Köhler's disease. The centre of the lesser trochanter on the right side has not yet appeared. It is suggested that the osteo-chondritic affection has exceeded the usual limits and extended to the region of the lesser trochanter.

Case of Recurrent Subluxation of both Knee-joints (Snapping Knees) in a Baby.

By H. A. T. FAIRBANK, D.S.O., M.S.

PATIENT, a female infant, aged 10 months, for three months has been noticed to snap right knee in semi-flexed position on many occasions. Similar snap in left knee on two occasions. Voluntary displacement takes place in an outward and slightly forward direction. In semiflexion passive displacement of tibia is possible, particularly in outward direction; tibia slips back with snap. No hyperextension of knees. Other joints normal. It is proposed to treat case with some simple splint to prevent child from subluxating the knees, and to order massage.

Case of Infantile Monoplegia of the Left Leg.

By P. JENNER VERRALL, F.R.C.S.

PATIENT, a girl, aged 14, whose disability is said to date from infancy. I ask for opinions as to whether the case is one of an upper motor neuron lesion with secondary degeneration of the lower neuron? or of poliomyelitis and polio-encephalitis affecting the same part? I regard the former as the more probable.

In the discussion various opinions were expressed, but, on the whole, the case was regarded as more likely to be an example of poliomyelitis.

Case of Multiple Loose Bodies in the Knee-joint.

By W. ROWLEY BRISTOW, F.R.C.S.

RADIOGRAMS of the knee-joints of a patient from whom thirty-three loose bodies have been removed from the right knee are shown, together with the loose bodies and microscopic sections of one of them and of the synovial membrane of the knee.¹

Case of Dislocation of Patella and Contraction of Knee.

By H. TYRRELL GRAY, M.Ch.

PATIENT, a girl, aged 5. Dislocation of right patella since she began to walk. At first she was able to put it in and out at will, but since the other leg has been in plaster for congenital dislocation of hip, the right patella has been permanently out. Before being in plaster, child was able to hobble about with a twisting movement of the right leg, but now when she tries to walk she falls down, the knee giving way beneath her. Never complains of pain. Holds foot everted and knee flexed slightly. No deformities in family. There is permanent external dislocation of right patella, with knee-joint in position of

¹ This case will be fully reported elsewhere.

flexion 120° and genu valgum. Full extension impossible. Right foot everted with marked external rotation of right tibia at knee-joint: condyles of femur project forward prominently. Right patella small, lying behind external condyle, and impossible to reduce. No marked muscle wasting. Other joints normal. Present condition of left hip, which had been dislocated, is now satisfactory. X-ray shows head of femur in acetabulum.

Apophysitis of Tibia.

By PAUL BERNARD ROTH, F.R.C.S.

PATIENT, a boy, aged 13, with "Schlatter's disease" of both tibiæ, which had subsided entirely with no other treatment than rest in bed for six months.

Sir ANTHONY BOWLBY, in discussing this case, called the attention of the Section to the fact that the condition was well known to Sir James Paget, and described by him in "Studies of old Case Books," published in 1891.

Section of Orthopædics.

President—Mr. E. LAMING EVANS, C.B.E., F.R.C.S.

DISCUSSION ON THE USE OF REMEDIAL EXERCISES AS APPLIED TO SCOLIOSIS.

Mr. D. McCRAE AITKEN.

I KNOW of only two main uses of exercises :—

- (1) To increase mobility by stretching tight structures.
- (2) To establish good habitual positions by training the patient to use her muscles properly.

I firmly believe that deformity is never caused or cured by active exercise. It is the habitual positions which are adopted during rest and relaxation of the body which are powerful for good or evil.

I think we may say that no surgeon nor gymnast ever cured a case of scoliosis, though the patient may cure herself under the guidance of a gymnast. The surgeon directing the case has to attack the patient's brain, not her back. He must therefore chose a gymnast who is a real teacher, not merely a good exponent of gymnastic movements.

If the deformity is at all rigid so that the patient cannot by voluntary effort improve her position, the first stage must be a series of active trunk movements to loosen the joints and secure greater mobility. If the deformity is passively mobile, but the patient cannot voluntarily correct, a stage of correction by jackets which allow movement in the direction of correction may be a necessary preliminary stage. If the deformity is mobile and the patient can voluntarily correct and hold the correction, the time has come for re-educating the patient.

It is essential that the patient should understand the nature of the deformity. A child with scoliosis does not feel crooked, therefore the first thing is to make her see that she is crooked by the use of mirrors. Next, show the patient how, by taking certain positions the deformity can be modified or entirely corrected. The next stage is to re-educate the muscle-sense by directing the patient's attention to her own muscle sensations when assuming the correct position. Then give exercises consisting of simple movements which stretch or relax tight structures or cause active shortening of relaxed structures.

Arm movements are of very little use. Thigh movements influence the lumbar region directly through the psoas, indirectly through the abdominal muscles, erector spinæ group and quadratus lumborum.

When by means of the performance of simple movements the patient has learnt muscle sense and can feel the corrective action in the trunk, the next

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stage in treatment is to make her apply these movements in the ordinary actions of every-day life—lying, sitting, standing, and walking.

Long tables of exercises only puzzle the patients. They should be taught to concentrate their attention on correct positions of sitting, standing, and lying.

My experience is that the surgeon and gymnast are entirely at the mercy of the patient. No amount of exercise is of any real use unless the patient takes the trouble to apply what she is taught in the positions she habitually assumes in ordinary life. Results in private practice are therefore better than those in hospital practice, for both the patient and parents are more likely to make an intelligent use of re-education by exercises.

Dr. CLAYTON

drew attention to the importance of teaching a habit of correct posture and to the difficulty of accomplishing this on three to five treatments a week, especially with younger children who would not perform home exercises properly. He divided scoliosis into three classes—postural, borderline and fixed. He advised that the back should be bare for the exercises in all cases, that each patient should have individual attention until she had learnt to stand and sit properly, but that postural cases could then be taken in classes.

As regards results he found that exercises did not cure fixed scoliosis, but that they tended to prevent it from progressing; that postural cases could be cured, but that in fixed cases exercises only improved posture, strengthened the patient, and developed the chest. In most cases he had noticed that the posture became worse when exercises were given up, and so he advised that exercises should be continued until adult life, either at home or in a gymnasium.

Dr. BARRIE LAMBERT.

Are we not teaching students a great deal which is never required by them in after life, and in which many of us have little faith?—namely the same exercises which I learnt in Stockholm fifteen years ago, based on the then current knowledge of orthopædics. How many of us, whether orthopædic surgeons or physiotherapists, believe in the efficiency of the so-called “pressure” exercises for fixed curves? Do the orthopædic surgeons advocate and order these exercises? Cases of scoliosis can be divided into two main groups—namely, those with postural curves, and those with fixed curves, and there is a small intermediate class. There is general agreement that children with postural curves, of whom there are large numbers, do not require individual remedial treatment; they are more suitably treated by regular exercises in school given daily with attention to their hygiene, &c.; to treat them individually is a waste both of money and of the children’s school time. Neither orthopædic surgeons nor physiotherapists advocate gymnastic treatment for severe fixed curves. There remain therefore the small number of intermediate cases, and these constitute the difficulty, for I am uncertain whether, even then, the stereotyped exercises which we teach are those from which we are likely to get the best results. Some agreement must now be reached on these points, and I hope that as a result of this meeting orthopædic surgeons and physiotherapists will go into the syllabus of exercises for scoliosis taught by the Chartered Society. Further, that they will ask the Chartered Society to meet them on this question, to decide whether or no they consider some of the exercises taught

to be of value. I specially emphasize the great danger of wasting the children's time in these remedial gymnastic clinics unattached to hospitals. In these clinics a large proportion of the children who are there should not be there at all, and those who are there are frequently not receiving the proper treatment. I therefore suggest: (1) that we should reconsider the whole question of the treatment of scoliosis by remedial gymnastics, and that a committee should be formed to go into the syllabus; (2) that we should none of us advocate any orthopædic remedial treatment unless it is part of an orthopædic system given under the supervision of an orthopædic surgeon; and that clinics which are left entirely to the masseuse, with occasional supervision by the school medical doctor, should not be advocated, nor should surgeons send children there for treatment.

MR. PAUL BERNARD ROTH

considered the system employed in this country frequently failed because it was complicated and based on the principle of "undoing curves." Instead of it he advised a simple system consisting of a few standard exercises based on the principle of keeping the patient always in the "best possible position," regardless of the particular curve present. Not only should it be maintained during the performance of each exercise, but in the intervals between two exercises while the patient was resting. From the moment this position became habitual, all tendency to grow crooked ceased. He also emphasized the importance of attention to clothing, re-education of the patient's muscular sense, and specially the daily personal supervision of the surgeon, who must himself take part in the treatment.

MR. R. T. TIMBERG.

I still adhere to my old faith in the principles and practice taught by the Swedish school in the treatment of scoliosis. I look upon them, not only as a solid foundation for the rational treatment of spinal curvatures, but as a pretty complete structure that has stood the test of time. Many of the more specialized methods of treatment appear to be merely modifications of long used measures elaborated in certain directions.

One of our first aims is to restore mobility to the affected part of the spine, this being achieved by trunk rotations, bendings and rollings carried out in positions that localize the effect of the movement to the desired section of the spinal column. The mobility and development of the thorax is another aim.

As to the principle of making the patient concentrate his efforts on walking in an extended upright position, we are carrying out this in our numerous balance exercises, and I sometimes employ the self-correction belt invented by Kjolstad as an aid in the performance. As to the controversial subject of the use of pressure in the treatment of scoliosis, my view is that manual pressure on the convexities of the curvatures should be employed with the various exercises. It most usefully supplies a fixed point of vantage for the action of the muscles in the desired section of the spine. To obtain greater effect I often make use of Oldevig's straps. The use of plaster jackets does not render exercises unnecessary; on the contrary, it adds to their need owing to the prolonged interference with the use of the patient's spinal muscles. Home exercises form a very important supplement to the treatment.

Dr. CARL WESTMAN.

I suggest that if one knew more about the causative factors of the individual cases of scoliosis the results would be better.

The method of applying remedial exercises in actual treatment might be improved in several ways:—

(1) The exercises are applied too infrequently, so that what is done during the hour of treatment is undone during the rest of the day. The patient should be encouraged to carry out exercises at home.

(2) More attention should be paid to the active co-operation of the patient in treatment by exercises.

(3) Many of the exercises employed at present are not sufficiently isolating in their action. The remedy lies in the adoption of Oldevig's strap exercises, which enables one to isolate the muscular work to almost any part of the spine. It is unfortunate that these exercises are not included in the syllabus of the Chartered Society.

(4) The old teaching that the muscles on the concave side of a curve are necessarily contracted and strong, whereas those on the convex side are relaxed and weak, is not always in accordance with facts.

(5) More attention should be paid to the performance of exercises on one leg for the purpose of redistribution of the body weight in cases in which, for some reason or other, one leg has been favoured at the expense of the other.

Section of Surgery.

SUB-SECTION OF PROCTOLOGY.

President of Sub-section — Sir CHARLES GORDON-WATSON, K.B.F.,
C.M.G., F.R.C.S.

DISCUSSION ON THE AFTER-RESULTS OF COLECTOMY (PARTIAL AND COMPLETE) PER- FORMED FOR COLON STASIS.

Mr. ETHELBERT FLINT (Leeds).

THE object of the following statements (which for the most part embody Sir Berkeley Moynihan's opinions) is to show that only in a few selected cases of intestinal stasis can success be expected to follow colectomy; and by colectomy I mean hemicolectomy. Complete colectomy is an operation which has never commended itself to Sir Berkeley, for reasons to which I will refer later.

The attitude we have come to assume towards stasis after a fairly large experience both operative and otherwise, is that it is not commonly a disease in itself; but that it is part of a general nutritional or metabolic disorder, probably arising from a deficient and ill-balanced diet; the effect of which is felt by the colon in common with many of the other organs of the body—more especially of the endocrine system. This view is held (tentatively it is true) partly because the results of surgery are on the whole unsatisfactory since it does not seem to reach the root of the disease, and partly because recent work seems to indicate a nutritional causation, which can be dealt with by a less drastic and more efficient method of treatment.

You will see from the statistics that there are sixty-nine cases of colectomy. To these may be added eighteen of my own in which the results, so far as I have been able to obtain them, agree in the main with Sir Berkeley's. In my series there were no deaths, so that we have seventy-eight cases with a mortality of 2.5 per cent.

With regard to the colon, it has been said by Metchnikoff and Sir Arbuthnot Lane that it is a degenerate structure, one which mankind would be as well, if not better, without. As our knowledge of the functions of the colon is very incomplete, this is a bold statement to make. Sir Arthur Keith has pointed out that the glandular structure put into a mass would form a gland larger than the pancreas, and the muscular tissue a muscle bigger than the biceps of a blacksmith, and he remarks this is not the sort of thing one would expect in a degenerate organ. I might add that the blood supply is on so lavish a scale that it is almost impossible to render it bloodless during surgical

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operations: and that this again points to some very real usefulness on the part of this organ.

The colon is therefore apparently endowed with some important functions, one being that of the absorption of fluid. After removal of the whole colon Nature sees that the body still gets its supply of water, this function being taken on presumably by the ileum. This is, I believe, the reason why it is so common to get a return of constipation after colectomy, for as the fundamental cause of stasis affects small as well as large intestine, we merely shift the trouble back; either water must be lost in diarrhœa, or it is retained with inspissation of the fœces in the ileum. An attempt has been made to get over this damming up in the lower ileum by retaining the ileocœcal valve; with what success I cannot say, as I have never seen it practised. But one would expect it to make little difference in the end, for whether the valve be retained or not the patient must absorb water, and if he cannot absorb it from the large intestine owing to its removal he must do so from the ileum, and so the inspissating process is continued in the small intestine.

Removal of an important organ, although diseased, must confer some compensatory benefit upon the patient, if such an operation is to be justifiable. I am afraid this is exactly what one cannot claim in the greater number of cases of colectomy for stasis.

When an operation, performed by skilled hands, produces complete cure one is entitled to claim that surgery is the proper treatment; operations in this category are, amongst others, cholecystectomy and choledochotomy for gall-stones, partial gastrectomy for gastric ulcer, gastro-enterostomy for duodenal ulcer, &c.

When, however, a large percentage of cases, and indeed, the larger percentage, as in the case of colectomy for stasis, is shown, at the best, to be only partially relieved we may claim that that particular surgical procedure is not only wrong, but probably founded on a false conception of the pathology of the disease, and it becomes necessary to discover where we have gone wrong.

Sir Berkeley Moynihan's results show quite clearly, that his successful cases were suffering from a chronic mechanical interference due to constricting bands across the ascending colon, usually in the neighbourhood of the hepatic flexure; and microscopic examination revealed an alteration in the neuromuscular mechanism of such a nature as to render the muscle less able to move on its contents. The colon and cœcum were always voluminous, thick, soggy and full of pultaceous fœcal material. The partial successes and failures only exceptionally showed this condition and probably represent those cases in which changes had gone on to extreme degrees in other organs, the liver for example.

One of the strongest arguments against colectomy for stasis is the scepticism with which it is regarded by most surgeons. It is certainly done with less frequency now than three or four years ago. When surgeons of the first rank believe they have discovered a sound operation the number of cases upon which they perform it tends to increase progressively in each succeeding year. Though colectomy had its vogue some few years back, it is less frequently practised now. This is certainly the case in Sir Berkeley Moynihan's clinic, though the type of patient upon which he used to perform the operation is seen by him quite as often as before.

The same view is expressed in another way. Many surgeons are forsaking colectomy for colopexy; not, I think, entirely on account of the greater

severity of colectomy, for the mortality is after all not more than 2.5 per cent.; but mainly, I believe, because of the unsatisfactory results they obtain. I might add, that in my opinion colopexy is no more likely to survive than colectomy; indeed it may suffer an earlier demise, for there can be little sound sense in fixing an organ which is naturally mobile.

Sir Berkeley Moynihan is inclined to attribute his failures to the removal of the colon, when in reality it was only one of many diseased parts, the whole being what is now called "deficiency disease."

Recently a great deal of work has been done in relation to this problem. McCarrison, especially, has produced very convincing evidence showing gastrointestinal lesions strikingly similar to those described by Sir Arthur Keith in intestinal stasis. Further evidence is forthcoming from the deficiently-fed prisoners of war, who presented symptoms closely allied to those of the ordinary civilian case of chronic intestinal stasis, and also showed X-ray evidence of stasis. I have seen several of these cases amongst pensioners. More experimental work is needed to establish the identity of deficiency disease with many of the cases of so-called colon stasis, but what has been done points strongly to such identity.

It is hardly necessary to multiply instances of ill health of the gastrointestinal type due to deficient diet; it is at least possible that the effects of such a diet acting over many years, and especially I believe in the earlier years of life, may be responsible for some colon stasis cases, particularly when we consider that very similar results have been experimentally produced.

No doubt a time arrives eventually when the morbid processes have produced changes so profound as to have become unalterable, and only then is it proper to do colectomy; but unfortunately the pathological state is not confined to the colon, and so only under special circumstances will colectomy give satisfactory results. I think this is the explanation of the success achieved in the first group of Sir Berkeley's classification—where we have the soggy, obstructed type of cæcum—a veritable cesspool—the removal of which may quite well be ridding the patient of his principal area of disease; it may also explain the poor results in patients who do not present this type of cæcum. In this case there is less of a focal character about the disease.

Is it possible to predict the state of affairs before operation? I think it is in most cases. All types present certain symptoms and signs, which it is unnecessary to enumerate; they are familiar to everyone. But the patients who will receive benefit from colectomy have certain distinguishing features. They are more obviously poisoned people, as evidenced by marked wasting, anæmia, outbreaks of septic skin conditions, low blood-pressure, fœtid breath, &c., and locally one can feel the distended "squélchy" cæcum; X-rays will show stasis of the barium meal for forty-eight to ninety-six hours in the cæcum and ascending colon, its progress beyond that point being very slow and giving the impression that it has been held up by some obstruction. These are the features which were more or less common to the class coming under the heading of soggy, obstructed cæcum and giving good results.

When stasis exists, but X-rays show barium in the descending colon and sigmoid within this same time limit, I believe colectomy is never indicated. Indeed the cause may be a very simple one as illustrated by two of Sir Berkeley Moynihan's cases. In two cases of grave intestinal stasis sent to him for colectomy during the periods covered by these statistics the radiographs showed a general distension of the whole colon, with greater enlarge-

ment of the sigmoid and rectum, and a delay of many days in emptying. Both cases were cured at once by stretching the sphincter. In one case a chronic fissure had been present for five years; defæcation was so painful that it was almost intolerable. Evacuation was then a protracted and painful procedure. Examination of the rectum had never been suggested, though medicinal treatment had been varied and drastic. The sphincter was so tight that examination was impossible without torture. An examination of the rectum and anus should often be made in the absence of any indication, and always if there is the slightest suggestion as to its necessity; it should always be infinitely gentle. If it is prevented by an excessively tight sphincter no effort should be made to overcome the muscle which is on guard against attack on some diseased condition. Our diagnosis is then made not by the examination but by the urgent resistance to it. An examination under gas is necessary. In this case it revealed an old hard fissure, the sphincter was stretched, the stasis cured and $4\frac{1}{2}$ st. added to the completely recovered patient in six months. No case of colectomy for stasis was ever so speedily and completely successful.

The loose, floppy colon associated with stasis in the condition called visceroptosis is not likely to be cured by colectomy, though it is for this state of affairs that colectomy is often done.

The mere presence of a loosely attached colon is no justification for assuming that gastro-intestinal symptoms arise from it. I have examined hundreds of cases both at operation and in the post-mortem room, and have found that in at least 75 per cent. a length of mesentery of $1\frac{1}{2}$ inches or more is present—even in cases in which during life there has been no stasis. Moreover, the cæcum is rarely found in the true pelvis—it often appears to be when inspected through an operation wound, but closer observation will show that it has passed forwards and inwards—and is apparently swinging on the ileocolic vessels as an axis. When one remembers that the limited space in the true pelvis is occupied by many coils of small intestine, by the rectum and by the bladder, which is rarely in the collapsed state for long, and in the female by the uterus, &c., it would be surprising if so voluminous a structure as the air-containing cæcum frequently entered the pelvis.

I hold that viscera receive support from the abdominal wall, not from their mesenteric attachments; and loss of tone in these muscles is often only a part of the same process as that which produced the stasis. It is easy enough to drag the cæcum through an abdominal wound and show peritoneal lines of stress caused thereby, but with the viscera inside the abdomen I have not been able to get the room necessary to develop these stressing forces.

The operation, upon which these reports of colectomy are founded, has always been of the same type—viz., hemicolectomy. The last 4 to 6 in. of the ileum together with the cæcum, ascending colon and proximal half of the transverse colon were removed, the cut ends crushed in a Parker's or Payr's clamp and invaginated with a Pagenstecher suture at first around the clamp, and after slipping this out and tightening the suture, it is continued back, picking up the serous coat to the point at which it was started, drawn up and tied, acting as a purse-string suture. A lateral isoperistaltic anastomosis was made close to the inturned ends, so as to leave no cul-de-sac. No drainage has ever been used, and the wounds have always healed by first intention. Any infection which is said to occur in these cases is from the opened intestine, and is therefore preventable.

One precaution is taken, especially with a view to preventing adhesions

and that is first of all to separate the omentum from the part of the transverse colon to be removed, reproducing the foetal state in fact, and at the end of the operation it is shrouded around the anastomosis and invaginated bowel ends.

The immediate results as regards the action of the bowels was varied. Sometimes a normal type of evacuation was set up at once, sometimes a rather loose motion was produced, and again in other cases the bowels were not moved without aperients. The ultimate result in most cases which did not progress favourably, was a return to obstinate constipation. There was no case of prolonged and uncontrollable diarrhœa, though there was a loose motion in two cases.

Complete Colectomy.—This is an operation which we have hardly ever practised in Leeds except after that very unscientific operation, ileo-sigmoidostomy. Holding the view, as we do, that the main seat of disease is on the right side, we have never leaned towards the performance of complete colectomy. We never see the soggy state on the left side as it occurs in the cœcum. Indeed, usually the intestine here appears quite normal, the spastic state spoken of by some surgeons does not seem to us to be anything beyond what is normally seen.

It is a significant point, indicating, I think, that the colon is only a part of stasis disease, that most of the cases I have seen after complete colectomy have had constipation as obstinate as before. I particularly emphasize this point, because it is obviously unfair to blame the remaining half of the colon if constipation persists or returns after hemicolectomy, when it is so common to see it after removing the whole of the colon.

If a return of constipation were the worst to be feared after complete colectomy, it would not be a very serious criticism to level at it; but there are at least two infinitely more severe charges to be brought against it. One is the mortality, and the other is inveterate diarrhœa.

Lord Dawson, who I have no doubt has seen a considerable number of the results of complete colectomy, says, "Its failures, not a few in number, are sometimes apt to be such dire failures as to mean for the patients misery and suffering little capable of alleviation." By this I presume he means diarrhœa amongst other things. It would be infinitely less of an evil to continue with the most obstinate constipation than to have it converted into what I believe some of these poor unfortunate individuals call the lavatory habit.

The other serious drawback to this operation is the mortality. It is difficult to arrive at an exact percentage, but surgeons, as a whole, I am sure, have the impression that it is very high. As I have already said, in Leeds we hardly ever do this operation, and our figures are therefore of no use for drawing conclusions.

Our conclusions are:—

- (1) Intestinal stasis is probably only a part of a general malnutrition due to a deficiency disease.
- (2) Removal of the colon can only be of use when it is acting as an obvious cesspool.
- (3) This condition arises when there is a soggy, thick, and full cœcum, together with a band across the ascending colon.
- (4) This is found only in a limited number of the whole number of stasis cases, and this type can usually be differentiated from the other types, for example, the merely loose colon, before operation.
- (5) When this particular type is present, it can be confidently expected that hemicolectomy will yield a good result: or at the worst, the patient will consider that the operation has been worth while.

(6) When this type is not present, we no longer practise colectomy; believing that the looseness of the colon is a matter of no importance.

(7) Under no circumstances is total colectomy recommended. The most brilliant results, when they occur, are no better than the excellent results which follow some cases of hemicolectomy, and the bad results are infinitely worse than anything ever seen amongst the bad results of hemicolectomy. The latter operation, at any rate, never makes the patient worse.

The mortality of hemicolectomy is about 2.5 per cent. I think this is less, to say the least, than the mortality after complete colectomy.

SIR BERKELEY MOYNIHAN'S CASES OF PARTIAL COLECTOMY FOR
INTESTINAL STASIS.

The list of cases includes all those submitted to the operation of colectomy or ileosigmoidostomy for stasis, or its results, up to the end of 1920. In all 68 cases were operated upon—60 were cases of primary colectomy, and 6 were cases of ileosigmoidostomy. Of the 68 cases, 21 were males; 47 females. Of the 68 cases, 2 cases of colectomy died as the result of operation, 1 three days after operation, the patient being a thin, weakly individual of low resistance for whom I ought to have performed one or more direct transfusions of blood. It was an error of judgment to operate upon so weak a patient for a condition that did not threaten life. Now we always improve the pre-operative state of this type of patient by the administration of large quantities of glucose, and sometimes by direct transfusion of blood.

The second patient died of pulmonary embolism on the fourteenth day, when, as is usual in such cases, all things had progressed most satisfactorily up to the moment of the dramatic and unexpected end.

No case of ileosigmoidostomy was fatal.

From the 66 cases remaining, replies to our inquiries have been received from 43. Of these 43, 37 had been submitted to primary colectomy; 6 had been submitted to primary ileosigmoidostomy.

The results in the 37 cases of primary colectomy were as follows:—

(a) In 15 cases the results were "good" "very good" or "excellent," to use the patients' or the doctors' words: 6 were males, 9 females. In all there had been a gain of weight varying from 1½ to 4 st.; there was no indigestion, flatulence or discomfort; the bowels acted normally; there was full capacity for work and enjoyment. In all these cases gross mechanical difficulties had existed, and the cecum was full, blue, soggy, dependent and obstructed; not all patients who had these conditions were improved; but all who were improved had such conditions.

(b) In 16 cases the result is called "fair." The patients were better in general health but occasional heartburn, flatulence and discomfort were present. Some patients had diarrhoea; some constipation as badly as before. In none was there a substantial gain in weight; many of the patients were grateful for the benefit though none were in vigorous and sustained good health.

(c) In 6 cases the result is "poor" or "bad." The patients are still ailing, complaining, miserable; weak, thin, and unfit. I think all are examples of "deficiency diseases."

The results in 6 cases of ileosigmoidostomy were as follows:—

(a) In 2 the result is stated to have been "good"; in one of these at the end of two years regurgitation into the ascending colon caused the formation of a tumour due to a soft faecal mass. Complete colectomy was performed for this elsewhere and the patient died.

(b) In 2 the result is said to be "fair" in one, "not much improvement" is recorded in the other.

(c) In 2 the result is "poor"; in one of these I performed complete colectomy afterwards; the early result after that operation was good, but the patient is now no better than before her first operation. This case is not included in those of primary colectomy.

On these patients the following previous operations had been performed:—

(1) *Colectomy Cases*.—The appendix had been removed without benefit in 17 cases; of these, later improvement by colectomy is reported in 7; colectomy without improvement, 4.

(2) *Ileosigmoidostomy Cases*.—The appendix had been removed without benefit in 5; of these later improvement by ileosigmoidostomy is recorded in 2.

(3) In both colectomy and ileosigmoidostomy cases: (a) various pelvic operations—curetting, oöphorectomy, ventrifixation, had been performed without benefit in 5; (b) nephropey, without benefit, had been performed in 2.

The state of the cæcum and ascending colon was as follows (in some cases conditions placed in two or more categories were present):—

(1) Constricting bands across the colon just below the hepatic flexure, usually with large, blue, soggy, overfull cæcum, with delay in onward passage of contents, as shown by radiographs, of 48-96 hours. This constricting band runs from the parietal peritoneum in the outer side of the ascending colon downwards and inwards and is lost on the upper surface of the ileal mesentery often in close association with the glands so frequently found here. It may be a congenital condition which, possibly, would not give rise to any delay were it not that the colonic neuro-muscular mechanism degenerates, and becomes unable to push its contents past.

All results were good or fair, 20 cases.

(2) Cæcum and ascending colon fixed, no mesocolon, 7 cases.

(3) Common mesentery to ileum and ascending colon, with very loose and mobile cæcum and colon, 18 cases.

(4) "Cracker colon," the name we give to an ascending colon turned to right and left alternately, the turns being packed on top of the other as in a firework cracker, and held together by strong adhesions, 5 cases.

(5) Adhesions round the cæcum, appendix and ileum, including Lane's kink, 14 cases.

(6) Mesenteric glands caseating and tuberculous, in ileocecal angle and along the ileocolic artery, 12 cases.

Professor Sir ARTHUR KEITH, F.R.S.

It is owing to Sir Berkeley Moynihan, Mr. Tyrrell Gray, Sir Gordon Watson (President), and several other surgeons, that I am able to contribute to the present discussion. They have forwarded the parts of the great bowel removed by operation to the Museum of the Royal College of Surgeons, where a macroscopic and microscopic examination of the degree of disorganization shown by the varying specimens has been made and the results recorded. The colons removed can be classified into two groups. The first group contains those showing an intense degree of inflammatory infiltration, being in the condition to which Sir Berkeley Moynihan has given the apt term of "soggy." In these all the coats of the bowel are affected, the incidence of the disorder falling particularly on the interglandular retiform tissue of the mucous coat. Instead of being an open cellular network this tissue has become crowded by the proliferation of its constituent corpuscular elements. The mucous cells lining the absorbent surface of the bowel, as well as those within the tubular glands, are irregular in form and abnormal in structure, but anything of the nature of ulceration is very rarely noted. The fibrous elements of the submucous coat are greatly increased in amount. The muscular coats showed the results of inflammatory changes, the cellular elements of Auerbach's plexus being degenerated in many areas. The peritoneal coat is thickened, and clearly the seat of chronic inflammatory changes. These appearances are not confined to the colon; the lower end of the ileum and the cæcum show them to the greatest extent. The state of structural disorganization in this group of colons

is such that it is difficult to conceive they could recover without great functional and structural impairment. On this point further observation is necessary.

In the second group, which may be called the pigmented group, signs of chronic inflammatory changes are present, but only to a slight degree. On microscopic examination the chief changes are again to be seen in the retiform tissue of the mucous coat. The lymph vessels lying in this tissue between the tubular glands are seen to be dilated, particularly as they approach the muscularis mucosæ, which separates the retiform tissue from the submucous coat. On the walls of the lymph vessels or spaces, especially just above the muscularis mucosæ, are to be seen large cells laden with a dark-brown granular material arranged in large clumps or clusters. The pigmented material, so I am informed, is closely related to melanin and to adrenalin. The muscular coats and Auerbach's plexus also show certain structural changes very similar to those seen by Lieutenant-Colonel R. McCarrison in the bowels of animals fed on a diet deficient in vitamins. This condition is being investigated experimentally by Dr. Louis Gross. From Professor T. R. Elliott's discovery of the action of adrenalin on the motor functions of the bowel, and from Lieutenant-Colonel McCarrison's researches on the effect of a deficient dietary on the adrenal bodies, it seems probable that there may be a relation between "stasis" and the presence of these pigmented cells in the retiform tissue. In this second group the structural changes do not seem to be of a kind which precludes complete recovery.

So far I have not had an opportunity of examining the alimentary tract of cases which have been cured by colectomy. A week ago, however, Mr. Tyrrell Gray submitted to me for report the alimentary canal of a case in which ileo-sigmoidostomy had been done ten years previously. As in a case of Professor Rutherford Morison's, recently reported by Mr. Hamilton Drummond, the great bowel had continued to act, the contents passing from the rectum back into the colon to its cæcal extremity. The various coats of this great bowel, although fed from its distal end, are intact, but certain structural changes are clearly evident within them. The most remarkable change is seen in the small bowel. The total length from pylorus to the termination of the ileum is 7 ft. 9 in. in place of 22½ in., the average length of the small bowel in an adult male. What this intestine has lost in length has been gained in size of lumen. Its coats are thickened, and it has the diameter of the great bowel. At the best colectomy appears to entail extensive adaptational changes in the small bowel. It is most desirable that the condition established in successful cases of colectomy should be examined and placed on record.

My opinion is that the results of colectomy make it impossible for anyone to accept the conception of the big bowel advocated by Metchnikoff twenty years ago, namely, that it has become a useless structure in the economy of the normal human body. The question which has now to be settled is the following: "Is it better to have no big bowel at all rather than one which is gravely disorganized by disease?" If the latter alternative is chosen, then we have to decide on the nature and degree of the lesion which demands colectomy, and determine the means of recognizing such conditions by clinical methods.

DR. ARTHUR F. HURST.

The invitation to a physician to take part in a discussion on the after-results of colectomy for colon stasis is, I take it, an acknowledgment that colon stasis is primarily a medical disease. Just as it has become a surgical

aphorism that no operation should be performed on the stomach in the absence of a demonstrable lesion of the stomach and duodenum, so I hope that the day is not far distant when a similar aphorism will be accepted for the colon.

The pathology of the living as revealed by the surgeon has received no more valuable contribution than that of Sir Arbuthnot Lane in his work on intestinal stasis. Though I cannot agree with many of his conclusions I gladly acknowledge my great indebtedness to him, which I feel in common with the whole profession, for his demonstration that intestinal toxæmia is an important factor in the production of many diseases of obscure origin. He has taught us that the intestine requires treatment in these conditions, in some of which it had hitherto been completely neglected. I am sure, however, that this afternoon's discussion will show that, now that this lesson has been learnt, medical treatment is in the vast majority of cases all that is required, and that, in view of its immediate and remote danger, colectomy is rarely, if ever, indicated for intestinal stasis in the absence of gross organic disease.

Partial Colectomy.

I have only had an opportunity of observing the after-results in two cases:

Case I, is that of a lady, aged 40, from whom Sir Charles Gordon Watson excised at my request in February, 1920, a very mobile cæcum, ascending colon and part of the transverse colon for stasis in these parts, accompanied by severe pain and tenderness and general symptoms of toxæmia, all medical treatment having failed to give relief. The excised colon was found by Sir Arthur Keith to show the pigmentary changes he has just described. The patient has very slowly improved, but she is now, two years after the operation, still constipated and still far from strong, though so much better that she is very glad the operation was performed.

The second patient only consulted me some years after the operation had been performed.

Case II: Six Abdominal Operations, including Partial Colectomy for Hypochondriasis: no Aggravation of Symptoms.—Colonel R., aged 54, had adhesions divided in 1900, a gastro-enterostomy performed without any ulcer or obstruction being found in 1907, an ileo-sigmoidostomy in 1912, and 4 in. of small intestine with the cæcum, ascending colon and half the transverse colon removed in 1915. In 1916, the end of the small intestine became dilated and was removed, and in 1918 pylorectomy was performed. In spite of his six operations I was unable to ascertain from him, when he came into New Lodge Clinic in January of this year, that he had ever had anything more than vague discomfort in the right side of his abdomen. Although a typical hypochondriac, he seemed to be in remarkably good health in spite of his six operations. His chief complaint was that, although only 54, he got tired towards the end of his second round of golf. He had hoped in vain that the colectomy or at least the pylorectomy would have altered this unsatisfactory state of affairs. He was disappointed to find that medical treatment could not do for his golf what six operations had already failed to accomplish.

The success of the operation in the first case, though incomplete, has been great enough to encourage me to advise it again, if a similar case were to come under my care, in preference to the various colopexies recently recommended—the results of which have not impressed me favourably—and certainly in preference to complete colectomy. I have, however, so far not seen any other patient with ascending colon stasis who did not improve sufficiently under medical treatment to make it undesirable to undergo the risk of an operation, the results of which are uncertain.

Total Colectomy.

I have had the opportunity of observing the after-effects in a considerable number of cases of total colectomy performed for intestinal stasis, although I have myself never felt justified in advising a patient to undergo the operation. With regard to the immediate mortality, I have obtained the statistics of the Guy's cases since 1914. A continuous record is thus available, which includes those recorded by Sir Arbuthnot Lane in 1908, and those published by Dr. W. S. Bainbridge of New York, from his analysis of all Sir Arbuthnot Lane's female hospital cases between May, 1909, and October, 1913. Cases of gross organic disease, such as ulcerative colitis and cancer, are excluded. A small number operated upon during the war, of which no records have been preserved, have also of necessity been omitted. Among the thirty-seven cases in the first series, seven died as the immediate result of the operation, one died six months later from intestinal obstruction, and one a year later from exhaustion. The immediate mortality was therefore 19 per cent., and the total mortality 24 per cent. In the second series of cases three out of fifty died before leaving hospital, and one after a secondary operation. In the last series of 111 cases operated upon between 1914 and 1921, the mortality was 18 per cent. The total mortality of the 198 cases is 16·5 per cent. It is interesting to note that since 1914 the number of cases operated upon has steadily diminished, until only two colectomies were performed in 1919, one in 1920 and none in 1921.

COLECTOMIES PERFORMED FOR INTESTINAL STASIS AT GUY'S HOSPITAL, 1904-1921.

Year	Number of cases	Deaths	Mortality
1904-08	37	9	24·0 (Lane)
1909-13 (female only)	50	4	8·0 (Bainbridge)
1914	40	5	12·5
1915	34	4	12·0
1916	12	4	33·0
1917	9	3	33·0
1918	13	4	30·8
1919	2	0	—
1920	1	0	—
1921	0	0	—
Total	198	33	16·5

Sir Arthur Keith has asked what becomes of the severe cases of intestinal stasis, if they are not operated upon. One thing I can say is that they do not die. With the exception of a case recorded by Mr. Lockhart-Mummery, I have never heard of death occurring as a result of intestinal stasis. The colon has an extraordinary power of recovery. In amœbic dysentery the changes in all coats of the colon may be exceedingly severe, far more serious than anything described by Sir Arthur Keith as a sequel of intestinal stasis, and yet complete recovery almost invariably takes place under treatment with emetine.

It can therefore rarely, if ever, be justifiable to recommend for intestinal stasis complete colectomy, which has an average mortality of 16·5 per cent. in the most experienced hands. Symptoms of intestinal stasis are rarely sufficiently serious to warrant anything but comparatively simple medical treatment. In the few cases, in which the local or general symptoms, or both, are of such gravity that the patient's life is rendered miserable, prolonged institutional treatment by non-surgical means should be carried out before the question of surgery is considered. I have often been appalled to see how

light-heartedly colectomy has been recommended, often for comparatively trivial symptoms, without any real trial of medical treatment having been made. It is sometimes forgotten that there are other aperients besides paraffin, and that psychotherapy, diet, massage, exercise, intestinal lavage, and perhaps bacteriology each has its place in the treatment of intestinal stasis.

Among the very large number of severe cases of intestinal stasis I have seen since I first became interested in the subject as a result of my X-ray investigations of the normal and pathological intestines fifteen years ago, I have only seen two in which the whole colon was involved in stasis, and in which the patient became steadily more poisoned and suffered more and more pain in spite of every form of medical treatment I could devise. In one, Sir Arbuthnot Lane performed an ileo-sigmoidostomy in 1911, and the relief, which was almost instantaneous, has proved permanent and complete. In the other, the same operation met with indifferent success, and a secondary cæcostomy was performed by Mr. Percy Sargent six months later, owing to the accumulation of faeces in the short-circuited colon. One other patient of mine, whose symptoms were less severe, had a colectomy performed by Sir Arbuthnot Lane, whilst I was abroad in 1915. She required a second operation for adhesions in 1916, but since then she has been extremely well; she presents a real triumph for the operation, though considering the absence of any serious local or general symptoms I should not myself have felt justified in advising a colectomy.

In discussing the later results of the operation, it is obvious that only cases in which a year at least has elapsed since its performance need be considered. In Sir Arbuthnot Lane's 1908 report, out of sixteen such cases ten were relieved to a greater or less extent, and in six the result was only moderately good. Five out of Bainbridge's 1909 to 1913 series of forty-seven survivors had returned to the hospital by 1913 suffering from adhesions and requiring further operation; one of these, with pain and vomiting associated with pregnancy, died from general peritonitis after a second operation. During the last few years numerous cases have been admitted to the surgical wards at Guy's for various complications following colectomy, and in many instances a second operation has had to be performed.

The medical treatment of these unsuccessful colectomy cases is most unsatisfactory, and they generally become hopeless chronic invalids. In the following case, quite unexpected improvement in the severe toxæmic symptoms, which had resulted from a colectomy performed for intestinal stasis, followed treatment with sour milk.

Severe Toxæmic Symptoms after Colectomy for Intestinal Stasis relieved by Sour Milk.—Mr. K., aged 50, had complete colectomy performed in 1918 for constipation and indigestion, neither of which appears to have been very severe. After slight temporary improvement he began to suffer from pain in the lower part of the abdomen. This gradually became more severe, and for some months before he was admitted to New Lodge Clinic in February, 1921, it had been continuous through the whole day. In addition it awoke him at about 3 a.m. and prevented him sleeping the rest of the night. He passed liquid stools three or four times a day with the aid of a little aperient, without which he was very constipated. He said that since the operation his nerves were "all to pieces," that he had lost confidence in himself, everything looked black, and that he could not manage his business. The X-rays showed that the lower end of his ileum had become very greatly dilated. The sigmoidoscope passed without resistance its full length into an enormously dilated bowel. Treatment by diet, intestinal lavage and drugs failed to give relief. We then tried the effect of sour milk. The result was surprisingly satisfactory. All the symptoms, which had made life miserable for over a

year, disappeared within a few days. He has remained completely well ever since, with the exception of ten days recently spent on the Continent, when he was unable to obtain the sour milk. Two days after having it again on his return home he felt quite well.

The ileo-cæcal sphincter is as important for intestinal digestion as the pylorus, and makes it possible for over 95 per cent. of the digestible part of the food to be absorbed before the cæcum is reached. In the cæcum and ascending colon the remainder, together with a considerable amount of water, is absorbed. Consequently, when either the whole colon or the proximal part alone, together with the ileo-cæcal sphincter, is removed, intestinal digestion is severely disturbed, and it is comparatively rare for the remaining part of the bowel to take on the functions of the parts removed so satisfactorily that the patient can ever again become completely fit, even if certain local and general symptoms disappear as a result of the operation.

The loss of the ileo-cæcal sphincter, which normally acts as a barrier preventing the bacterial activity, which is natural in the colon, from occurring in the small intestine, also results in the fluid contents of the lower end of the ileum undergoing excessive fermentation and putrefaction. Intestinal flatulence results, which often causes much discomfort and even pain, especially at night, and severe insomnia may follow. A temporary diminution in the symptoms of intestinal toxæmia not infrequently follows the operation, but sooner or later these generally return, though often only in a milder form. It is this bacterial invasion of the small intestine which leads to the very disappointing recurrence of certain serious symptoms, which occasionally disappear in an almost miraculous manner after colectomy. This happened, for example, in a very severe case of Raynaud's disease, in which the immediate result of colectomy had been an astonishing improvement in the symptoms which had been present for some years. But the improvement was already less obvious by the time the patient left the hospital, and the final result of the operation was entirely unsatisfactory.

I have not yet seen any convincing evidence from the results of colectomy that intestinal intoxication plays any part in the production of Graves' disease or diabetes. The improvement, which has sometimes followed the operation in rheumatoid arthritis and especially in Still's disease, can be obtained with far less danger and much better prospects of permanency by eradicating the primary source of the intestinal infection, which is generally in the mouth or pharynx and occasionally the appendix, and treating any residual infection by bacteriological and other non-surgical measures.

Peristalsis in the colon is not continuous, as in the stomach and small intestine, but only takes place three or four times in the day, generally immediately after meals. A powerful contraction, beginning, as a rule, in the ascending colon, carries the whole of the contents of the large intestine into the pelvic colon, from which they are propelled by the early morning peristalsis into the rectum; this gives rise to the desire to defæcate, which normally initiates the reflex act of defæcation. Partial or complete colectomy removes the segment of the colon in which the majority of peristaltic waves arise, and complete colectomy removes all or most of the part which retains the fæces till just before defæcation. It is, therefore, not surprising that the bowels should act with considerable irregularity after colectomy. The stasis in the colon, for which the operation is performed, is sometimes replaced by stasis in the small intestine. More often, however, the continuous arrival in the rectum of fluid fæces from the ileum leads to the frequent passage of fluid stools. It is rare

for anything approaching the regular morning call to defæcation with its sequel, the passage of a normal formed stool, to occur after a complete colectomy.

The following case is an example of the intestinal disturbance which may follow colectomy:—

A lady, who had been in fairly good health with some constipation, but no diarrhœa, since her colon was excised for chronic intestinal stasis eight years before, was suddenly seized with the most intractable diarrhœa, which for nine months did not respond to any treatment by drugs, diet, or starvation, and almost proved fatal. There was no apparent cause, but this may perhaps be an instance of the inability of the body, in the absence of the colon, to deal with certain forms of poisoning. Thus very small doses of morphia made her feel extremely ill, and I understand that similar intolerance to morphia is present in most, if not all, patients who have had their colon removed, probably owing to the fact that the colon is normally the chief channel for its excretion. In the seven years which have since elapsed her intestines have been a constant and painful source of anxiety. She tends to become constipated, but all aperients give rise to fluid stools, the passage of which does not give adequate relief, and she constantly feels that there is an accumulation of fecal material which has been left behind. This condition lasts for weeks, the patient feeling more and more ill until finally a severe attack of diarrhœa occurs, which eventually gives relief. The frequent straining has led to a severe prolapse, which has recently been relieved by operation.

I can summarize my views on the subject in the following way: The majority of cases of intestinal stasis, which are treated with sufficient perseverance by non-surgical means, do very well. Even in the very small number in which little or no improvement follows medical treatment, the high mortality, the danger of complications, and the uncertain prospect of lasting relief after colectomy, make it doubtful whether the operation should ever be performed in the absence of gross organic disease.

DR. VICTOR PAUCHET (Paris).

Chronic intestinal stasis now occupies an important place in medico-surgical pathology, and should, for the future, be termed "Arbuthnot Lane's disease."

Total colectomy is a well-defined operation, indicated not only in cases of intestinal stasis, but also in cancer of the colon, in megacolon (Hirschsprung's disease), in severe forms of colitis, and in volvulus, whether accompanied or not by intestinal occlusion. Total colectomy should be designated as "Arbuthnot Lane's operation."

Why is colectomy warmly advocated by some and strongly opposed by others? Because those who have frequently performed this operation have observed some failures, but also brilliant successes. While its partisans dwell upon the successful results its opponents maintain that:—

(A) *Colectomy is a serious operation.*

(B) *The remote results are not uniform, some patients suffering from (a) diarrhœa; some from (b) constipation; and others from (c) persistence of the pre-operation troubles: neuralgia, asthenia, loss of weight, indigestion, menstrual irregularities, &c. I know a whole group of patients who have undergone colectomy and are still suffering from neurasthenia; they are satisfied with the result of the abdominal operation and are greatly improved; however, they still imagine that they are a prey to all sorts of illnesses.*

Favourable Results.—These are apparent in the disappearance of the constipation, the headache, migraine, dyspepsia and asthenia: the patient

begins to put on weight, takes an interest in life, experiences once more the "joy of life." The effect on the female genital organs is very marked, as shown by the following brief case-records:—

Case I.—A lady from the North of France suffered from recurrent abortion during pregnancy and complained of intense headaches and insomnia. I performed colectomy. Six months after the operation she became pregnant; this first pregnancy lasted for six months; the second for eight months, when she gave birth to a still-born child; the third pregnancy was normal and she was delivered of a living child. She was not syphilitic.

Case II.—A female who before the operation was agenesic returned to see me a few months later suffering from gonorrhœa.

Case III.—A woman who had been divorced for agenesia was in such a hurry to get married again that she married her doctor.

A large number of patients are enthusiastic with regard to the results, since the disappearance of their debility or malady enables them to resume a normal life.

Unfavourable Results.—Although chronic intestinal stasis has been studied for twenty years by Sir Arbuthnot Lane, and its operative treatment has been undertaken by some surgeons during the past fifteen years, nevertheless comparatively little is known about this affection and its treatment, and colectomy is rarely performed. Therefore, it may be said that chronic intestinal stasis is a new disease, that the proposed method of treatment is of recent date, and that, so far, it does not rest on an established basis. The clinical and technical aspects of the treatment are well defined, but this cannot be said of its *indications*.

On the basis of my statistics, it must be confessed that *total colectomy still is a serious operation*. I have performed 198 operations, without taking into account minor operations such as cæco-plicatures, colopexy, or resection of adhesions. These operations included 122 cases of short-circuiting, with eight deaths; 52 cases of total colectomy, with six deaths (four due to rupture of the suture and two to cardiac failure); 21 cases of right-sided hemicolectomy, with one death; 3 cases of segmentary colectomy performed for volvulus of the sigmoid flexure, without a single fatal termination. Among the total colectomies, 20 were performed in two stages—i.e., 17 following short-circuiting, and 3 consequent on right-sided hemicolectomy. *I never had a fatal result after performing colectomy in two stages, nor after segmentary colectomy.*

Referring to the *causes responsible for the six deaths* following total colectomy, I would point out that in four the cause was easily preventable: two died from leakage at the suture line of the intestine, in obese subjects, on the fourth and fifth day respectively. Since then, in the case of adipose patients, I confine myself to short-circuiting, or to performing an incomplete resection with latero-lateral anastomosis, if necessary resorting to a secondary colectomy. One patient, who had not been properly purged, died because his colon was filled with fæces, and I was compelled to operate on a soiled intestine; post-operative drainage by means of the œsophageal tube proved unsuccessful and the patient died from leakage at the suture line. I should have closed the abdomen without operating. Another case ended fatally through the onset of gangrene of the ileal extremity, due to deficient circulation. In making an end-to-end suture it is imperative to be sure that the dissected extremity of the ileum is well vascularized.

There will be no recurrence of these four failures in my practice because:

(a) I shall avoid end-to-end suturing of intestines in obese patients; (b) I shall select a better vascularized end of the ileum; (c) I shall only operate on a colon which has been thoroughly evacuated.

There remain two cases in which the patients died shortly after the operation, one after twelve and the other after twenty hours without any immediate shock. They became cyanotic and died from cardiac failure. I can offer no explanation in these cases; perhaps death was due to suprarenal insufficiency, and at present I am powerless to deal with such complications as occurred in these two instances.

My opinion is that after taking into account the avoidable accidents, but which were not prevented, it will be possible in future to reduce the mortality to 4 per cent.

Remote Results.—In order to advance therapeutic progress, it is not enough for experienced surgeons to point to the splendid results achieved, for opponents will immediately seize upon the failures and make use of them to discredit the method. The experienced surgeon should draw attention to the possible accidents and explain their causes in detail, in order that his colleagues may be in a position to avoid such untoward occurrences. It is by pointing out the accidents following operation and their causes, and by emphasizing the cause of remote failure that we shall succeed in establishing precise indications for operation, in avoiding mistakes in technique, and shall be in a position to select suitable cases for operative treatment.

I have sometimes observed the occurrence of the following untoward after-results:—

(1) *Diarrhœa.*—This is not due to faulty digestion of food-stuffs, but to defective functioning of the ileo-sigmoid anastomosis, to faulty co-aptation of the intestinal extremities, to torsion of the anastomosed loop, to adhesions on a level with the ileo-sigmoid suture, causing incomplete evacuation of the ileum, coupled with irritation, and thereby giving rise to this diarrhœa. After the operation, and indeed for several months to follow, it is imperative to insure regular evacuation of the intestinal contents by means of liquid paraffin, if necessary, in combination with castor oil. The occurrence of adhesions is the complication most to be dreaded, and to avoid these the operation should be performed carefully, reducing to a minimum the area of operation and insuring perfect control of the hæmorrhage.

(2) *Constipation.*—Should constipation recur, its reappearance is due to the same causes that produce diarrhœa.

(3) *Persistence of Pre-operation Troubles.*—Some patients complain still of neuralgia, asthenia, loss of weight, indigestion, menstrual irregularities, &c.

I will now briefly report the unfavourable cases:—

Case I.—A woman, aged 40, on whom I performed colectomy ten years ago. She became addicted to morphine, and for this reason constantly complains of troubles due to the effects of morphine.

Case II.—Male, aged 27, in whom the diarrhœa present before the operation persisted afterwards, coupled with pronounced asthenia and complete impotence. He went to Africa, where he was seized with an attack of acute intestinal obstruction. An operation was performed in the hospital, where the surgeon discovered an adhesion at the junction of the ileo-colic anastomosis. He divided the flap, and the trouble ceased. The diarrhœa disappeared, and the patient goes regularly twice a day to stool, while his general condition has greatly improved. This proves that the diarrhœa is caused by a flexure of the ileo-sigmoid anastomosis, a sequela consequent on the formation of adhesions or due to faulty technique.

Case III.—Patient, a male, aged 27, an electrician, stated that he was dissatisfied with the result of his operation. Thanks to physical exercise, he now possesses a magnificently developed muscular system, but he is not yet satisfied, as he is still impotent and neurotic.

If I were now called upon to state the ratio of successes to failures, I should say that: (a) of ten colectomies, we should reckon with one death to nine operative successes; (b) of these nine successful operations, three may be classed as very good results; up to three as average and satisfactory; three in which the result is *nil* or almost *nil*. However, the cases in which no result, or scarcely any result, follows operation are attributable to the fact that the patient receives neither the care nor the advice which his condition demands. If a forest is destroyed by fire, we extinguish the conflagration, but we must replant the trees and wait until they grow. Stercoræmia has destroyed the cells of various organs—e.g., thyroid, ovaries, kidneys, liver, suprarenal capsules, pituitary gland, &c. Endocrine insufficiency is present, pulmonary trouble, bad psychic habits, &c. The mere removal of the colon abolishes the cause of the stercoræmia, but does not recreate the destroyed gland cells. Colectomy does not immediately remove the mental defects and the functional disturbances. If we wish to obtain good remote results, we must not discourage the patient by stating that the operation is a serious one, and that he is “mutilated.” On the contrary, we must make him undergo a course of organo-therapeutic treatment, with physical exercise and muscular development, coupled with breathing exercises and *mental re-education*. In other words, we must seek *patiently to build up once more all that the disease has destroyed*. To this a large number of surgeons will object that it is scarcely worth while to perform a colectomy on a patient if you are compelled to have recourse to medical treatment after operating. In my opinion, this objection shows lack of judgment. Colectomy has one definite object: to abolish the cause of the mechanical or stercoræmic troubles in the individual. All the troubles which appear later must be removed gradually. I believe that in future we shall achieve still better results, and this for the following reasons:—

(a) There will be a greater number of surgeons ready to perform operations in cases of Lane’s disease. (b) Further, if left unoperated upon, the lives of these patients will be destroyed by the stercoræmia. (c) The operation will be carried out with greater precision, and the patients will receive better post-operative treatment.

Summary.

Colectomy is a good operation. Intestinal stasis is a well recognized affection, but what we lack at present, and the point which requires elucidation, is a knowledge of the *indications*. In what cases should recourse be had to short-circuiting, to colectomy, or merely to medical treatment? When should the operation be performed? What treatment should be adopted before and after colectomy? The future of Arbuthnot Lane’s operation depends upon the solution of these problems.

SIR WILLIAM ARBUTHNOT LANE.

I had not intended to make any remarks, as I have already written and spoken so often on the subject of chronic intestinal stasis, but as your President has asked me to say a few words I will do so.

I am entirely in agreement with Dr. Hurst when he says that intestinal stasis and the several operations called for to meet it are due to the incompetence and ignorance of the physician. This is a condition which will I trust gradually disappear. I feel certain that when the profession has grasped the meaning of chronic intestinal stasis and of the dependence upon it of a very large number of diseases, an immense advance will be made in preventive medicine.

I would impress on the Fellows of this Society Colonel McCarrison's experience in India. He spent some years in the Himalayas, during which time he did a large practice, performing more than 400 capital operations each year. During that time he never saw one case of asthenic dyspepsia, of gastric or duodenal ulcer, of appendicitis, of mucous colitis, or of cancer. I have heard the same evidence from other very experienced medical men who have been engaged in practice for many years among the primitive races.

The opponents of colectomy do not grasp for a moment what a big question stasis is and do not realize that in attacking an operation which deals with a very advanced condition of stasis, either simple or complicated by the presence of superadded disease which is very often fatal, their area of vision is very limited. On the same ground they might just as reasonably oppose operations on the stomach, appendix and gall-bladder, since the infections and changes in these organs are merely earlier stages of chronic intestinal stasis. The time is coming (if it has not already arrived) when quite a number of medical men will realize the sequence of stasis, and recognize that the rôle of the profession is not so much to devise new operations as to attempt to prevent the diseases for which their operations are done. It is obvious that if in a primitive existence stasis and its associated conditions do not exist we should be able by a study of the relationship of these people to their surroundings to free ourselves from what is practically the cause of almost all disease in a civilized community. I need hardly waste your time by indicating further the weakness of the arguments that have been put forward against colectomy, but much is due to the want of familiarity of the opponents with stasis and with the conditions of the patients operated on. To say that no patient dies of stasis, or of the conditions which develop in consequence of its presence, shows a mentality that is utterly beyond my grasp.

As regards colectomy, which I consider to be an operation that is as indefensible but as necessary as appendicectomy, gastro-enterostomy, gastrectomy, cholecystectomy, &c., and will I trust like them become obsolete with the growing intelligence of the physician, I believe that this operation in experienced hands offers a minimum of risk to the patient as compared with the almost incalculable advantages generally obtained from it. It is apparent even to the most critical that the chances of the patient surviving the operation must depend on the condition at the time of its performance, as well as on the skill and experience of the operator. Its immediate risk is remarkably small as compared with other intra-abdominal operations, for the reason that the removal of the large bowel usually leaves the patient with a loose abdominal wall and with a low degree of intra-abdominal tension. The mortality of this operation, as indeed of all very serious abdominal operations, varies inversely as the intra-abdominal tension. With a flaccid abdominal wall in an aged female the risk is practically *nil*. The only cause of failure and anxiety is the formation of adhesions, but this complication is common to all intraperitoneal operations. It may be reduced to a minimum by the exercise of great care in covering up raw surfaces and in the avoidance of

damage to the bowel or to the peritoneum. This is sometimes rendered very difficult by previous operations such as colostomy, appendicostomy, fixation of portions of the colon to the abdominal wall, gastro-enterostomy, &c. For establishing continuity between the divided ends of ileum and pelvic colon, every precaution should be taken against fouling the adjacent bowel or the edges of the wound in the abdominal wall from which adhesions may result. I am certain that when surgeons have become familiar with this operation and have realized the immense benefit the patient so frequently derives from it, it will be resorted to much more frequently and with greater safety than it is at present. As to when it should be performed much depends on the thorough recognition of the causation, pathology, symptoms and results of chronic intestinal stasis. To assert that the healthy large bowel is a useless organ is just as ridiculous as to make the same statement about the healthy uterus, stomach, duodenum or small intestine. The important question is, why should the large bowel be removed and what are the benefits afforded by its removal? This I have answered so frequently that it would be useless to repeat my reasons here now beyond saying that the fundamental object of the operation is to free the ileal effluent and to remove the infection of the gastro-intestinal tract. It is the turn of others to state their views and experience of the operation, and I am very glad to have this opportunity of learning them from those present.

Dr. LOUIS GROSS.

During the last eight months I have been making observations on the ætiology and pathology of the condition, or perhaps I should say conditions, known as "intestinal stasis."

I am indebted to Sir Arthur Keith for the opportunity of examining a considerable amount of material, both macroscopic and microscopic, obtained from these cases at the operating table. In these examinations I was particularly struck by two interesting lesions. One of these is the accumulation of large cells with indistinct nuclei, containing numerous brown granules in their cytoplasm. These cells lie in the stroma under the epithelium and above the muscularis mucosæ. They are sharply localized to that area, often bathed in lakes of lymph and strictly limited to the colon. The other point is the distinct pathological lesion often found in Auerbach's plexus, ranging from inflammatory cell infiltration and necrosis to extreme fibrosis.

In reading through the literature, my attention was attracted to the results which McCarrison obtained in his experiments on deficiency feeding of animals. He showed, amongst other things, that a deficiency of vitamin B produced a marked dilatation of the intestine, necrosis of Auerbach's plexus and decrease in the adrenalin content of the adrenal glands. I was anxious to ascertain the dynamics of an intestine which is the seat of such lesions and have accordingly been carrying out experiments in Professor Starling's laboratory with the object of determining this point.

I have been feeding rats on diets completely deficient in vitamin A, B and C respectively. In some cases I administer intraperitoneal injections of endocrine extracts, in others I endeavour to diminish the different endocrine contents of the body by surgical removal of glands, for I desire to determine, if possible, the part which these play in the economy of bowel activity. For controls I have been feeding rats on exactly the same diets, which by the way are cellulose-free, but with the addition of the necessary vitamins. During the course of these feedings I take notes of the condition of the animals, the temperature, weight, food intake, weight of fæces, &c.

Although my experiments are not as yet completed I have made several observations of possible interest.

After a special feeding of between four and six weeks, sometimes longer according to the diet and condition of the animal, the rat is killed by a blow on the head. Autopsy is performed and notes taken as to the condition and appearance of the organs. Sections are preserved for microscopic study, the adrenal glands are used for chemical estimation of the adrenalin content, and slips of known and uniform length are removed from the terminal ileum and proximal ascending colon to be tested for motility. This is accomplished by suspending the slip of bowel in a bath of Tyrode's solution kept at a uniform temperature of 37° C. and attaching it to a lever which draws a tracing on a moving kymograph. A slip of ileum obtained from a rat which has been fed on a diet adequate in vitamins gives a tracing which shows the following characteristics:—

Regular beats, 20 to 25 per minute, with an amplitude (depending of course on the leverage which I employ) of 2 to 3 mm.

Regular periodicity of groups of beats resembling a tracing of Cheyne-Stokes respiration.

A slip of colon gives a fairly regular beat of 2 to 3 per minute with an amplitude of 1.5 mm.

These intestinal beats represent the myogenic contractions (pendulum movements), but can safely be taken as an index of the general muscular activity of the organ.

If I now submit to the same test slips from ileum and colon taken from rats fed on vitamin B deficiency, I obtain the following:—

For the ileum often irregular beats, 21 to 27 per minute, with an amplitude of 1.25 to 2.5 mm.

The periodicity is at times completely lost or so altered that the periods of comparative rest are greater than the periods of more normal beats.

In most cases the slip of colon fails to show any myogenic contractions.

The macroscopic examination reveals briefly: (a) increased incidence over the normal of dilated ascending colons; (b) the descending colon more often full of faeces; (c) increased incidence of enlarged peri-ileo-caecal glands, of small spleens, of small thymus, of enormously filled stomachs and of dilated small intestine frequently containing numerous gas bubbles. Throughout the period of feeding the average lowering of temperature from what is found in the adequately fed rats is approximately 1° F. Lastly, the adrenalin content of the adrenal glands is well above the normal, and the adrenalin content of the blood is increased as evidenced by the greater excitability of the animal, the pilo-motor reflex (the animal's hair standing up) and the greatly raised blood-pressure.

I cannot as yet make any definite statement as to the results of vitamin A and C deficiency, since I have not yet killed sufficient animals to allow of my forming any definite conclusions. But it begins to appear that vitamins A and C are necessary for the maintenance of proper bowel activity, and it will not surprise me if I find that their lack is accompanied by quite well-marked decrease in, and change from, the normal intestinal muscular activity.

What is the significance of these observations as applied to the problem of intestinal stasis in the human alimentary tract?

Several inferences may justifiably be made. A diet deficient in vitamin B produces well-marked evidence of general intestinal stasis and particularly of

iliac¹ and colonic stasis. In addition, vitamin B cannot be stored to any extent in the human body but requires daily ingestion. I have found that its absence from the diet for one day produces an easily recognizable diminution of intestinal activity and upset in the adrenalin equilibrium in the adrenal glands. This deficiency may become a factor of considerable importance in the production of stasis. For it is conceivable that not only may individuals be deprived of this vitamin for long periods—since vitamin B does not occur plentifully in the average daily dietary, but also that whole communities may actually be living on the verge of a complete lack of a vitamin without its loss being realized. Not long ago it required the failure of only one potato crop to precipitate a large number of cases of scurvy in Manchester, Newcastle and Glasgow, and what is possible in the case of vitamin C may at least hold equally good in that of vitamin B.

If these suggestions are even tentatively accepted we arrive at an explanation of the curious lesion which I have already described as occurring in a number of colons removed from cases of colonic stasis, namely, the accumulation of large pigment-bearing cells confined to the subepithelial stroma of the colon. Gunn and Underhill showed some time ago that adrenalin causes *in vitro* a tonic contraction of the muscularis mucosæ, and this I have been able to confirm. Now the lymphatics from the subepithelial stroma pass through this muscular layer and such a tonic contraction due to the increased adrenalin content of the blood would tend to constrict the lumina of the lymphatics as they pass through the muscularis mucosæ, cause lymph stasis (hence the lymph lakes) and prevent these large overladen cells from being carried off to the general circulation. This may also account for the "soggy colon" so often found in chronic intestinal stasis. If we add to this the vicious circle which is set up with all its sequelæ, we are faced with a very tempting hypothesis.

As my experiments are yet not completed, I will not dogmatize as to the frequency with which this mechanism is the cause and explanation of "constipation" as it is clinically observed; but in confirmation of McCarrison's work the consideration of vitamin deficiencies in the diet as an ætiological factor in the production of at least some cases of this condition is strongly suggested by the results which I have so far obtained.

Mr. J. P. LOCKHART-MUMMERY.

I have never believed that the colon in man is a useless excrescence. I am convinced that the colon is a very useful organ and that apart from its function in conserving the amount of fluid within the body—which at one time must have been of great value, when prehistoric man had to cross tracts of country where there was no water—the colon has other functions of the greatest value to civilized man living in communities. Many of these functions are only vaguely guessed at, even at the present day, but we know that the colon has important excretory functions, and that certain substances are mainly excreted through the walls of the colon. There is good reason to believe that many of the hydrocarbons, such as chloroform and ether, are largely excreted by the colon, and also many other poisons, including arsenic and mercury. An interesting instance of this has occurred within my own experience.

The patient was a young woman whose colon I removed some five years

¹ This is best seen by comparing the figures obtained by multiplying rate by amplitude. In vitamin B deficient rats it is roughly 45, in normal rats it is 56·25.

ago for multiple polypi of the colon. She was attached to one of the Dogs' Homes in London, and last February she came up to the hospital evidently very ill with vague symptoms which did not point to anything very definite, but suggested yellow atrophy of the liver. On careful inquiry it was found that she had been killing dogs with chloroform at the rate of from thirty to forty a day for some time. I have come to the conclusion that she was suffering from chloroform poisoning, largely due to the fact that she was not able to eliminate the drug satisfactorily owing to the absence of her colon.

Sir Arthur Keith asked if anybody had seen a patient who had died from intestinal stasis, who had not been operated upon for the condition. I have seen such a case. The patient came from Southern Ireland and was a man from 30 to 40 years of age. He had had no real treatment and when seen his bowel refused to act satisfactorily with any kind of treatment. The patient exhibited in an extreme degree all the symptoms of intestinal stasis. Although he was perfectly clean he smelt so badly it was hardly possible to sit in the room with him. Owing to the fact that he had almost complete obstruction, I had to do a cæcostomy. Up to the time the patient died, some six weeks later, the wound in the abdomen showed absolutely no signs of healing or of suppuration; recuperative function was entirely lost and the wound had to be sewn up some five or six times. Never before, or since, have I seen this condition of failure of a wound to heal except in very old, asthenic people.

I believe there is a definite class of case suffering from intestinal stasis for whom a colectomy is the only satisfactory treatment. In these patients the large bowel has so completely degenerated that nothing short of its removal will offer any hope of recovery. In my own experience such cases do well after colectomy. In one case I had sections made from the cæcum, the transverse colon and sigmoid flexure and none of the sections showed any signs of muscular tissue whatever. The whole of the colon consisted simply of a fibrous sac lined with atrophied mucous membrane. It is unreasonable to expect such a condition to be curable short of removal of the colon.

The operation should only be done in cases in which extreme stasis exists and in which there is evidence that the colon wall has degenerated. For instance, if X-rays show well-marked segmentation of the colon it may safely be concluded that the muscular tissue is still present and functioning, and in such cases I do not consider that colectomy is indicated.

Patients ought never to reach the stage at which a colectomy becomes necessary, and I believe that in the future, when the days of empiricism are past, and scientific medicine has come into its own, colectomy for stasis will only be necessary when somebody has blundered.

MR. E. G. SLESINGER.

Dr. Hurst has given figures which he states are the Guy's Hospital cases over a number of years. I note that between 1909 to 1913 he finds fifty cases with four deaths. I have obtained fifty cases done in 1912 and parts of 1911 and 1913, so that the figures for the period 1909 to 1913, at any rate, cannot be correct.

For the purposes of this meeting I have endeavoured to follow out fifty cases of total colectomy for chronic intestinal stasis performed by Sir Arbuthnot Lane in 1912 and parts of 1911 and 1913. I assisted at most of these operations, and have been in touch with many of the cases since. Unfortunately during the war I lost the address book of the male cases

operated on during the same period. They were considerably fewer, but my impression is that though the mortality was possibly a little higher, the results were just as good. Anyone who has endeavoured to trace cases since the war will realize how difficult it is owing to frequent change of address, so that I feel that the proportion of this series untraced is not greater than one must expect after an interval of roughly ten years.

Before giving the figures which I have found in answer to my inquiries I would call attention again to the fact which has to be considered in all operative statistics, namely, that all mortality and result figures depend on what is generally an unknown factor, and that is the operability figure. It is easy to have a high percentage of success if only the good surgical risks are operated on, and, as W. J. Mayo has pointed out, if the operability is increased, it may bring with it a relatively higher mortality; but the actual number of patients relieved will be higher, even though the percentage may be lessened. It is not easy to give a verbal impression of the pre-operative condition of many of the cases in the present series. All of them had been chronic invalids for years, and most of them had suffered much at many hands, both medical and surgical. A considerable number were really extreme surgical risks, and anyone who, like myself, saw these cases before operation cannot but be amazed at what was, in many instances, literally a resurrection.

Of the fifty female patients on whom colectomy was performed in 1912 and part of 1911 and 1913, two died from the operation—one from hæmorrhage and one from a pulmonary embolus, giving an operative mortality of 4 per cent. One other patient, some months after operation, was readmitted to hospital and died of acute intestinal obstruction, giving a mortality from operation and one year afterwards of 6 per cent. Of the remaining forty-seven cases all were written to, but only a further twenty-five could be traced. Of the twenty-two cases not traced, I have no reason to suppose that the results were any less favourable, as in my experience it is usually far easier to keep in touch with failures than with successes; and many of these twenty-two were seen and were well a year or more after operation.

The twenty-five cases in which the patients have been traced I have divided into four groups:—

(A) Those who are quite well in all respects and live a normal life at work or otherwise—eighteen cases = 72 per cent.

(B) Those who are definitely better, but are still suffering in certain ways—three cases = 12 per cent.

(C) Those who are no better for the operation—two cases = 8 per cent.

(D) Two cases who have died since—one in 1915 and one in 1919. In both cases the relatives state they were much better for the operation in the intervening four and seven years—two cases = 8 per cent.

I will not occupy your time by reading these letters in full, though they are available for those who care to see them; but certain striking features in their letters are worth recording. Of the eighteen patients who write that they are quite well:—

(1) Runs a busy boarding house.

(2) States that she was digging up her allotment when her letter arrived.

(3) Was conducting a choir and singing solos at an Eisteddfod.

(4) Has been married and widowed since operation, and has a baby 6 years old and does all the work for herself and child.

(5) Writes: "I could not possibly wish to feel better than I do." She is in domestic service.

(6) States she feels quite well and has put on weight from 8 st. to 12 st. 12½ lb.

(7) Has married and been living happily in Pretoria for three years.

(8) Has been working in a Government office for several years and has had no sick leave.

(9) Is back at her original work as a children's nurse, having been an invalid for some years before operation.

(10) Is well and has gained weight from 7 st. to 9 st. 6 lb.

(11) Patient who had phthisis and a tubercular tarsus states: "Am better than ever in my life, no trace of tubercle, and working as housekeeper to a friend."

(12) Patient who exhibited microbic cyanosis, and who weighed 4 st. 6 lb. in 1911 is now 10 st. 10½ lb., and states: "For years I have played tennis, danced, rowed, and done everything a normal person does. I have been happily married for five years." This case was reported in the *Practitioner*, June, 1911.

(13) Patient was a doctor's daughter, and he writes: "She is now in perfect health."

The five other patients state that they feel quite well, and three of them have married since operation. These are all patients who were chronic invalids—in many cases bedridden for several years before operation.

Of the three cases partially relieved two are well between attacks of what they term "flatulence." During these attacks they have pain and diarrhoea, which certainly means that adhesions are present. One patient states that she has had pain and indigestion, but is now able to do light work at a Y.M.C.A. hostel.

Of the two remaining patients who are no better, one is apparently suffering from severe chronic intestinal obstruction, and the other patient stated to her doctor, who wrote to me, that she was no better and suffered very great pain. Her doctor adds that she is able to do all her housework, though she says she does it "in agony."

Apart from the three cases only partially relieved, none of the patients have troublesome diarrhoea—in fact, many still take paraffin as a precaution. Only two of the patients in the successful cases state that they occasionally have diarrhoea for a day or so, but not enough to trouble them. Even if we class the two cases who died four and seven years after operation with the failures, the figures in this series are striking enough. Here we have a group of patients, most of whom were at the end of their tether, and ten years after operation 72 per cent. are perfectly well in all respects, 84 per cent. are well, or very much better, and only 16 per cent. are either no better or have died since. Many of these patients were the derelicts of the medical wards; all of them had endured years of misery and unsuccessful treatment and most of them had undergone previous operations. I think the facts speak for themselves, and Sir Arbuthnot Lane is to be congratulated on an ample vindication of the brilliant success of the operation in these cases. It is striking that in several of these cases the patients state that it was eighteen months or two years after operation before they regained their full health, though they were improving all the time. Another interesting fact consists in the high proportion of patients married since operation. Anyone who saw the condition of these patients before operation would have put their chance of marrying only second to that of their recovery—and yet both recovery and marriage occurred in a high proportion of cases. I have not obtained the actual figures of cases

in [which only short-circuiting was performed, but I have a very definite impression from the cases I have seen that the results are not so good, and from the experience of these and other cases there can be no doubt that colectomy is associated not only with a mortality which is no higher, if as high, as short-circuiting, but very definitely with a far better prospect of permanent relief.

I think the figures in this group of cases and the details of the patients' condition which I have given are an adequate vindication of the *results of colectomy*, which I was under the impression was the subject under discussion.

MR. TYRRELL GRAY.

In the investigation of the value of the operations under discussion, there are two essential preliminary requirements: (1) To ascertain the exact changes which take place in the intestine at and in the neighbourhood of the anastomosis, and (2) to group as far as possible the cases for which such operations are performed; for the term "intestinal stasis" includes a number of pathological processes each having a different pathology.

For some time past I have been investigating both clinically and experimentally the remote fate of intestinal anastomoses with the kind help of Sir Arthur Keith; but I speak with some reserve as these studies are far from complete.

(1) There is one feature common both to complete and partial colectomy, namely, that the anastomosis enlarges, in the course of time, to two or three times its original size; and, together with the adjacent large intestine, it becomes thinned, atrophied and flaccid. In hemicolectomy (when the ileum is united to the transverse colon) this atrophy involves the adjacent small intestine also for a variable distance from the anastomosis. I believe this atrophy to be due to degenerative changes, the exact nature of which is not yet clear. After complete colectomy on the other hand (when the ileum is united to the pelvic colon) the small intestine becomes progressively hypertrophied and distended in marked contrast to the partial operation. I think this feature is less marked the greater the distance between the rectum and the anastomosis. In the partial operation I believe the voluntary control over the anal sphincter (even if intermittent) constitutes a definite mechanical obstruction. In the absence of its normal protective mechanism (the ileo-cæcal valve) the small intestine responds by progressive hypertrophy and dilatation; and this masks the degenerative changes previously referred to. The establishment by operation of a mechanical obstruction on the lower ileum is to impose upon the intestine a function which it was never intended to fulfil; and this constitutes in itself so serious an objection to complete colectomy that the conditions for which it is indicated must be very rare.

Such X-ray observations as I have had the opportunity of making appear to support these deductions. For, in the marked ileal delay after complete colectomy, the shadow joins that of the large intestine; whereas after hemicolectomy, the ileum adjacent to the anastomosis is often empty, the delay being observed in a loop more remote from the colon. Such degenerative changes would be quite sufficient to account for the gradual modification of the post-operative diarrhoea (or loose and frequent stools) to the normal, and ultimately in many instances for a return to the original constipation. Flatulence, dyspepsia and a sensation of wind which will not pass, are not

infrequently complained of, even when the general condition is greatly improved; and these are probably attributable largely to the same processes. That the remote results of hemicolectomy are often unsuccessful or only partially successful (in my experience only about 30 per cent. can be said to be entirely successful) is most disappointing, particularly when the brilliance of the immediate results is so encouraging.

(II) These sequelæ to hemicolectomy must, therefore, be weighed against the symptoms of the condition for which the operation is performed. Of the surgical causes there are at least three main groups:—

(1) *Obstructive lesions* (partial or complete), e.g., carcinoma. It is in this class that the results of partial colectomy are almost uniformly good. We should remember that, though the disease produces certain symptoms for which advice is sought, mutilating operations of the character under discussion must necessarily set up sequelæ due to (1) the trauma to important and sensitive structures, as well as (2) to the degenerative changes already referred to. An important factor in the surgical cure will be the relative severity of these two kinds of sequela. In this class, local symptoms so predominate that the milder sequelæ of the operation are hardly appreciated, and the cure is complete.

(2) When inflammation from the lumen of the gut invades the neuromuscular tissues, their function becomes impaired. The results of hemicolectomy in such primary lesions of the intestine are fairly good when the gut is hopelessly diseased, since the operative sequelæ are in no sense comparable. But the cure is not so complete as in the obstructive lesion, since the original symptoms are not so strictly localized.

(3) In the large majority of instances I believe colon stasis arises in the subjects of marked enteroptosis from the failure of the normal supports of the bowel, and from the consequent assumption by the mesentery (together with its contained vessels and nerves) of this function, which it was never intended to fulfil. On the one hand the resulting constant stimulus to the sympathetic nerve fibres inhibits intestinal movement; so that the muscle, unable to contract against distension, becomes progressively thinned and atrophied. On the other hand the most collapsible structures (i.e., the veins) share in this abnormal tension, and the impeded venous return leads to a venous stasis, which still further impairs the muscular action.

Here the intestinal failure is not due to primary intestinal disease; and, if hemicolectomy is performed, not only do the degenerative changes and operative sequelæ establish their own symptoms, but there is nothing to prevent the original factors continuing to inhibit the muscle of the anastomosis and of the remaining intestine. The unpleasant sequelæ referred to gradually develop, therefore, in the course of time; and in my experience this is to be noted to a greater or less extent in 70 per cent. of cases.

These considerations not only account for the indifferent remote results of hemicolectomy for this type of colon stasis, but have led me to abandon it for all cases except those in which the gut is hopelessly diseased.

Lastly, in view of the extraordinary power of recovery of the intestine both from trauma and disease, I believe that hemicolectomy should be practically reserved for primary gross incurable lesions; and that the occasions when it is performed for stasis will become rarer as our knowledge progresses, and as the primary causes of the intestinal disability are more understood and treated in the early stages.

Section of Orthopædics.

President—Mr. E. LAMING EVANS, C.B.E., F.R.C.S.

Case of Arthroplasty of both Hips.

By O. L. ADDISON, F.R.C.S.

A. L., GIRL, aged 15. First seen, May, 1915, with ankylosis of both hips, numerous abscesses and sinuses present. Condition followed attack of "rheumatism," November, 1914.

Murphy arthroplasty right hip, August, 1916. Leg put up in abduction three to four weeks. Free flexion to right angle and movement painless.

Murphy arthroplasty left hip, January, 1918. No voluntary movement after operation; a few degrees only under anæsthetic.

Patient not seen since about March, 1918, when no movement present in left hip, until March 30, 1922.

X-ray, March 30, 1922: Complete absorption head and neck right side. Very small part of neck remaining on left side in acetabulum.

Free flexion and good abduction right side. Flexion to right angle of left with about 30° abduction. Never has any pain or trouble in walking.

This case suggests that arthroplasty has no advantage over excision.

Case for Diagnosis.

By P. MAYNARD HEATH, M.S.

A. B., MALE, aged 42, discharged from Army in January, 1919, with the diagnosis "osteo-arthritis, hip, left." The trouble came on in 1918 while marching. He complained of weakness in the left side and pain in the left loin. He still complains of shooting pain in the left hip and thigh but is able to do light work.

Examination shows prominence of the left great trochanter, and some lordosis. The left thigh is slightly adducted, but movements in the hip-joint are smooth and free except for slight limitation of flexion. There is no shortening of the femur. The condition does not appear to be progressing.

X-rays show that the upper extremity of the femur is normal in appearance but that the head of the bone is displaced upwards and outwards so that half the upper surface of the head is uncovered by acetabulum. The upper lip of the acetabulum is also poorly developed. There is no evidence of osteophytic outgrowth. Partial relief of symptoms has been obtained by raising the boot ½ in. on the unaffected limb.

Two Cases of a New Metatarsal Disease.¹

By PAUL BERNARD ROTH, F.R.C.S.

Case I (the patient shown).—Mrs. L. D., aged 37. First seen on February 2, 1922. Complaining of pain and swelling in right foot for five weeks. X-ray showed a swelling on both sides of shaft of second metatarsal bone. Wassermann reaction negative. No history of injury. Treated by rest and pot. iod. 3 gr. t.d.s. Now, April 4, 1922, much improved, but not completely cured.

Case II (note and radiograms only).—Nurse C., aged 22. First seen on December 6, 1920, complaining of pain and swelling of dorsum of left foot at base of second and third toes, for one week. X-ray showed swelling on outer side of shaft of second metatarsal bone, at junction of middle and distal third: and also a very slight swelling at junction of proximal and middle thirds. Wassermann reaction negative (twice). No history of injury. Treated by five weeks' rest at seaside with foot up, and pot. iod. 3 gr. t.d.s. Complete cure with bony consolidation (*see X-ray*).

Case of Arthritis of the Hip in a Girl, aged 9.

By B. WHITCHURCH HOWELL, F.R.C.S.

WHEN 6 months old, the patient had an abscess of the left hip-joint, which was drained—as shown by scars in the ham and groin—at St. Bartholomew's Hospital. In February, 1922, she came to me for a high boot. The left lower limb was 1 in. short, and there was adduction of the femur, with flexion deformity of 60°. The hip was stable, with a fair range of movement.

X-ray: Destruction of the head, with broadening of the upper end of the femur.

Points raised: (1) Should a sub-trochanteric osteotomy be done? or (2) Should the anterior superior iliac spine be detached, with extension? or (3) Should (1) and (2) be done?

¹ Described by Deutschlaender, *Brit. Med. Journ.*, December 3, 1921.

Section of Orthopædics.

President—Mr. E. LAMING EVANS, C.B.E., F.R.C.S.

Case of Crush Fracture of Tenth Thoracic Vertebra.

By E. LAMING EVANS, C.B.E., F.R.C.S. (President).

CORPORAL W. B. C.—In action, November, 1916, patient was struck a violent blow on the right shoulder and then in the small of the back; he became unconscious, and says he was left on the ground for forty-eight hours. Part of this time he was conscious of his surroundings but dared not move as the ground was being shelled. He states that he had control over his limbs, and that there was no disturbance of the functions of the bowel and bladder. It is likely, the patient thinks, that the blow in the back was the result of a shell bursting near by. He suffered from pleurisy and (?) pneumonia afterwards, and had to be taught to walk after his four months in bed. This was at Lincoln, and he states that his disability to walk was purely due to weakness. He never lost the power of moving his limbs at will.

He now complains of aching in the back, in the angular curvature present in the lower dorsal region. For this reason he wears a spinal jacket. Knee-jerks, +; plantar reflex flexor; can walk up to four miles without too much fatigue.

Non-union of Ulna after Operation.

By D. McCRAE AITKEN, F.R.C.S.

AUGUST, 1918: Gunshot wound, fracture right radius.

February, 1919: Operation to shorten ulna (surgeon not known).

September, 1921: Admitted Shepherd's Bush. Non-union of both bones. Deviation to radial side and flexion at fracture. Flexors of wrist very short and strong. No voluntary extension of wrist.

Treatment: Operation. Elongation of tendons of ulnar and radial flexors. Full supination.

Points of special interest for which the case is exhibited: (1) Union of radius after correction of alignment and erroneous direction of muscle action, without any operation on bone. (2) Operation on ulna was at site of Gallie's operation for producing pseud-arthritis: a site at which non-union is specially prone to occur.

Case of Renal Dwarfism.

By DONALD PATERSON, M.B.

PATIENT, a boy aged 6. History: When aged $2\frac{1}{2}$, it was noticed that he was getting "knock-kneed." Has progressively become more and more deformed since. Height, 35 in.: the normal height for his age being 44 in.

[May 2, 1922.]

Interstitial nephritis present. Urine: Specific gravity 1004 to 1014; cloud of albumin always present. Blood-pressure 100 mm. Hg. Urea concentration test: Maximum output, 1.9 per cent.; normal 3 to 4 per cent. Eyes: No change.

X-ray examination: (1) "Woolly" appearance of diaphyses at wrists. (2) Fracture and irregularity of epiphyseal line at lower end of femur.

It is suggested that the agent which brought about the renal fibrosis also caused the bony changes. This agent is not dietetic as the patient's diet has contained plenty of both antirachitic and antiscorbutic elements.

Changes are present at birth in some cases. Fractures are almost certainly due to deficiency of osteoid tissue and absence of lime, bony trabeculæ being replaced by trabeculæ of fibrous tissue especially near the epiphyses.

Accessory Bone representing Tubercle of Scaphoid of Foot.

By R. C. ELMSLIE, O.B.E., M.S.

PATIENT, a school-girl, aged 12. Pain first noticed over right scaphoid tubercle twelve months ago; it came on after skipping. Talipes valgus right and left.

X-ray shows accessory bone right and left. Right foot larger.

Madelung's Deformity of Left Wrist.

By R. C. ELMSLIE, O.B.E., M.S.

PATIENT, a male messenger, aged 17. Condition noticed for two years. No injury. Deformity of spontaneous occurrence.

Case of Curious Deformity of Ulna following Injury.

By H. A. T. FAIRBANK, D.S.O., M.S.

A. A., BOY, aged 13. Six years ago fractured right ulna. Hand displaced bodily to ulnar side, ulna being very short. In lower third of forearm a piece of bone was felt projecting forwards among flexor muscles.

Radiogram: Ulna shaft much shortened and having a process of bone nearly an inch long projecting forwards from the lower extremity. Epiphyseal line is oblique; centre for the lower epiphysis is of good size, but abnormal shape. On the tip of the piece of bone projecting forwards is a separate piece of bone, apparently a detached portion of epiphysis. The radial epiphysis is set obliquely on the shaft, corresponding with the ulnar displacement of hand. It is suggested that the condition is the result of splitting of ulnar epiphysis and shaft, deflected portion having grown, while growth at main portion of epiphyseal line has been deficient.

Treatment suggested is removal of piece of bone projecting forwards, and lengthening of ulna.

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Section of Surgery.

President—Mr. RAYMOND JOHNSON, O.B.E., F.R.C.S.

DISCUSSION ON DIATHERMY IN SURGICAL PRACTICE.

Mr. W. H. CLAYTON-GREENE.

DIATHERMY is a method of treatment which, we admit, is under trial. It was introduced into this country largely through the agency of Mr. Douglas Harmer and Dr. Elkin Cumberbatch, to both of whom I am much indebted for the help they have given me in showing how the method is to be applied and in advice as to certain details of its application.

The principle of the method is, as you know, that of a cautery, but with this difference, that whereas with the cautery the destruction is limited to an area practically in contact with the cautery point, in diathermy, as used for surgical purposes, the area of destruction is enormously increased. There are two poles—the passive pole, which is connected to the patient's body by means of a pad impregnated with salt solution, and the active pole, or electrode, to which points of varying size and shape can be attached. And the chief advantage in applying this method, as opposed to the cautery, is the great ease with which the current from the active electrode can be directed to growths in relatively inaccessible positions.

I shall discuss, this afternoon, two regions of the body to which the method has been applied fairly extensively—the oral and the vesical regions, and, after considering the various details in connexion with the application of the current, I shall give you a brief résumé of my experience as to its results.

The Oral Region.

It was my great dissatisfaction with the results obtained by the ordinary operative methods in carcinoma of the tongue, tonsil, and floor of the mouth, that led me to adopt diathermy as an alternative. I admit that in an early case it is probable that the resection of the affected portion of the tongue is just as efficient as the diathermic current; but in the more advanced cases I believe that diathermy has the following advantages: (1) It does away with any question of cancer infection of the wound. (2) It allows of the destruction of a much wider area of tissue than can be attained by the knife. (3) It is a method applicable to cases which are otherwise inoperable—i.e., inoperable unless one attempts a procedure of such magnitude as to be extremely hazardous.

It is particularly with regard to the inoperable cases that I have had the experience, because many of my colleagues have sent me cases which they

declined to attack with the knife, asking me to do what I could to alleviate the sufferings of these patients; and my experience is, that first there is the moral effect on the patient when he feels that something active is being done in the way of destroying a foul ulcer in his mouth; secondly, there is the undoubted physical benefit which accrues, because quite a number of cases of extensive epithelioma are retarded in their growth, and actually healed, after the application of this cautery. And, finally, in the majority of cases, there is a suppression of the pain of which the patient so often complains.

Disadvantages of the Method.

The disadvantages of the method, apart from the fact that it requires a somewhat complicated apparatus, are these:—

First of all, it is advisable to ligate any of the main vessels, such as the lingual, that cross the route traversed by the cautery. Some surgeons assert that the mere prolonged contact of this escharotic current is sufficient to obliterate a vessel of the magnitude of the lingual, but I have never relied upon that, as, I understand, a considerable number of cases of secondary hæmorrhage have occurred. So in all patients in whom it is necessary to remove a portion, or the whole of the tongue, or the floor of the mouth, I invariably, as a preliminary, tie the lingual arteries. It has not been necessary, in any of my cases, to tie the carotid. In the application of the current to the tonsil I have generally been able to confine its activity to a safe area, except in one instance, in which, later on, from recurrence of the growth, the carotid became invaded, and the common vessel was tied.

The second—and this is perhaps the most important—objection is that the wound is, of necessity, septic; we produce a foul slough in a foul cavity. And although, in cases submitted to operation in the ordinary way, a certain amount of sepsis is more or less inevitable, I admit that the amount of sloughing appears at first sight to be a detrimental factor in the application of the method. When we come to judge by results, however, it is interesting to note that in only one case—and that case will be referred to later—has any ill effect resulted from the presence of this large, extensive slough.

The third objection is the fact that bone touched by the point of the cautery will necrose, and the amount of time necessary for the separation of such necrosed bone usually amounts to several weeks; during that time the patient has to be scrupulously careful of the cleanliness of his mouth lest any septic complications should supervene. On the other hand, this necrosis may be regarded as an advantage, since it destroys that portion of the bone which is approximated to the cancerous area, without unnecessary mutilation; and therefore at the same time kills any cancer cells which may have invaded that particular portion of the bone.

The last objection is a personal one; it is that, in operating, a considerable amount of care has to be exercised, both by the operator and by the manipulator of the machine; otherwise the patient or the operator, or both, may be burned. So far, none of my patients have been burned, but on one occasion I burned myself very considerably, and the burn took some time to heal.

Method of Attack.

The next question in the discussion is: How should these cases be approached? That is to say, should they be treated in one or in two stages? Assuming that an extensive dissection of the neck is necessary for the removal

of glands, it obviously seems unwise, on first consideration, to produce at the same time an admittedly septic condition of the mouth with a foul slough. On the other hand, it is impossible to deal with the case by local treatment in the oral region first, with subsequent dissection of the glands, owing to the risk of secondary hæmorrhage, and because of the inflammatory infiltration which occurs. I am becoming more and more doubtful as to the value of the big gland dissections practised in cases of carcinoma of the tongue. If the glands are not involved, the operation is unnecessary; and if they are extensively affected, it does not seem to prevent recurrence, and I believe that is the experience of many of my colleagues. If a moderate dissection is done in which obviously enlarged glands, such as the carotid group and the submaxillary triangle group are cleared, and the lingual and the facial arteries are tied, I do not find that there is very great disadvantage in completing the operation by destroying the cancer in the mouth. But if the dissection is of wider range, then I should hesitate to do it.

In contrasting cases in which there has been an interval between the removal of the glands and the ligation of the vessels and the subsequent destruction of the carcinomatous area, I have been impressed by the fact that the recurrence seems to take place more certainly in those in which the operation is done in two stages. I am inclined to think that an actively inflamed area draining into glands infected with cancer cells is of some benefit.

There has been some discussion lately upon the effect of radium locally in cases of glandular infection secondary to epithelioma of the tongue; and Quick, of New York, is emphatic in stating his experience that the glands are protective, and can be dealt with *in situ* by the embedding of radium tubes. Judging from the very unsatisfactory results obtained by the ordinary methods of surgery, I am becoming more conservative in my treatment of the glandular areas, and it is possible that the active suppuration associated with the separation of the slough and with the subsequent hyperæmia and irritation of the glands may be beneficial.

Results in a Series of Cases.

With regard to the results, thirty-one cases have been submitted to operation. There were no complications. There was one death from bronchopneumonia. This, in itself, is curious, when it is considered how much sloughing there is in the mouth in these cases; and I attribute the immunity which undoubtedly exists after this method of treatment to the fact that no blood whatever trickles down the trachea into the lung. The cases were all operated upon under intratracheal anæsthesia, and of course it is very necessary to stop the administration of any ether before the local attack on growth is made. But I have been struck by the absence of this pulmonary trouble and with the complete absence of pain subsequent to the most extensive destruction of what would appear to be a very sensitive region.

It is, perhaps, too ambitious to expect many cures. Most of the cases submitted to this treatment have been too advanced to allow of any of the ordinary operative methods being practised; but of the thirty-one cases there are six in which there has been no recurrence, either local or in the glands. In a certain number there has been a local cure, so far as the primary focus is concerned, but the glandular enlargement has continued, and has subsequently led to the death of the patient. But, with few exceptions, there has been alleviation; and I would lay great stress on this side of the question in diathermy. As I said before, the moral and physical value is very great.

In one case only was there failure to alleviate the distressing pain and discomfort from which the patient suffered, and that was in a case of my colleague, Mr. Low; in this there was a very extensive growth, involving the floor of the mouth. It was in a woman, and, as you will agree, inexplicable though it is, cases of epithelioma of the tongue seem more malignant in this sex than in men. In that instance I did no good at all. She got little benefit, and then there was a rapid return of the growth locally. But, with that exception, I am satisfied that I have supplied comfort to many sufferers by this method when no other active treatment was available.

The Vesical Region.

The second region in which I have used diathermy has been the bladder. In this region the method has been more popular and more widely applied than in the mouth. The number of these cases in which I have used it is comparatively small, and many others here present are more competent to deal with this part of the discussion than I am. But, as far as my experience goes, the small localized growths seen early are efficiently treated and well controlled by intravesical cystoscopic methods.

When the growth is very extensive, the alternative will lie between resection of the bladder wall and destruction of the papillomata through a suprapubic opening, and this is the method that I have practised in some six cases. In all of them the growth was advanced and an extensive area of the bladder was involved, which would have necessitated a wide resection, and in some of them the technical difficulties in the operation would have been considerable. It is admitted that one of the unsatisfactory features of operations upon papillomata of the bladder is their tendency to recur, not only at their original site of growth, but also from implantation infection in the edges of the wound. By widely opening the bladder, and passing down a non-conducting speculum, the growth is adequately exposed, and it can be destroyed there and then with considerably less risk of cell-infection of the wound than if it is to be excised with subsequent suture of the rent in the mucous membrane. Of the six cases which I have treated by this suprapubic method, one patient died as a result of a too-prolonged operation in a very unsuitable case; all the others have done singularly well. The scarring of the bladder does not appear to interfere with the function of the ureters, even when the cauterized area passes over the ureteric opening, and the *restitutio ad integrum* has been surprising.

Conclusion.

In conclusion, I am not urging that this method is one which should be adopted in preference to any other; I am simply giving you a frank expression of my experience in connexion with the method over some three years. I think it has a great deal to commend it. It is not going to cure impossible cases; it very likely is of no more value in early cases than local excision, but in the large group of inoperable cases, in which the surgeon has to turn away, reluctantly admitting his inability to operate with any chance of success, then the method of diathermy supplies a want in the most admirable manner, and gives relief where no other relief but morphia can be found.

Mr. DOUGLAS HARMER

said that the credit for having introduced diathermy into this country should be awarded to the late Dr. Lewis Jones, who induced Professor Nagelschmidt to give a demonstration with his own apparatus at St. Bartholomew's Hospital. Professor Nagelschmidt successfully treated three cases: (1) a nævus of the tongue; (2) a post-nasal tumour; and (3) a case of ordinary enlarged tonsils. The patient suffering from post-nasal tumour lived for two years. In Mr. Harmer's own experience, diathermy was the best treatment for growths of the upper air passages; excision by the knife in these cases yielded only 33 per cent. of cures, and recurrences were almost invariably in the scar, suggesting that dissemination had taken place at the time of operation. He then described a number of cases in which he had successfully employed diathermy, notably an endothelioma at the root of the nose, and a large rodent ulcer involving the whole orbit. For carcinoma of the floor of the mouth he practised excision, followed by diathermy applied to the wound by a plate or button electrode. If the glands were not involved he did not perform any operation in the neck, and he had found the prognosis very good. He had one patient still living eight years after this operation, who presented no signs of secondary glandular involvement. For carcinoma of the tongue he practised excision with the diathermy knife. Ligature of vessels was unnecessary in this operation. For carcinoma of the tonsil, in which surgery admittedly failed, diathermy gave relief from pain, increased mobility of the mandible, and prolonged life sometimes for three or four years. For growths of the upper jaw he performed Moure's operation, followed by diathermy to the wound. In the case of the cheek the use of diathermy was limited by the proximity and consequent risk of sloughing of the skin. From over a hundred cases he considered that the results were excellent. There had been two deaths from secondary hæmorrhage—not more, he thought, than would have occurred after ordinary surgical operations upon similar cases. There was a striking absence of fever or toxæmia after diathermy, and most of his cases were able to get up after forty-eight hours. The relief from pain and the smooth and pliable scars which resulted were remarkable. His conclusion was that diathermy should be employed in all cases of malignant disease of mucous membranes, and that it might usefully be employed also for lupus, extensive nævi, and warts.

Dr. E. P. CUMBERBATCH

said that of the cases he was showing, one was that of a woman who had a rodent ulcer of her scalp. A section showed cell nests of carcinoma. He had cauterized the edges and base by diathermy *seven years ago*. The periosteum was destroyed and a sequestrum eventually separated. Epithelium gradually grew over the bone. It would now be seen that the bone was covered, except a small area in the centre. There were no signs of any return of malignant disease.

The second case was that of a boy who had originally presented a deep brown mole which occupied the greater part of his submaxillary region. This had been cauterized by diathermy a year ago, and a soft uncontracted scar in the same region was now visible.

Mr. Harmer had spoken of the advantages peculiar to diathermy in the treatment of malignant growths and he (Dr. Cumberbatch) fully agreed with the claims which he had made out for this treatment. Even if the final results

of diathermy were no better than those given by cutting operations, the quickness of the newer method, the absence of bleeding, the relief of pain, the rarity of complications and the absence of shock and the very short stay in bed with little discomfort would render diathermy a formidable rival to the knife in the treatment of malignant disease. Diathermy was especially suitable for rodent ulcer. The invasion of bone or cartilage was no contra-indication to its use, and these tissues could be destroyed by diathermy with the same thoroughness as soft tissues.

Nævi in certain regions were also suitable for treatment by diathermy. When they lay under the skin, he thought that electrolysis was the better treatment, but when they were situated in the mucous regions diathermy was a quick and effective method of removing them. The first operation of surgical diathermy performed in this country had been conducted by Nagelschmidt, who came to St. Bartholomew's in 1910 at the invitation of the late Dr. Lewis Jones. The case was one of a pulsating nævus of the tongue and floor of the mouth in a young girl. Electrolysis had been tried previously and found ineffective. Nagelschmidt cauterized the nævus in its entirety by diathermy. He (Dr. Cumberbatch) had seen this patient some years afterwards. A soft scar without contraction or adhesion occupied the site of the former nævus. Other nævi had formed on the tonsil and pharyngeal wall. These were successfully treated by diathermy. He had treated other cases of nævus of the tongue by the same method; one application had been sufficient in each case. It was difficult to believe, after healing had taken place, that any of the tongue had been destroyed at an earlier date.

He had treated a few cases of lupus by diathermy. Nodules had been destroyed and ulcers made to heal and the usual soft non-contracting scar had been left. It would be unwise at present to speak with confidence as to the permanencē of the results, but the results were good enough to demand a serious and prolonged trial of the method.

MR. CECIL ROWNTREE

thought it indisputable that diathermy was of great value in the treatment of some forms of cancer, and said that this discussion should be of great use in helping them to arrive at a conclusion as to the kind of case most suited for its application. He had employed it in such varied situations as the rectum, the uterus, the breast, and the mouth, but had felt that up to the present his work had been largely of an experimental character.

Difficulties of technique had been considerable. For instance he had had the unfortunate experience of burning not only himself but also the patient owing to an unaccountable short circuit; but their procedures were becoming simplified by the use of wood or vulcanite specula and retractors, and increasing knowledge of the amount of current necessary and the kind of terminal most suited to the individual case.

It was in mouth cases that diathermy was most valuable. For inoperable cancers in this region it unquestionably offered a degree of relief unobtainable by other methods. He used it in all such cases and also in early growths of the pharynx, soft palate, and floor of the mouth, which did not lend themselves to extensive resection operations. With regard to the lymph glands it was important that they should be excised at the same sitting, for, when they were left to a later date, he had sometimes been disappointed to find that a good local result had been nullified by rapid enlargement and necrosis of the glands apparently infected as a result of the diathermy.

His experience in situations other than the mouth had been too limited to allow of any conclusion being drawn from the results, but he was paying some attention to the development of a technique for dealing with inoperable growths of the rectum, and much hoped that it might prove possible to do something in these difficult cases for which at present so little could be done.

Mr. W. G. SPENCER

could not subscribe to the extreme views expressed by one of the speakers. He pointed out that the scar left after diathermy was virtually a burn scar and therefore liable to become epitheliomatous. As regards the boy shown by Dr. Cumberbatch (the pigmented *nævus* case), he thought still further improvement would result from excision of the scar. He pointed out that as regards rodent ulcer, even when fairly extensive, excision with the knife in combination with plastic surgery proved very satisfactory, and left scars that were almost invisible. He congratulated Mr. Harmer on the results of his treatment of endotheliomas, but inquired whether in the cases of ulcers of the floor of the mouth the diagnosis of malignant disease had been confirmed by microscopic examination in all cases before diathermy was applied. Hitherto Butlin's 30 per cent. of cures represented the high-water mark of success in these cases. Mr. Spencer himself disliked the idea of cauterizing large areas in the mouth; he preferred to make a wound which he could suture, and obtain healing by first intention. The patch or ulcer, thus excised, could then be submitted to microscopic examination.

Mr. PHILIP TURNER

said that he had had a considerable experience of diathermy, having first employed the method in 1909. He had shown two patients treated by this method before the Clinical Section of the Society in 1911.¹ One of these was a man aged 57 who had a large rapidly growing rodent ulcer involving the whole of the right cheek and extending to the superior and the inferior maxilla. The whole of the growth was destroyed by diathermy and after sequestra had separated from both maxillæ the large wound cicatrized, leaving a wide gap continuous with the mouth. An interesting point was that the antrum which had been widely opened at the operation completely but gradually closed. Mr. F. J. Pearce designed an obturator to close the gap and to support a denture. He had lost sight of the man owing to the war, but Mr. Pearce had seen him about two years ago and there was then no recurrence. One striking fact about this patient was that emphasized by other speakers—namely, the remarkable suppleness of the scar. In conjunction with Dr. C. E. Iredell he (Mr. Turner) had also contributed to a discussion on diathermy before the Section of Electro-Therapeutics² an analysis of twenty cases of malignant disease treated by diathermy between 1909 and 1912.

He considered that diathermy was the most satisfactory method of treating rodent ulcers when, either owing to the extent or the situation of the disease, excision was impracticable. Smaller ulcers could be treated in the same way, though, as a rule, excision was the quickest and most certain procedure in these cases. In the treatment of malignant disease the best method in the state of our present knowledge was early and thorough removal, and this

¹ *Proceedings*, 1911-1912, v (Clin. Sect.), p. 95.

² *Proceedings*, 1918-1919, xii (Sect. Electr. Therap.), p. 23.

depended upon early diagnosis; but in addition to the surgical methods there were certain important auxiliaries—radium, X-rays, and diathermy; and of these he was inclined to assign the first place to diathermy.

It was especially in cases of epithelioma and carcinoma involving skin or mucous membrane, when the lesion was accessible, that diathermy could be employed and it was of the greatest service in growths of the tongue, mouth, and pharynx. When the growth could be excised he believed that this was the best way of dealing with it, but when owing to its position or extent satisfactory removal was doubtful or impossible, diathermy should be tried. A combination of excision and diathermy might often be employed with advantage. In either of these ways the primary growth might be satisfactorily dealt with, but so frequently secondary growths appeared deep in the neck at a later date even after the most careful and thorough removal of the lymphatic glands at the original operation.

There was, as a rule, but little shock after diathermy, and, when used as a palliative measure, symptoms such as pain, hæmorrhage, and discharge were greatly relieved. Even a large inoperable ulcer might heal up and remain healed for some time. The action of diathermy was different from that of an ordinary cautery, for in addition to the superficial cauterization there was a deep-seated coagulation of the underlying tissues which was not found when the actual cautery was used.

With regard to accidents he had had one case of secondary hæmorrhage and one case in which the patient had a very bad burn.

Section of Surgery.

SUB-SECTION OF PROCTOLOGY.

President of Sub-section — Sir CHARLES GORDON-WATSON, K.B.E.,
C.M.G., F.R.C.S.

Case of Retro-rectal Sarcoma (Chordoma ?).

By Sir CHARLES GORDON-WATSON, K.B.E., C.M.G., F.R.C.S.
(President).

A LADY, aged 61, was seen in December, 1921, complaining of constant tenesmus and constipation, with occasional bleeding. Examination revealed a pedunculated villous tumour on the posterior wall of the rectum, about the size of a walnut, and behind the rectum the hollow of the sacrum was filled by a large hard uniformly smooth swelling, extending upwards beyond reach of the finger.

Four years previously a very large lipoma had been removed from her thigh, and other smaller lipomata previously. I transfixed and removed the villous tumour and confirmed its innocency with the microscope.

I asked Mr. Lockhart-Mummery to see the patient with me. I was anxious to know whether I was dealing with an innocent tumour, such as a lipoma or a dermoid, or with a malignant tumour. If innocent, it was probable that the tumour could be shelled out from behind without injury to the rectum; if the tumour was a sarcoma complete removal would be practically impossible except by an abdomino-perineal excision of the rectum, and even then the chances of avoiding recurrence would be small.

With the history of removal of a large lipoma from the thigh I was inclined to make a diagnosis of fibrolipoma. Mr. Lockhart-Mummery, having in mind a somewhat similar case, was in favour of the diagnosis of a dermoid cyst. I decided to remove the coccyx, and make an attempt to shell out the tumour from behind. I found the tumour was more or less encapsuled posteriorly, and could be separated from the sacrum fairly easily. It was with great difficulty that the upper limit could be reached. Above and in front the tumour was adherent to the bowel, and was separated with difficulty. The rectum was not torn during removal, but it was clear that if the tumour was a sarcoma, as it seemed to be, removal was not complete. The cavity was lightly plugged for twenty-four hours with flavine and paraffin gauze. The tumour removed was about the size of a cocoa-nut. On the fifth day the bowel leaked into the wound, but no sepsis occurred. The wound closed down rapidly, leaving a

small faecal fistula. Seven weeks after operation a small lump could be felt behind the rectum in the situation of the original tumour. The recurrence gradually increased in size, and the liver was obviously enlarging. Two weeks later the patient became jaundiced, and complained of pain under the costal arch, where a hard lump could be felt. The patient died of uræmia in just under three months from the date of operation.

The tumour is a round-celled sarcoma, but presents certain unusual features, and it has been suggested that the tumour is a chordoma.

Mr. Gabriel has been working at the microscopical sections, and I will ask him to add a few remarks on the special features.

The interesting points in the case are:—

(1) The "tumour tendency" as evidenced by the lipomata, villous tumour and sarcoma.

(2) The rapid recurrence and high degree of malignancy of a tumour suspected of being a chordoma.

(3) The difficulty in diagnosis and its bearing on treatment.

The questions I would like opinions on are: (1) Should a portion of growth have been removed for section, and, if the tumour was proved to be malignant, followed by an abdomino-perineal excision? (2) When a large sarcoma occurs in this situation, is any operation worth while, except a colostomy to relieve symptoms?

I am inclined to hold the view that should I meet with a similar case and be able to establish a diagnosis, I would not attempt removal.

MICROSCOPICAL REPORT ON SECTIONS FROM THE TUMOUR BY W. B. GABRIEL, M.B.

Sections of this tumour have been taken from several parts, and all show much the same structure. There is a stroma consisting of fibrous tissue, which shows generally an advanced stage of hyaline degeneration, with irregular areas of hæmorrhage and of fatty degeneration. The parenchyma is seen to be alveolar in arrangement, and consists of cellular masses of varying sizes, which stain deeply with hæmatoxylin. The nuclei are rounded, packed closely together, and their variations in size, with occasional evidence of mitosis, form an indication of the malignancy of the tumour.

In one section the invasion of the muscular wall of the rectum is clearly seen. The extensive mucoid degeneration and nucleolar vacuolation, said to be so characteristic of chordomata, are not seen. In many places these malignant cells are seen surrounding circular spaces, which appear to be lined by endothelium.

Diagnosis.—I think this is either a round-celled sarcoma or, in view of the suggestion of lumina, it may perhaps be regarded as a malignant endothelioma.

Specimen showing Carcinoma of the Pelvic Colon and Rectum co-existing and causing Acute Obstruction.

Shown by Sir CHARLES GORDON-WATSON, K.B.E., C.M.G.,
F.R.C.S. (President).

PATIENT, a male, aged 60, admitted to hospital March 31, 1922.

History: March 28, 1922. For the past four days there has been abdominal pain, which has increased in severity. Vomiting has been frequent, and

nothing has been passed. Has always been slightly constipated, and thinks he has got thinner recently. No blood or mucus noticed in stools.

Condition on admission : Looks ill. Tongue furred. Abdomen distended, and does not move on respiration. Slightly tender to palpation all over. *Per rectum* : A growth felt on anterior wall, within easy reach of the finger.

Operation at once, March 31, 1922 : Incision through right rectus as for colostomy. On bringing out the pelvic colon a hard constricting growth was found in the summit of the loop. The growth, which had been felt *per rectum*, was palpated from above. The loop of pelvic colon, with the growth, was brought out and fixed. A Paul's tube was tied into the bowel above the growth and a catheter into the bowel below the growth.

Progress : Very little faecal matter was passed by the tube, and the abdomen became more distended. The patient died on April 4, 1922.

Post-mortem : Great distension of all the alimentary tract. General peritonitis present. No leak found. Congestion and oedema of bases of both lungs.

Report on specimen removed post mortem : Two growths are present, one $3\frac{1}{2}$ in. above the anal sphincter and the other 6 in. higher up in the pelvic colon. The rectal growth consists of a circular shallow ulcer, $1\frac{1}{2}$ in. in diameter, with thickened, raised and slightly irregular edges. It has led to no constriction of the bowel, and is situated in a relatively thin-walled and dilated portion of it. The colic growth is evidently older. It forms a hard annular constriction, capable of admitting a lead pencil, but little more, and about an inch in length. This growth invades the muscular coat of the bowel and the mesenteric attachment, and is ulcerated on its mucous surface. The wall of the bowel above this growth is thickened from congestion, and also from muscular hypertrophy, but there is little dilatation. There is some evidence of peritonitis. No affected lymphatic glands were found in the preparation of the specimen. Histologically both of the growths are columnar-celled carcinomata.

Case of Epithelioma of the Anus.

By J. P. LOCKHART-MUMMERY, F.R.C.S.

PATIENT, a gentleman, aged 69, who had complained of pain at the anus for two months. On examination he was found to have an epithelioma on the anterior margin of the anus and extending for a little over 2 in. up the anterior wall of the rectum. There were no palpable glands in either groin and no sign of any secondary deposits. The growth was a typical squamous-celled epithelioma.

I decided to remove the entire rectum and considered it safer to clear both groins and Scarpa's triangles. A specimen was first of all removed for microscopical examination by Dr. Carnegie Dickson, who confirmed the diagnosis, and a preliminary colostomy was performed. I then proceeded with the operation and began by clearing first the right and then the left groin, together with an elliptical piece of skin, and a strip of skin right down to the perineum. All the glands and tissue and fat were cleared out of the groin and the whole of Scarpa's triangle right down to the fascia : this being performed first on one side and then on the other. The area removed was as shown in the diagram exhibited. This involved a very extensive dissection. The anterior wounds

were then sewn up and covered with dressings, and the patient was turned over, the removed glands and skin being turned down, as they were still left attached by strips of skin to the front part of the perineum. The incisions were then extended backwards around the anus with a large sweep, and the rectum was completely removed and everything cleared in the pelvis. The wound was all brought together without drainage, and the patient made an excellent recovery, without any complications. The anterior wounds healed in a most satisfactory manner and he returned home a month after the operation.

The whole of the parts removed were sent to Dr. Carnegie Dickson who made a most careful examination and cut a great number of glands. It is very interesting to note that while he did not find any secondary deposits in the glands on either side, there were lymphatics blocked with malignant cells at a spot half-way between the anus and the left groin, and it is clear that unless I had done this complete dissection and removed a strip of skin on either side containing the whole of the lymphatics, these would have been missed and an inevitable recurrence would have occurred.

Completely to clear both groins and Scarpa's triangle in a case of epithelioma of the anus renders the operation a very formidable one, but I think this case is worth recording as showing the extreme importance of doing this in cases of malignant disease in which the groin is involved.

Dermoid Cyst of the Rectum.

Shown by RODNEY MAINGOT, F.R.C.S.

Half of a cystic tumour from the rectum. It is made up of four distinct cysts joined together but not in communication with one another. In the fresh state all the cysts contained white caseous material which has been washed out in order the better to display the inner aspect. One cyst, which is smaller than the other three, contains a quantity of hair. The tumour was attached to the anterior wall of the rectum by its upper extremity and there was no pedicle.

History: Passed per rectum by a woman during her third labour. The child was delivered by forceps without difficulty and the tumour was extruded just before the head was born. After delivery a lacerated surface was found on the anterior wall of the rectum which was thought to be the point of attachment of the tumour. No communication between the rectum and any other structure was found.

Microscopic examination: This shows a number of definite structural elements—unstriped muscle, cysts lined with columnar epithelium, goblet cells, acinous glandular tissue, cartilage, and fibrous tissue.

Case of Sacro-coccygeal Tumour with Sinus.

By H. GRAEME ANDERSON, M.B.E., F.R.C.S.

AT a meeting of this Sub-section last year, the President, Mr. Miles, brought up for informal discussion the subject of sinus over the sacrum.¹ The general consensus of opinion then was that these sinuses were generally found over the

¹ *Proceedings*, 1921, xiv (Sub-sect. Proct.), pp. 181, 182.

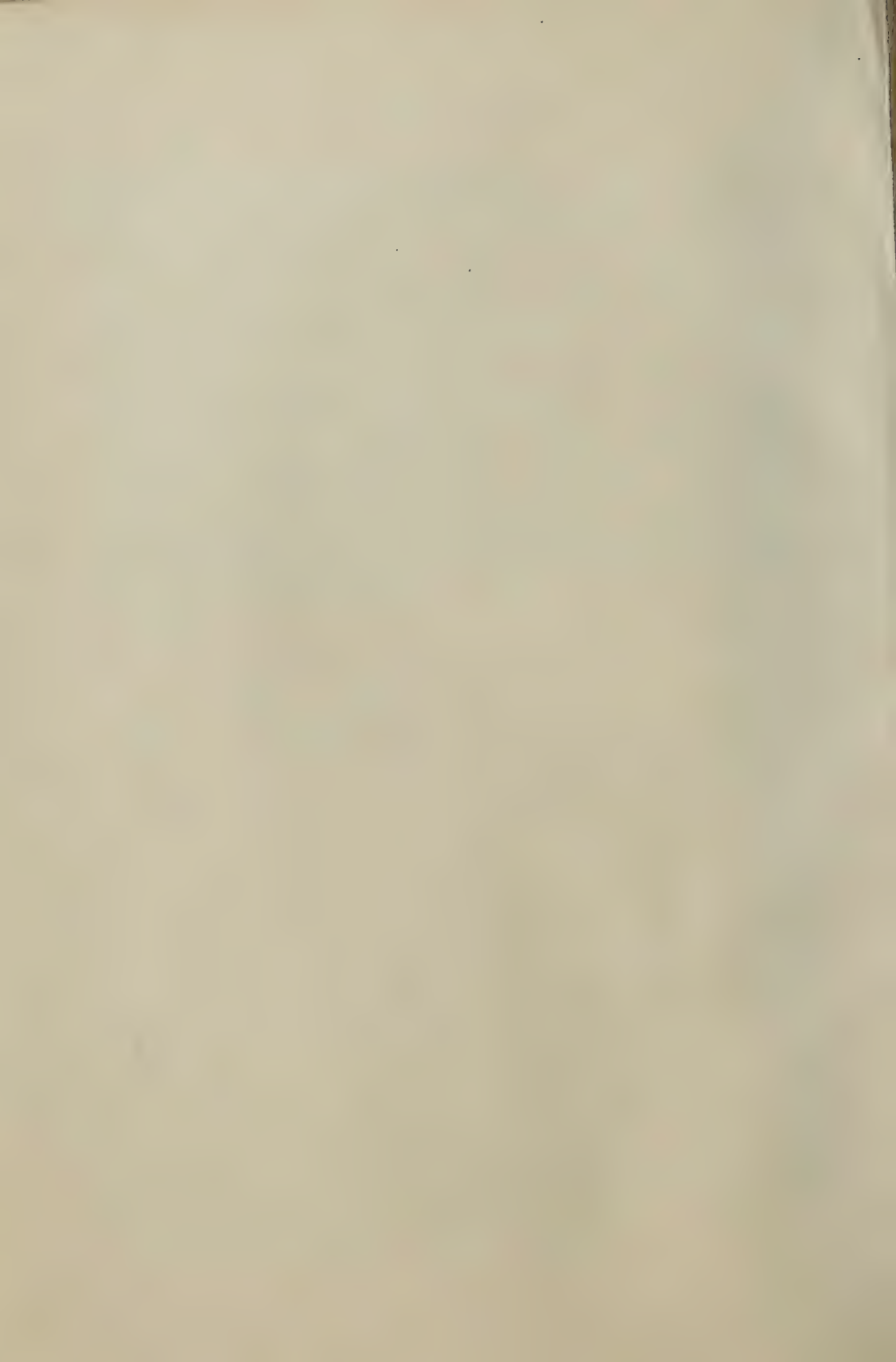
lower part of the sacrum and slightly to the left side of the mid-line; and that they were either connected with dermoid cysts or less commonly with disease of bone, and that they were hardly ever connected with the rectum. At that meeting Sir Charles Gordon-Watson proposed that members should report as many of these cases as possible.

The specimen shown to-day was removed from a man aged 25—a tailor's cutter.

History: The disease began two years ago with an abscess over the lower part of the sacrum. This ruptured and had been discharging up to time of admission.

Examination revealed an oval swelling the size of a split walnut, situated just above the sacro-coccygeal joint and $\frac{1}{2}$ in. to the left of the mid-line. There was a small opening, lined with dark granulation tissue, situated in front of the tip of the coccyx and slightly to the left of the mid-line; a probe was easily passed through this into the centre of the tumour. There was no communication with the rectum, nor was bare bone felt.

The tumour and track were excised as a whole and the wound sutured without drainage. On opening the tumour it was found to be lined with dark granulation tissue with many hairs embedded in it. Unfortunately microscopic sections are not ready for this meeting but the mounted specimen of the tumour is shown for your inspection.



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SECTION OF THERAPEUTICS AND PHARMACOLOGY.

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Section of Therapeutics and Pharmacology.

President—Dr. W. LANGDON BROWN.

PRESIDENT'S ADDRESS:

The Threshold of the Kidney.

By W. LANGDON BROWN, M.D.

[AFTER thanking the Section for the honour conferred upon him by his election and sympathetically referring to the loss the Section had suffered by the death of Dr. Douglas Cow, the President said] : This Section is composed of two elements—the laboratory workers and the clinicians; it embodies a happy and successful symbiosis between “the knowledge-getters and the knowledge dealers.” A recent writer has emphasized “the sterility of medical art when unfertilized by laboratory research and abstract thought. Again and again history teaches this lesson. As scientific research slackened, so medical practice, based on empiricism, rule-of-thumb, and day-by-day observation became stagnant and ineffective.” Speaking as a clinician whose incursions into the laboratory have now become few and far between, I am fully conscious of the inaccuracy that inevitably accompanies work that must be carried on without proper controls, and the difficulty of interpreting its results without bias or prejudice. Yet on this pleasant meeting ground we can receive that friendly and helpful criticism which stimulates renewed effort.

I therefore felt it incumbent on me to choose as the topic of my address a subject which would have, if possible, some interest for both elements in the Section, a subject which would exemplify the reciprocal advantages to be derived from co-operation between the laboratory worker and the clinician. For at least the latter can find problems which it will tax the ingenuity of the former to solve. And I have chosen as my subject the study of the regulating mechanism of the kidney as a guide to treatment, because it fulfils those conditions, and is one in which I have been personally interested.

The function of the kidney is to keep the chemical composition of the blood constant, and the ways in which this is achieved are beginning to be understood. This has also enabled us to appreciate better the significance of certain departures from the normal performance of this function in disease. Given three factors—the urine, the kidney and the blood—if the condition of any two of these is known, the third can be determined. The question is simplified by the fact that the kidney effects very little change in the material it excretes. Indeed, so far as we know, the formation of hippuric acid in the kidney by the synthesis of glycocoll with benzoic acid is the only instance of its altering the chemical constitution. But until recently we have been attempting to solve the problems presented us with two of the above factors unknown. Thus in nephritis the condition of the kidney was deduced from the state of the urine alone, while in glycosuria it was generally assumed that the kidney remained passive and unchanged. Now it is realized that in glycosuria the kidney reacts in a curiously subtle way, whilst in albuminuria much may be learned from a bio-chemical study of the blood.

For many years physiologists were chiefly interested in the death of the blood almost to the exclusion of its other aspects. For its clotting represents its death, even though in dying it serves the vital purpose of the arrest of hæmorrhage. I well remember the interest excited by Ehrlich's discovery of staining methods, which was the foundation of our knowledge of the

morphology of the blood. Yet while this has thrown most valuable light on the diagnosis of blood diseases, it has not given as much information concerning the functions of the blood cells as was anticipated. Blood cultures in septicæmic states next came into vogue and proved of much assistance, though even now they may repeatedly give negative results in cases which are clinically infective endocarditis, and which may ultimately be proved so by examination of the vegetations on the heart valves. But until recently biochemical examinations of the blood remained confined to occasional cryoscopic estimations in nephritis. Now certain tests have been elaborated which enable us to compare the condition of the urine with that of the blood. This has thrown some unexpected light on the way in which the kidney functions, and this in its turn has been a valuable aid in treatment. I should like at the outset to express my deep indebtedness to Dr. Mackenzie Wallis, who made nearly all the biochemical tests in my cases, and without whose cordial co-operation I could have made no headway. To Dr. George Graham I am also indebted for helpful criticism.

May I, in the first place, summarize some of the conclusions reached by Ambard in his interesting work on the physiology and pathology of the kidney, as this will define the issues? I can then go on to discuss certain conclusions I had drawn from my own cases, for the most part while still in ignorance of Ambard's results. Ambard pointed out that the kidney passed material into the urine in three ways: (1) Blood and lymph only appear when there is actual solution of continuity; (2) alcohol, acetone and the like escape by diffusion, without concentration occurring; (3) urea and other normal constituents are secreted in a higher concentration than that in which they are present in the blood. This process of concentration is one of the characteristics of renal activity, and its occurrence is proof of secretory activity. In passing it may be noted that the diffused substances are solvents of lipoids, while the secreted ones are not.

Further, these secreted substances fall into two categories: (1) Those which are purely waste products and useless to cellular life, such as urea, ammonia and uric acid. There is no threshold for their excretion. Most drugs are apparently eliminated in this way. (2) Those which may play a useful part in cellular life, such as sugar, sodium chloride, hæmoglobin and water. The kidney interposes a threshold in the way of elimination of these, so that they only pass into the urine when their level in the blood exceeds this barrier. With the substances for which there is a threshold, it is the height of that threshold which matters. For those without a threshold it is their concentration in the blood that counts.

Ambard compares the excretion of different substances to Dalton's law of partial pressure, according to which each gas exerts its tension irrespective of the other gases in the mixture. There is a limit to the degree of concentration that the kidney can accomplish, but this is not reached under normal conditions. He believes these maximal concentrations of different substances to be isotonic. The more the parenchyma of the kidney is diseased the less will be this power of concentration. He, further, states that a fall of maximal concentration may also occur after surgical operations, in gastro-intestinal troubles, and when excessive amounts of sodium chloride are given. This would account for the development of uræmia after operations, especially on the urinary tract. It might also explain those extraordinary instances in which gastric carcinoma simulates uræmia—where, no doubt, the great reduction in the fluid absorbed seriously diminishes the renal circulation and consequently the urinary secretion. If excess of sodium chloride interferes

with the power of concentration even in health, we can understand why it appears to be injurious in nephritis. Another factor which, according to Ambard, interferes with the power of concentration is exposure to cold. My own strong impression from my study of war nephritis is that cold is not a cause of acute nephritis, but may easily exacerbate chronic nephritis. One can easily understand this, if cold diminishes the power of concentration, since in a kidney already damaged this may lower it below the level compatible with adequate secretion, whereas in the normal kidney there is a large margin available. Purely waste products cannot accumulate in the blood so long as the kidney is healthy, since the rate of their output is not proportional to their concentration but to the square of that concentration. With regard to the substances which are ordinarily useful to cellular life but which may have to be excreted because of their presence in the blood to excess, the same law applies, but only to that part of the excess which is above the level of the threshold. Since, however, the threshold is mobile they can be eliminated in large amount by lowering the threshold. Ambard illustrates the position thus: The organism is a free-trader with regard to waste products, but a protectionist towards substances of value, imposing an export tax in proportion to their importance. The central nervous system only interferes by regulating the threshold, concentration being effected autonomously by the kidney.

The distinction here drawn may prove of practical value, even though we may not be able to accept Ambard's view of the mechanism by which it is effected. It would explain, for instance, the absence from the urine of bile pigment in acholuric jaundice, and of bile salts from it in many other forms. For the pigment being a waste product has no threshold and is, therefore, readily excreted. But as Camus urges, the acholuric type is really a hæmoglobinæmia, resulting from the recognized fragility of the red corpuscles. Now, Ponfick found that one-sixtieth of the total mass of blood had to be hæmolysed before the threshold for hæmoglobin was reached. In acholuric jaundice this is not reached, and the hæmoglobin is slowly deposited in the tissues as hæmatoidin. Bile salts are used over and over again; if there were no threshold for them, there would be some waste every time they are reabsorbed from the bowel. As it is they only appear in the urine with very complete obstruction, particularly if this occurs suddenly.

In considering the more detailed influence of the kidney threshold in disease, I will start with toxæmic kidney, in which I take the threshold to be abruptly lowered by a degenerative but not inflammatory change, the result of some chemical poison. Febrile albuminuria is a mild form of this condition, but severer forms are seen in poisoning by mercurial salts, arsenic, cantharides, uranium, bichromates and in the toxæmia of pregnancy. Mackenzie Wallis's recent studies of this condition have helped us to distinguish it from nephritis. All non-nephritic proteinurias are characterized by the presence of an amount of globulin in the urine which is not found in nephritis. This is true alike of orthostatic "albuminuria," toxæmic kidney and leaky kidney. In the first two the albumin to globulin ratio is 2 : 1 instead of 6 : 1 as in nephritis. This may be due to a lowering of threshold for globulin, for if it were due simply to transudation of blood proteins through a damaged structure it could hardly be an intermitent process as it is in orthostatic cases. The clearest evidence as to lowering of the threshold in toxæmic kidney is obtained, however, from the output of diastase and sugar. It is recognized that the output of diastase is lowered in many forms of nephritis and raised when there is an obstruction to its outflow through the pancreatic duct. In the former, there is, of course, damaged permeability, in the latter there is excess of diastase in the

blood. Neither of these conditions tell us anything about the threshold. But in toxæmic kidney there is a marked increase in the diastase output. Since there is no other evidence of pancreatic damage, this increased output must be due to the threshold for diastase being lower than normal, since it is not then present in excess in the blood. Certainly one would expect it to have a threshold, being a substance useful to the body. The presence of sugar in normal urine was first described by Pavy. By a new method, involving the precipitation of the other constituents first, Mackenzie Wallis finds there is normally an excretion of 0.089 per cent. of sugar, i.e., nearly three times the amount found by Pavy. In nephritis this tends to diminish, and the blood sugar to rise, as if a new barrier were interposed, but this does not occur in toxæmic kidney where the blood sugar is either normal or falls, while there may be frank glycosuria. For substances either with or without a threshold the toxæmic kidney is unduly permeable; thus, there is never an increase of urea in the blood and a decrease in the urine as there is in many forms of nephritis. The practical lesson for this is that here we have an opportunity of flushing out the kidney, and thus helping the elimination of the toxin, such as does not occur in acute nephritis, when the threshold for water is raised. And the prognosis is different, for if the patient with a toxæmic kidney can be kept alive, complete recovery may be expected. The condition does not lead to chronic nephritis, and these tests are capable of showing that supposed instances to the contrary in pregnancy were really cases of nephritis. Recently I had a case of chronic nephritis with retinal changes which I was told originated from a toxæmia of pregnancy. Fortunately these tests had been carried out during pregnancy and they showed conclusive evidence of nephritis then, not of toxæmic kidney.

Ambard has attempted to draw a very sharp distinction between dropsical nephritis and azotæmic nephritis. He maintains that the excretion of urea remains unaffected in dropsical nephritis, and that only the substances with a threshold are affected, particularly sodium chloride. According to him when the chloræmia rises, the threshold rises, and vice versa, but the parallelism is not perfect, the threshold rising and falling more than the chloræmia. Thus restriction in salt ingestion would produce a fall of chloræmia in two ways, while the threshold by rising with a salt diet, not only does not do its duty but fails when most needed. It seems to me that it requires special pleading to uphold this thesis in its entirety. Thus Ambard draws a contrast between the elimination of potassium iodide, a substance without a threshold, and of sodium chloride, which has one. Now I have found in some cases of dropsical nephritis iodide may produce a prompt return of œdema, which certainly ought not to have occurred if this be true. Still I can quote a case in support of this view. A lad with chronic nephritis and dropsy showed no azotæmia, and a urea concentration of 1.7 per cent. Yet his threshold for useful substances had risen, since the diastase output was only 3.3 and his blood sugar had gone up to 0.132 per cent.

Again, though not prepared to agree with all Epstein's conclusions, I am convinced he is right in saying that a salt-free diet is not always successful in abolishing œdema. I would agree with him that a diet rich in protein is sometimes more successful. This he attributes to the reduction of the blood-proteins by proteinuria, with consequent fall of osmotic pressure in the plasma. I am inclined, however, to think that the disappearance of œdema may be due to the diuretic effect of the urea to which this diet gives rise, as much as to its increasing the blood proteins. For there is no evidence that one can directly increase blood proteins by food proteins, since the latter are all broken down

to amino-acids before absorption. And, again, the œdema of acute nephritis can hardly be due to reduction of blood protein, since it comes on at a time when the total volume of the urine is too small to have affected this seriously. At any rate I have tried the following plan with an improvement, sometimes only temporary, it must be admitted, in the œdema. The permeability of the kidney is first determined by MacLean's urea concentration test. If this shows a result above 1.5 per cent. I give 45 to 60 gr. of urea three times a day, while the patient is on a diet which is not excessive in proteins but is poor in fats. For I think Epstein is right in saying that lipoids may increase œdema. MacLean has independently also advocated this line of treatment.

I can of course conceive that the apparent retention of urea in dropsical nephritis may simply be due to its passing into the increased body fluids so that its concentration in the blood falls, which would affect its output. Then the administration of the urea, by raising the amount in the blood, would materially assist the elimination of fluid. And we might even explain the temporary character of the improvement by saying that as the sodium chloride was still retained, a point would still be reached when its osmotic pressure would check further diuresis. Nevertheless, I do not think the production of œdema in nephritis is quite so simple as Widal and Ambard would have it. That the threshold of the kidney may then be disturbed we must be prepared, however, to admit. Thus, in dropsical nephritis a dose of phlorhizin may not cause glycosuria, which would normally do so, while a larger dose is still effective, i.e., there is a difficulty in lowering the threshold for sugar which does not normally exist. Mackenzie Wallis's observation that the sugar tends to rise in the blood and to fall in the urine points in the same direction. In azotœmic nephritis, on the other hand, Ambard holds that the threshold behaves like that of a normal individual, retaining its mobility. The kidney's power of excretion of all substances is disturbed, but in the case of those with a threshold, it can be aided by this being lowered. There being no threshold for urea, it cannot be lowered, and the diminished power of concentration has its full effect. Hence urea retention. A recent case of mine is difficult to explain on this hypothesis. The patient was a man, aged 52, with a blood-pressure of 220 mm., and with slight dropsy which soon cleared up. That his threshold had risen was shown by the fact that his blood sugar had gone up to 0.157 per cent., and the "normal" urinary sugar had fallen to 0.055 per cent.; moreover, his diastase output was only 5. As for purely waste products—on a low protein diet his blood urea was 92 mgm. per cent. (the normal being not more than 50) so that there was retention, yet the urinary urea after MacLean's concentration test was 3 per cent. This suggests a good power of urea concentration by the kidney although there was retention in the blood, a paradox hard to explain. The difficulty to which Ambard is put in order to maintain his distinction that in dropsical nephritis it is the mobility of the threshold and in azotœmic nephritis the power of concentration that are affected is shown by the following contentions he makes: (1) Where œdema and azotœmia co-exist there is mixed nephritis. (2) An azotœmic who increases in weight on a salt diet is suffering from mixed nephritis. Surely this is saying that most cases of nephritis are mixed, which I am prepared to believe. (3) An increase of weight in a nephritic on a salt-free diet is due to a true increase of tissue consequent on improved appetite and assimilation. If this is so, how can we explain the cases which, though increasing in weight on a salt-free diet, lose it rapidly on Epstein's treatment?

In his very full report on war nephritis MacLean also distinguishes between an azotœmic and hydrœmic, or dropsical, type. In the acute stage he found

manifestations of both types, as if the kidney were suffering from a diffuse inflammation, but if this did not resolve completely, the convalescent stage which followed tended to one or the other type, although sometimes the types were mixed. The distinctions he draws between the two types are much the same as Ambard's, but he also discusses the diastase output on which Ambard does not touch. He finds the diastase reaction low in the azotæmic type and normal, or nearly so, in the hydræmic. That is to say, he finds it behaves like a substance without a threshold. I must not take up your time with a mass of details, but in a good many cases I investigated with Mackenzie Wallis we did not find this distinction concerning diastase. Thus a severe and ultimately fatal case of the parenchymatous or hydræmic type showed good urea concentration but a low diastase output. Conversely, a young officer of the azotæmic type who showed severe manifestations such as uræmic convulsions, aphasia, a blood-pressure of 180, and much retinitis, but no œdema, and yet who made a good and apparently complete recovery, had a low output of urea, chlorides and diastase alike. My conclusion is that it is rather difficult to maintain a clear-cut distinction between a parenchymatous hydræmic type in which the output of substance with a threshold is principally involved, and an interstitial azotæmic type in which the power of maximal concentration is the function that is impaired. In support of this I may mention Canti's observations on urea retention in cases with true uræmia. This he found decidedly most frequently in hydræmic cases, which is not in accordance with the distinction drawn between the types. Though we may recognize the interstitial and parenchymatous types clinically, the bio-chemical tests point to an interference with renal function, which is generally diffuse in its effects, though the stress may fall sometimes on one and sometimes on the other of these two great functions of concentration and of regulation of the threshold.

Perhaps the threshold of the kidney has been most fully studied in relation to sugar. The earliest systematic studies with which I am acquainted are those of Graham, who distinguished between cases of glycosuria with what he called a high and low "leak point." I would suggest he might substitute the term "threshold," which has a wide and generally recognized significance in biochemistry.

The normal threshold of the kidney for sugar lies between 0.15 and 0.17 per cent. The giving of carbohydrate food is followed by a rise in the blood pressure from its fasting figure of 0.1 per cent. within about twenty minutes, which reaches its maximum in half to one hour and then begins to fall. Should the blood sugar rise above about 0.17 per cent., this is normally followed by some glycosuria, which will show concentration by the kidney in so far as the glycæmia exceeds the threshold. Thus Ambard quotes a case where the threshold had risen to 0.25 per cent. and the glycæmia was 0.26 per cent. The excess of glycæmia was, therefore, only 0.01 per cent. above the threshold, but the glycosuria being 0.2 per cent. there had been a real though not an apparent concentration.

To investigate the threshold, therefore, it is necessary to give a dose of sugar, and to see how the body disposes of it. The plan which has been followed in my cases is as follows: The patient is temporarily freed from glycosuria by a fast or vegetable and egg diet. On the morning of the examination a light breakfast of some tea and two eggs is allowed. About 11 a.m. the first drop of blood is taken, and then a drink of water containing 25 or 50 grm. of dextrose is given. Further drops of blood are examined a quarter, half, one, one and half, and perhaps two hours after. The results are

plotted out on a curve. MacLean, who recently published a paper on this method, has shown that 5 gm. of dextrose will produce an appreciable rise, 10 gm. rather more, 25 gm. will cause a distinctly greater rise, but 50 or 100 gm. will produce hardly any more effect than 25 gm. All sugars produce a rise except lævulose, though lactose causes a slower one. From this we may deduce that lactose may sometimes be allowed in the less severe forms of glycosuria. Starches also produce a rise in blood sugar. The fact that normally the blood sugar begins to fall after a half to one hour, although absorption of sugar is still continuing, he points out must be due to the sudden intervention of a storage mechanism which extracts the sugar from the blood more quickly than it enters. That the mechanism is highly efficient in the healthy person is shown by the difficulty or impossibility of forcing the blood sugar above the threshold. We assume that this rapid storage occurs as glycogen, chiefly in the liver, but also in the muscles.

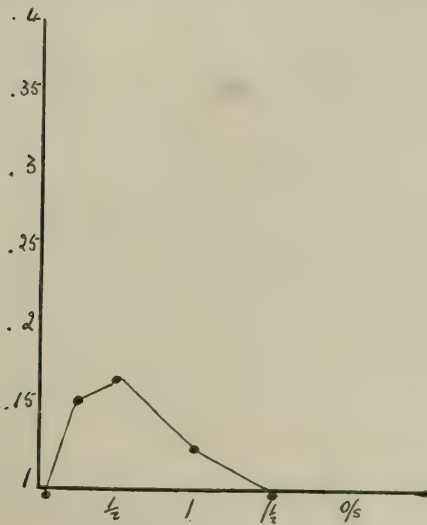


FIG. 1.—Normal.

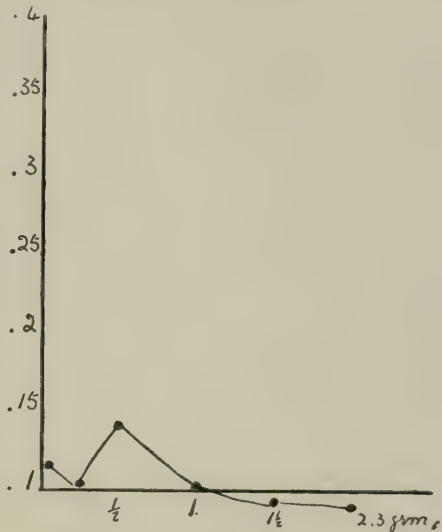


FIG. 2.—Renal glycosuria.

Here I may digress for a moment to say that those observations do not support von Noorden's statement that the liver, although the largest gland dealing with carbohydrate metabolism, appears to have nothing to do with diabetes. The French school have always held a contrary view, though they have not adduced much evidence in its support. But this method clearly indicates, as I hope to show, that one considerable factor in some cases of glycosuria is a delay in the storage of the sugar after it is absorbed. Before considering this point I would like to show a curve illustrating the normal sequence of events (fig. 1). Next, I show a case with an abnormally low threshold, the so-called renal type (fig. 2). This appears to be a congenital and often familial peculiarity and is not in itself evidence of disease. Its usual feature is a low grade of glycosuria almost independent of diet, and no treatment is called for. It is interesting to note that the brother of the patient in this case subsequently showed a similar curve, glycosuria having been detected on examination for life insurance; while the sister of another patient has moderately severe diabetes. This latter case cannot, however, be

of the ordinary renal type since he passed no sugar during this observation. Graham's type of diabetes innocens appears to be renal glycosuria sometimes complicated by a lowered tolerance for sugar, so that an increase in the carbohydrate of the diet is followed by a rise, though not a proportionate one, in the glycosuria. I am inclined to think that recorded cases of glycosuria with a low blood sugar, which subsequently become true diabetes, are not of this type at all; probably the confusion has arisen by depending on a single estimation. I shall refer later to curves which demonstrate how misleading a single examination would have been.

The next group of cases had intermittent or transitory glycosuria (*see fig. 3*). They show a normal threshold; that is to say, glycosuria occurred as soon as this was exceeded. There is a sharp peak on the curve at this point, but it is followed by a normally quick fall. There would appear, therefore, to be a delay in the storage mechanism coming into action, but as soon as it did act,

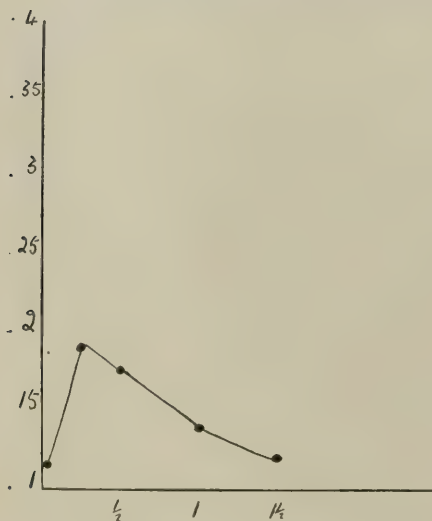


FIG. 3.—Intermittent or transitory glycosuria.

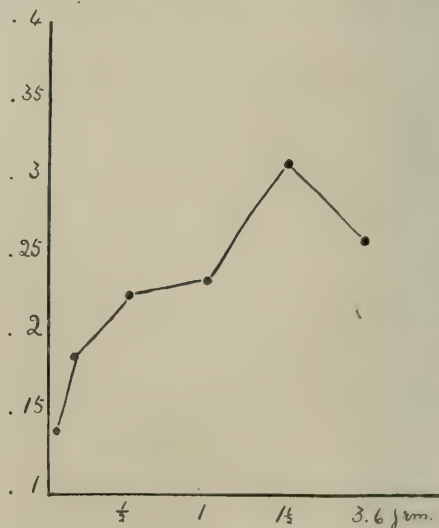


FIG. 4.—Obesity with consecutive glycosuria.

assimilation proceeded normally. In these cases sugar was not allowed as such in the diet, but starch was not restricted. Some of the starch was given cooked in fat, as suggested by Mackenzie Wallis, in order to delay its digestion and subsequent absorption into the blood. The more slowly the blood sugar rises the more likely is assimilation to be effected in this type.

It may be asked, what objection is there in this type to allow the overflow to occur? Well, for one thing the patient feels better when this is not happening. One of these cases was that of a child who had nocturnal enuresis when the threshold was exceeded. In another, caution was certainly indicated since one of the patient's uncles died of diabetic gangrene and another uncle has glycosuria which he can keep under control with care, and with benefit to his health. But I think the strongest argument in favour of keeping within the limits of carbohydrate tolerance is derived from the study of the threshold in persistent glycosuria.

For how does the body react to lowered sugar tolerance? The simplest and quickest method of dealing with absorbed dextrose is apparently the

storage as glycogen, and this seems to be the first to suffer. To prevent escape of a valuable foodstuff the body can either store more of it as fat or it can raise the threshold of the kidney. Von Noorden maintained that some cases of obesity are latent glycosurics, i.e., increased fat storage comes into action to compensate for diminished power of glycogen storage. But fat once stored is not so easy to metabolize as glycogen. The latter process has been compared to putting money into the bank, but one might compare storage as fat to putting money into war loan. It is there, but it is difficult to realize. There is a limit to fat storage, and another compensatory measure is to raise the threshold. In illustration I give a curve (fig. 4) from a man, aged about 50, who has been abnormally stout all his life. It shows a large and long-continued rise in the blood sugar curve after a start from a normal level. I cannot say whether in such cases the increased storage as fat precedes the rise of threshold or not. At the time von Noorden made his observations

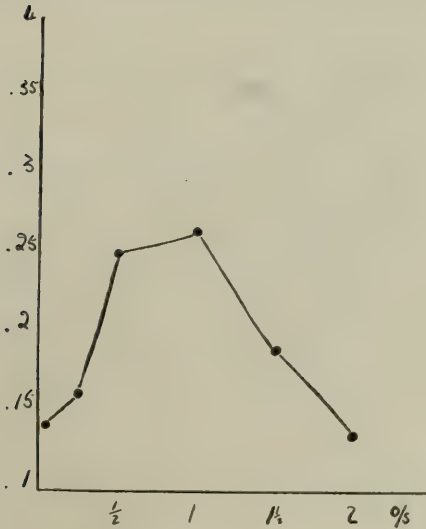


FIG. 5.—Hyperthyroidism.

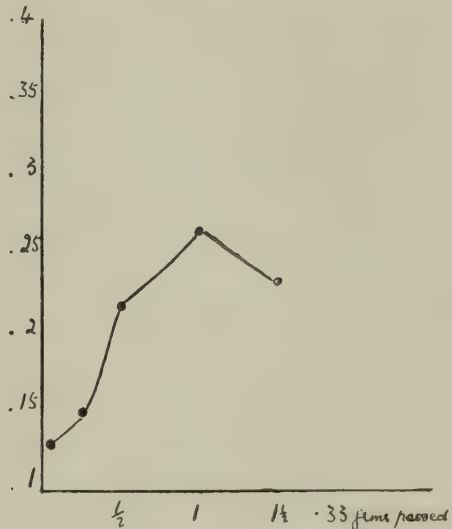


FIG. 6.—Diabetes of moderate grade.

nothing was known of kidney thresholds. But in my next group one can positively say that the rise of threshold occurred without increased storage of fat and for the most part even without glycosuria. It is generally accepted that in hyperthyroidism there is diminished sugar tolerance, and that there may be actual glycosuria. What is perhaps not so clearly recognized is that this diminished tolerance must be due to delayed storage of carbohydrate, so that the blood sugar curve shows an excessive rise with a delayed fall. Yet this may excite no glycosuria, as in two of my curves of this series, although the glycaemia reached a level which would have produced an overflow in the normal subject. In the second case no sugar was passed after administration of dextrose, though there was subsequently a temporary glycosuria on ordinary diet, following some mental agitation (*see* fig. 5). A third case, curiously enough, did not show any glycosuria until her exophthalmic goitre was subsiding, and then it was readily amenable. None of these were putting on weight, and the second was losing it rapidly, ultimately falling to less than half his initial weight. It would appear, therefore, that raising the threshold can

occur without any increased storage of fat, and indeed I would suggest that such increased storage is due to the raised threshold, which by holding more sugar in the blood enables the more sluggish fat anabolism to occur, if the condition is not associated, as it is in hyperthyroidism, with general increase of katabolism.

The next group of cases of diabetes of intermediate severity offers some difficulty in interpretation. They all had a normal blood sugar before the dextrose was given. Two of these were treated by Graham's method with benefit. The symptoms in the second of them started two years previously when the boy was aged 15, with all the appearance of severe diabetes, yet under treatment he was able to tolerate 64 grm. of carbohydrate on a daily diet of about 2,200 calories (*see fig. 6*). A third is a case of unusual interest. She was a girl, aged 17, who had started with symptoms five years previously. She had done badly on Allen's method, but kept her general health on a less

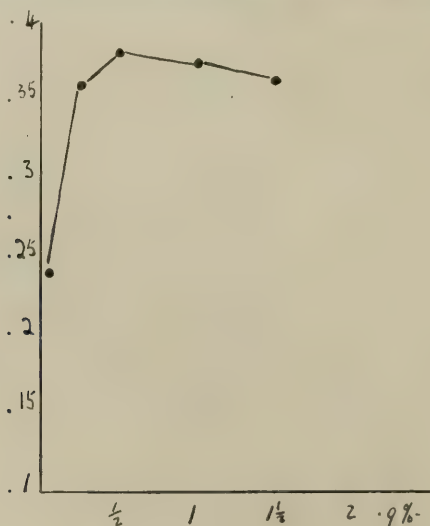


FIG. 7.—Severe diabetes.

restricted regime, even though passing some sugar. Her blood sugar first kept below the normal after giving dextrose, which illustrates my point as to the error which a single determination may cause, since subsequently an excessive and prolonged rise occurred. Another most interesting feature is that the "normal" urinary sugar was entirely absent at the beginning of the observation, showing conclusively that the threshold was already raised.

Contrast with these my last group of clinically severe diabetes. There is an extraordinarily sharp rise which is long continued at a high level; in all except one case of recent origin the curve starts from a point above the normal level; even during fasting there is hyperglycemia. Sugar is always passed after 25 to 50 grm. are given. How are we to account for the initial fall in blood sugar in the early case of this series? It seems to me that this must be due to the storage mechanism coming rapidly into action, but becoming speedily exhausted—a condition of irritable weakness. This case, though clinically severe, shows a blood sugar curve closely resembling the last example of my intermediate type—it responded well to treatment and will probably

become milder if that treatment is maintained. It affords a link between the two groups.

The curve I give in illustration of this group (fig. 7) I would particularly ask you to observe. The threshold is greatly raised but the amount of glycosuria is slight and did not vary very much with the diet. Thus the patient did not become completely free from sugar on a forty-eight hours' fast but continued to pass 0·2 per cent. then and with increasing diet; the glycosuria only rose to 0·5 per cent. when the diet was materially added to. It would appear that the threshold had been pushed up so high that the blood could hold enough sugar for the overflow to be so slight and so little dependent on the diet that she simulated the renal type. I have under my care at present a similar case in a girl aged 10½ who had been known to have glycosuria for three years, and who had had symptoms for a year before coming under treatment. She has a blood sugar of 0·23 per cent. and yet only passes 0·5 per cent. of sugar with increasing diet.

It is by no means uncommon for a glycaemia of 0·25 per cent. to be found in a treated diabetic who is kept free from glycosuria—an amount of sugar in the blood which would inevitably cause glycosuria in a normal individual. I have already said that this rise of threshold is presumably compensatory to avoid loss of a valuable foodstuff, but two further questions arise—What is the mechanism by which the rise is produced, and has this rise any disadvantages?

The mechanism which Ambaré describes for chlorides might apply to sugar, i.e., when glycaemia rises the threshold rises, and vice versa, the threshold rising and falling more than glycaemia. Thus restriction of carbohydrate diet would produce a fall of glycaemia in two ways. But I am not sure that this is the whole matter. Hyperglycaemia tends to increase even, and perhaps especially, in treated cases. It seems to be more marked in the cases which have had an abundant non-carbohydrate diet. It certainly can be reduced by fasting or by a diet of low calorie value, such as the egg and vegetable diet. Thus, I saw a fall from 0·23 per cent. to 0·15 per cent. as the result of a single egg and vegetable day. The important point, however, is that a rise appears to follow rather than to precede glycosuria.

Hyperglycaemia, like most pathological attempts, may overshoot its mark and may bring other troubles in its train. All the processes in the body have an optimum point. Thus, respiration works best with an alveolar CO₂ of 5 per cent., gastric digestion with an HCl of 0·2 per cent., the circulation with 5 million red corpuscles and so on. Wide departures from these figures can be tolerated, but the body works with much more friction and various inconveniences arise. It has been claimed that cataract, neuritis and septic infections are consequent on hyperglycaemia, though each of these statements have been contested.

Certain it is that polyuria and consequent thirst only occur when the glycaemia considerably exceeds the threshold, so that the kidney can concentrate and eliminate a good deal of the sugar. Thus, neither of my cases with marked hyperglycaemia but with a small and almost constant output of sugar in the urine have polyuria or thirst. It is important to determine whether hyperglycaemia is the enemy. Like hyperpiesis or hyperpyrexia, hyperglycaemia, no doubt, sometimes calls for direct treatment, especially if it is damaging the eyesight or causing intractable neuritis or septic complications. Thus, I have a man under my care with a blood sugar of 0·22 per cent. and glycosuria. The solitary symptom is early cataract. Now, we can lower and even abolish the threshold for sugar by phlorhizin. Would it be justifiable and helpful to use this drug in such cases? So far I have not

ventured to do so, and have contented myself with days of fasting or of egg and vegetable diet. But it is a point on which I should specially like opinions.

Yet again, does persistent raising of the threshold ultimately damage the kidneys? That albuminuria and even true nephritis may develop chronic glycosuria has been known since the days of Pavy. In this connexion Dr. Mackenzie Wallis tells me that in India chronic glycosurics with hyperglycæmia often die of uræmia rather than from coma. I have at present under observation a lady aged about 48, who was investigated some eighteen months ago at Duff House. She then had well marked glycosuria and slight albuminuria, with moderate hyperglycæmia. Now she has no glycosuria on a moderately restricted diet, but the urine is loaded with albumin (not globulin), and contains casts, while the blood-pressure is rising rapidly. Her symptoms are those of high pressure with secondary cardiac failure. The raised threshold seems to be by no means free from risk or disadvantage.

Finally, I suggest that one of the functions of the pituitary is to regulate the threshold for water and that the essential feature of diabetes insipidus is a lowering of this threshold, probably due to pituitary deficiency. I would exclude from this category the type of polyuria described by Erich Mayer, in which there is a failure of renal permeability to many solids, especially salt, and in which theocin-sodium acetate, by increasing permeability, positively acts as an anti-diuretic. Such cases are more allied to nephritis and may end, as Saundby showed, in uræmia. Many cases of clinical diabetes insipidus show either gummatous meningitis of the base of the brain or some lesion of the pituitary. In others in which no such organic changes have been demonstrated a sympathetic nervous irritation of the pituitary has been postulated, based on evidence of the effect of experimental stimulation of these nerves. That the polyuria of diabetes insipidus can usually be kept in check by injections of pituitrin has been repeatedly observed, and the marked discrepancy between the experimental production of polyuria through the pituitary and the clinical evidence is probably due to the action of the anæsthetic in the former. It seems to me that the secretion of the posterior lobe regulates the output of water in some way, and I suggest that it does so by affecting the threshold. This would explain its apparently discrepant behaviour in different circumstances. Priestley has shown that the injection of pituitrin may greatly diminish the output of water without preventing the excretion of a dye simultaneously injected. This proves that the capacity of the kidney for dealing with one substance is independent of its behaviour with others. The pituitrin apparently raises the threshold for water, but the dye having no threshold its excretion is unaffected. A further tempting speculation occurs to me: in the valuable studies on diuresis made by our late secretary, Dr. Cow, he pointed out that water given by the mouth and absorbed by the gastrointestinal tract is incomparably more active as a diuretic than the same quantity of water injected subcutaneously or intravenously, and that some substance, possibly a ferment, is taken up by water during its absorption, which has either directly or indirectly a diuretic action. Is it possible that this substance affects the secretion of the pituitary gland?

But I must refrain from further speculation, and I am conscious that even in the less speculative parts of my address I have oftener classified rather than explained phenomena. My main thesis is that in the mobile threshold for substances of value to the body we have an important regulating mechanism in the kidney, and that its study is already yielding a guide to treatment and is rich in promise for the future determination of renal function in disease.

Section of Therapeutics and Pharmacology.

President—Dr. W. LANGDON BROWN.

Parathyroid Therapy in Calcium Deficiency.

By H. W. C. VINES, M.B.

(Beit Memorial Research Fellow.)

IN the year 1880, Sandström [1] discovered the two external parathyroid glands, but little importance was attached to their discovery till Gley, in 1881 [3] rediscovered them, and four years later Köhn [2] announced the discovery of the two internal glands. At first the functional differences of the thyroid and parathyroids were not distinguished, and even their structural dissimilarity was not recognized. In 1897, Gley [3] published an account of these glands, in which he claimed a very intimate connexion between them, considering that parathyroid tissue was immature thyroid tissue and that tetany could be prevented by the injection of thyroid extract. This work, however, has not found support among other investigators, and Moussu [4] was one of the first to insist on the functional and structural independence of the parathyroids. Of recent years this differentiation has become gradually more defined, and there is now little room for doubt that the parathyroids and thyroid are separate entities, both structurally and functionally.

A short consideration of the known functions of the parathyroid may not be out of place. By far the larger bulk of the work on these glands has been in the experimental production of tetany in animals by ablation of the parathyroids, and much clinical work has been carried out on cases of tetany in the human subject. The outcome of this work is found in the results obtained by Noël Paton [5] and his co-workers. They established the fact that the convulsions of tetany are due to the presence of methylguanidine in the blood, a result foreshadowed by the work of Koch [6] in 1913, and that, further, the parathyroids were normally capable of rendering this substance innocuous. MacCallum and Voegtlin [7] had previously shown that parathyroid would control the convulsions of tetany, and Edmunds [8] was able to demonstrate that large doses of calcium salts would prevent tetany following parathyroidectomy. It may still be considered a moot point as to whether the calcium on the one hand acts by depressing the hyperexcitability of the nerve cells, and the parathyroid on the other by its actual chemical effect on the toxin, or whether the latter action is an additional function of the calcium salts.

The experimental control of tetany by calcium salts and their connexion with the parathyroid glands led to the application of similar treatment in human subjects and in other conditions in which convulsive seizures were a feature of the disease. Vassale [9] used parathyroid therapy with some success in eclampsia and also in epilepsy. Naame [10] considers that epilepsy is due to a toxic state, and suggests that the attacks are caused by an intermittent failure in the detoxicating function of the parathyroids. In paralysis agitans [11], chorea and uræmia, successful results have been claimed for the

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use of parathyroid, with or without the addition of calcium salts. In general it may be said that since so much of the experimental work has been done on tetany, the clinical application of the results obtained has been chiefly to cases of the nervous convulsive type. In view of subsequent remarks, some of the conclusions arising from the study of tetany may be shortly considered.

Attention has already been drawn to Paton's work, and to the evidence obtained of neutralization of methylguanidine. Launoy [12] has formulated the theory in regard to the origin of infantile tetany, that very probably some previously existing acute or chronic toxic state has permanently damaged the parathyroids, and that any subsequent toxic condition will precipitate a typical tetanic seizure. MacCallum [7] suggests that tetany is an acidosis—and this view is strongly upheld by Togawa [13], though other investigators have taken the view that the condition present is that of alkalosis. MacCallum supposes that in the presence of a parathyroid insufficiency certain substances are formed in the tissues, which may be capable of combining with the calcium salts, and in combining may favour calcium excretion. Finally, Massaglia [14] ascribes to the parathyroids the function of rendering harmless simple waste products, or complex katabolites or toxic substances derived from the large intestine, or due to pregnancy and the puerperium or muscular fatigue. There is, therefore, some evidence that one function of the parathyroid glands is that of detoxication, but there is very little evidence produced to indicate the rôle of the calcium salts in the detoxicating process.

While bearing in mind the theories founded on the study of convulsive and nervous states in which the parathyroid glands are of apparent importance, we may turn our attention to other conditions which seem to be definitely without nervous connexion. The most extreme case of this group is perhaps the condition of osteomalacia. Freund and Lockwood [15] have obtained some evidence that the parathyroids may be of importance in this state. After a week's control period, they treated a patient with well marked osteomalacia for successive periods on different endocrine substances, and observed the changes in the balance between calcium intake and output under the different treatments. Using thyroid gland first and then pituitrin, the calcium balance in each period was negative, but after using parathyroid the balance became positive. At the end of the parathyroid period the ovaries were removed, and then the calcium loss was found to be very much increased, although in a proportion of cases castration has been found beneficial. Assuming that osteomalacia represents the extreme in this group of cases, other conditions should be found in which the deficiency of calcium is less pronounced, and it is to these cases that I will now pass.

In a paper in the *Journal of Physiology* [16] evidence was brought forward to show that the calcium of unclotted blood is normally present in two forms, ionic and combined. The combined form was found to be essentially coagulative, and after clotting had occurred the calcium of the serum was all in the ionic form; in the normal individual this amount of calcium was found to be about 10.5 mg. per cent. It was also found that in unclotted blood the combined calcium could be converted to the ionic form by treating the blood with dilute alkali. In the morbid states to be discussed, however, some of the calcium of the serum was found to be in a combined form after coagulation had occurred, and in some cases there was not only a deficiency of the ionic calcium, but also of the total calcium of the blood.

Rather more than 100 cases have so far been dealt with, and a variety of conditions investigated; the bulk of the cases fall under a general heading of

chronic toxæmia, and it is with the ulcerative cases that I wish to deal at present. This group includes ten cases of varicose ulceration, twelve cases of gastric or duodenal ulcer, two cases of erosion of the cervix uteri and six cases of carcinoma. With the exception of the last, all these cases belong to the class of simple ulcerations, and one may assume that in all of them there is a preceding chronic toxic state, whether it be caused by continued venous stagnation or by toxic absorption from the intestine or from some septic focus. As the varicose cases have been studied in greater detail [17] than the others, they may be discussed as typical ulcerative cases.

After the fact of the relative deficiency in ionic calcium had been established, an attempt was made to correct it by the intramuscular injection of calcium chloride: 1 or 2 gr. were given once a week [18]. This treatment yielded beneficial results up to a certain point and some progress was made in healing, but after a period of about thirty days an apparent tolerance to the injected calcium became established, and the amount of the dose had to be increased in order to obtain the same effect. This increase was followed by another period of improvement until once again the condition became stationary, and in only one case was cure of the ulcer obtained by this method of treatment. This would suggest that the administration of calcium salts is a symptomatic rather than a radical form of treatment.

The injections were then combined with the oral administration of thyroid substance, but no better results were obtained, this indicating that the thyroid has no very marked action on the calcium metabolism of these cases. The oral administration of calcium salts was also ineffective in changing the state of the local condition.

Parathyroid substance was then given in doses of $\frac{1}{10}$ gr. of the dried gland daily, and a marked improvement was soon apparent. The ulcerated area commenced to heal rapidly, and the calcium of the serum returned to normal. Usually the parathyroid was given daily until a maximal effect was obtained (seven to fourteen days), as shown by the reappearance of the normal amount of ionic calcium in the serum; no symptoms of overdosage appeared in any of the patients. After the end of about a fortnight, the dose was reduced to $\frac{1}{10}$ gr. twice a week until healing of the ulcer was complete, a period which of course varied with the size of the lesion. After-treatment is important, for if we are to suppose that the ulcerative condition is due to a parathyroid insufficiency dependent on some damage to the glands, it will not be expected that the ulcers, though once healed, should remain so indefinitely after the withdrawal of the external source of parathyroid secretion. The patient should therefore be carefully watched and any sign of the return of the ulcer, such as eczema or local irritation, should be met at once by a resumption of the parathyroid treatment. The best results are obtained with the patient in bed and at rest; under these conditions early cases of ulceration or of varicose eczema respond very readily, and no special treatment of the ulcers is required.

In a series of gastric ulcers, parathyroid alone was used from the beginning of treatment; here again considerable benefit was derived. In one case the patient gave a history extending over seventeen years with repeated hæmatemesis; she was sallow in appearance, and her blood count and hæmoglobin percentage were of the chlorotic type. She was given parathyroid $\frac{1}{10}$ gr. every other day for a month, and then less frequently. Three months later her colour had improved, and there had been no hæmatemesis since the commencement of treatment. In all cases treated in this way remission of symptoms

has been observed, and in the early cases of the series apparent cure of the condition has been obtained. The disappearance of symptoms is again consequent on the return of the ionized calcium of the serum to normal.

From clinical data one may pass to the consideration of the possible cause underlying these ulcerative states. The common factor in both the conditions mentioned are a deficiency of ionic calcium, and presumably a chronic toxic state. In varicose ulceration one may suppose that the latter arises from a continued, though partial, failure of the circulation in the varicose areas, which causes an overloading of the blood with products of katabolism. In gastric ulcer, various chronic toxic states have been assigned as the causative factor, either locally in the gastric mucous membrane or at a distance. But the beneficial effect of parathyroid therapy on these two factors, toxæmia and calcium deficiency, and on the ulcerative condition which is symptomatic of them, would lead one to suppose that in these cases the parathyroid glands are at least partially deficient in function.

There is then some indirect confirmation of Launoy's hypothesis, which has already been referred to, and the results of examination of the blood calcium bear out MacCallum's theory that toxic substances may arise under conditions of parathyroid insufficiency, which are able to form combinations with the calcium of the blood and tissues. It is not possible to say whether the toxic substance is the same as that found by Paton following parathyroidectomy, but in no case has a patient with a complaint of the type described shown any symptom of tetanic convulsions. The reason may be first that the toxic agent is different in the two cases, and secondly that the calcium values do not fall sufficiently low. Howland and Marriott [19], in a series of eighteen cases of tetany in children, show an average value for the total blood calcium of 5.6 mg. per cent.; they state also that convulsions do not usually occur until the calcium has fallen to 7 mg. per cent., and that the same figure holds good in the case of parathyroidectomized dogs. In the ulcerative cases of the present series, the total calcium was never found to be below 7.6 mg. per cent. in any individual case, and the average for the cases before treatment commenced was 9.2 mg. per cent.

In regard to the focus of development of the ulceration in these cases, with the exception of the gastric ulcers, the reason for the locality is apparent. If varicose ulcers are a trophic lesion depending on nutritional defects, they would naturally occur at the site where tissue resistance and nutrition are least effective; this will be at a point within the varicose area, determined probably by some slight local trauma. The cause of the appearance of gastric ulcers in the gastric mucous membrane does not seem so obvious. Various views are held as to their pathogenesis, founded for the most part on the theory of thrombosis or embolism, and subsequent focal necrosis; the necrosed tissue is digested by the gastric secretion, and the acid medium prevents subsequent healing. This view is formed from the results of experimental thrombosis in animals, but in man the origin of embolus or thrombus must presuppose in the majority of cases a septic focus, either localized to, or at a distance from, the stomach, and it is generally held that gastric ulcers are associated with some toxic condition, as for instance, oral sepsis. In Osler and McCrae's "System of Medicine" [20] three factors are mentioned which are considered necessary for the production of the gastric lesion: (1) trauma of the gastric mucous membrane; (2) a circulatory disturbance, apparently of the mechanical type mentioned; (3) the acidity of the gastric juice. Since it has been found in the group of ulcerative cases that the ionic calcium deficiency is uniformly present, it is possible that

the origin of gastric ulcer may be primarily due, in some cases at least, to a local trauma, the failure of which to heal, and the subsequent ulceration, is due to the lack of ionic calcium, which in turn is dependent on a partial parathyroid deficiency. This view is further supported by Friedman's work, for he found that in parathyroidectomized rabbits acute ulcers of the stomach or duodenum frequently developed, and that they showed no tendency to heal spontaneously. He forms the theory that in man such lesions may be due to thyroid deficiency. The acidity of the gastric juice may be an adjuvant factor, more probably delaying healing after ulceration has occurred, but its activity is probably secondary rather than primary; in Allbutt and Rolleston's "System of Medicine" [22] the statement is made that gastric ulcer may be produced experimentally in a variety of ways, but that such ulcers invariably heal spontaneously and do not tend to spread. It seems reasonable to suggest that the cause of healing in this case is that the ulcers are produced in healthy animals, in which the blood calcium is normal; in man however, the faulty condition of the blood calcium may predispose to ulceration following a local trauma.

The results obtained by parathyroid therapy, which are consequent on the restoration of the normal amount of ionic calcium in the blood, suggest the hypothesis that while the combined calcium of unclotted blood is essentially coagulative, the ionic calcium is closely associated with tissue resistance and repair. This is further borne out by the partial but undoubted improvement which is obtained by the injection of calcium salts without the addition of parathyroid substance, though it is difficult to understand how this is brought about. Estimations of ionized and total calcium made at short intervals for the twenty-four hours following an injection, showed a maximal increase in the ionic calcium in about eight hours, after which the amount tended to decrease; the total calcium however was not decreased at the end of twenty-four hours. It would seem, therefore, that part of the injected calcium has entered into combination with some substance but that it is not actually excreted from the system. If, then, twenty-four hours after the injection of one grain of calcium chloride the ionic calcium of the blood is already returning to its original low level, it is difficult to understand how one injection can cause an increase in the ionic calcium at the end of seven days as it apparently does. Assuming that some toxic substance is present in the blood of these cases with which the ionic calcium combines, it is not clear whether the ionic calcium acts simply by chemical combination, reducing the amount of toxin, or whether it acts purely physiologically. The physiological action which would suggest itself would be a direct stimulant effect on the parathyroid glands, but such action is not yet proved, and on the majority of organs the action of calcium ions is depressant rather than stimulant.

In the ulcerative cases, the results of this form of treatment seems to substantiate certain facts which have already been mentioned. First they are in accordance with the two parathyroid functions which have been defined by other workers, the function of detoxication and the function of control of calcium metabolism. Secondly, there is evidence to emphasize the importance of the ionic calcium of the blood in the repair of the ulcerated areas, and since calcium metabolism is controlled by the parathyroids, the property of tissue repair must be assigned indirectly to these glands. Finally, the beneficial effects obtained by the use of parathyroid substance in ulcerative conditions suggest that that state is at least partially due to an insufficiency of parathyroid secretion, or of one element of the secretion in the patient.

A type of ulceration which has not yet been mentioned, is malignant ulceration. Here the calcium deficiency is also present, but it is in all probability secondary to the ulcerative state. Goldzieher [23] has shown that calcium salts are to some extent inimical to cancerous growths, but it is not expected that any very marked change will occur in the growth itself following the administration of parathyroid substance to the carcinoma patients under observation. The effect such treatment might have would be to rectify the blood calcium balance, and thereby raise the resistance of the tissues, so that life might be prolonged without definite inhibition of the increase of the actual neoplasm. Practically, the chief effect is the marked decrease of pain in inoperable cases.

It is noticeable that the common factors in these cases, which represent various types of ulceration, are first a protracted toxic condition and secondly a deficiency in the ionic calcium of the blood. This would perhaps suggest that though these conditions are classified and treated as separate diseases, yet the underlying cause may be the same throughout. Similar treatment should therefore benefit them all, and this has been found to be the case. It would be possible to form a speculative hypothesis—and this is in some degree supported by the investigation of cases other than those mentioned—that all acute or chronic toxæmias react to a greater or less degree on the parathyroid glands and cause a more or less permanent damage, a disturbance of calcium metabolism and a consequent decrease in the resistance of the tissues. It is of interest to find in the literature of experimental parathyroidectomy that post-operative septic states in dogs are not uncommon [24]. Thompson and Leighton [25] were able to produce chronic parathyroid deficiency by ligation of the glands and reported that the most prominent symptoms were a progressive loss of weight and a greatly diminished resistance to infection. It is therefore possible that parathyroid therapy may be of use not only in such chronic conditions as have been discussed, but also during the acute attack of the more common infective ailments, especially those in which the sequelæ tend to chronicity.

In conclusion, I express my thanks to Dr. R. Grove, who has undertaken the treatment of the patients and for whose many valuable and helpful suggestions I am much indebted.

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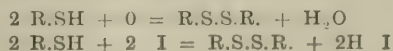
Section of Therapeutics and Pharmacology.

President—Dr. W. LANGDON BROWN.

The Therapeutic and Pharmacological Action of some New Sulphur Compounds.

By J. E. R. McDONAGH, F.R.C.S.

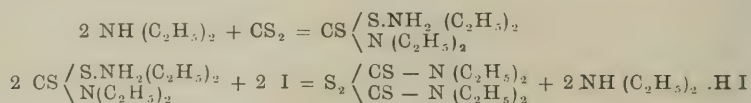
FOR some time past I have been experimenting with various sulphur compounds with the hope of being able to find one sufficiently soluble in water for intravenous use. Di-ortho-di-amino-thio-benzene or intramine does not lend itself well for this purpose, because it has to be rendered so dilute and has to be so highly protected against producing shock. The more highly protected a colloidal suspension of a drug the more its therapeutic action is diminished. The compounds I found to be most suitable were the carbon-di-sulphide products of di-ethyl-amine and of di-methyl-amine. These compounds are not unknown to chemists, as they were first prepared by Hoffmann some years ago, but as far as I know they have not been used medicinally or commercially. Though it is extremely difficult to prove how a drug acts chemically in the body, I believe the vehicle of the compound, mainly through an amino-group, brings about adsorption with the protein particles in the serum in the process of which the sulphur is liberated and acts as a di-sulphide. Experimenting with the thio-benzenes I found that the compound which had the amino-group in the ortho-position was the most active therapeutically, then the compound with the amino-group in the meta-position, and finally the compound with the amino-group in the para-position. This was my reason for selecting di-ortho-di-amino-thio-benzene. If intramine is injected intramuscularly into a rabbit mercaptan is formed locally, a body which rapidly disappears on oxidation and under the influence of iodine; presumably the mercaptan is converted into a di-sulphide.



In support of this view is the fact that the poisonous properties of ether, which Mackenzie Wallis showed to be due to the formation of mercaptans, vanish if the anaesthetic is passed over iodine or is treated with a permanganate. I therefore thought that by combining intramine with iodine I could practically double the action of the former. It is interesting to note, as it has a direct bearing upon this point, that if the carbon di-sulphide products of di-ethyl-

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amine and of di-methyl-amine are treated with iodine an S_4 body, along with hydroiodic acid, is formed.



The methyl bodies need not be considered, because they are more toxic than the ethyl bodies, because di-methyl-amine is more expensive to prepare than di-ethyl-amine, and because the iodized product of di-methyl-amine is insoluble in alcohol. In many instances the methyl-group is more toxic than the ethyl-group, as evidenced by the readiness with which methyl alcohol will produce optic atrophy, and by the fact that ether made from methyl-alcohol and sulphuric acid is more toxic than the product made from ethyl-alcohol. All non-metallic organic compounds containing the sulphur group of elements, which includes sulphur, selenium and tellurium, when injected in repeated small doses, cause the serum which was previously negative to give a positive Wassermann reaction. As their continued administration changes the positive into an anti-complementary reaction, the advent of this stage is a good sign of toxicity, because it takes longer to reach with some preparations than with others. From my experimental work on the Wassermann reaction and upon the action of certain metals and non-metals in rabbits, I have formed the conclusion that the protein particles in the serum do not adsorb complement in the absence of the antigen unless they have undergone considerable damage. Further, this amount of damage is suffered before the parenchymatous cells of the important viscera are attacked. The CS_2 compound of di-methyl-amine renders the Wassermann reaction anti-complementary in a shorter time than the corresponding ethyl body.

There is no doubt that these non-metallic preparations, when they reach the blood, have a physical action as well as a chemical action, and at present it is impossible to tell where one ends and the other begins; but it is from their physical action that we are able to measure their degree of toxicity. When the CS_2 compound of di-ethyl-amine is injected intravenously it retards coagulation, it diminishes the number of the protein particles visible with the ultra-microscope, and it causes many of them to aggregate into clumps. In other words, it causes condensation of the protein particles and diminishes both the surface tension and viscosity of the serum. In the first stage of condensation the sugar in the blood diminishes, and in a subject who has albuminuria the urea concentration is also lessened. Metals which cause dispersion do exactly the opposite. In a later stage the sugar in the blood is increased and the hyperglycæmia produced may be so great as to bring about glycosuria. In the last stage the animal develops paraplegia of the hind quarters with sphincter incontinence and dies from a transverse myelitis due to vascular degeneration. If rabbits are killed during these stages the following important visceral lesions are to be found. At first the liver and kidney cells contract, later they swell, and then the liver becomes profusely infiltrated with glycogen (determined by Best's carmine method). Later still the glycogen disappears from the lobules, from the periphery of each lobule first, and finally the liver is left two or three times its normal size. Though the cellular infiltration may be most marked around the central vein it is usually of portal origin. The renal changes are in the main vascular. The interlobular arteries show marked hyperplastic sclerosis, usually in the form of sub-endothelial proliferation. The

arterioles show proliferation of the intima and then fatty infiltration. The cells of the Malpighian tufts undergo similar changes. At first a small round-celled infiltration, with and without plasma cells, which appears to be of lymphatic origin, picks out and surrounds here and there a Bowman's capsule. Later it gives rise to lamination of the capsule and complete hyaline degeneration of the glomerulus involved. These mesenchymatous changes are peculiarly local. The vessels in the spleen, which is also hypertrophied, likewise undergo hyperplastic sclerosis and arterio-sclerosis, but not by any means conformably with those in the kidney. For instance, the arterioles may show well-marked arterio-sclerosis in the kidney and no more than proliferation of the intima in the spleen of the same animal. Hyperplastic sclerosis also affects the arteries in the lungs and both forms of sclerosis affect the vessels in the central nervous system. There is no difference between the changes produced by non-metals in both the blood and organs of rabbits and those produced in man by syphilis. A positive Wassermann reaction is obtained in both, and a normal saline extract of the liver makes as good an antigen as an extract of a congenital syphilitic liver, while a similar extract of a normal rabbit's liver possesses no antigenic properties. Furthermore, the visceral changes are parenchymatous if affected early and mesenchymatous or cirrhotic if affected late. Injecting intravenously 0.25 gm. dissolved in 3.0 c.c. of normal saline, at weekly intervals, of the CS₂ compound of di-ethyl-amine, between 24 and 30 c.c. are required to kill a rabbit weighing roughly 2 kilos. At first sight, owing to the ease with which both alkalis and acids liberate carbon di-sulphide, it might be thought that the compound in question would be toxic, but such is not the case, as the alkalinity of the blood is not sufficiently powerful. Large doses can be injected into man with impunity, provided the solution is diluted and protected with glucose, because, oddly enough, concentrated solutions, while not embarrassing rabbits, are apt to produce transient splanchnic shock in human beings. The percentage of sulphur in the CS₂ compound of di-ethyl-amine is 28.4 per cent. and the solubility in normal saline is 1 in 2.5. I have found that patients suffer no inconvenience from repeated doses of 0.25 to 0.5 gm. dissolved in 1.5 to 3.0 c.c. of normal saline for intramuscular use and of 0.25 gm. dissolved in 10 c.c. of normal saline containing 10 per cent. of glucose for intravenous use. If an overdose has been administered, the effect produced can be nullified immediately by injecting a metallic preparation, such as I advised in mustard-gas poisoning. The iodized compound contains 9.2 per cent. of sulphur and 50 per cent. of iodine; therefore it is more than probable that it is a mixture with a considerably larger proportion of NH(C₂H₅)₂.HI than is indicated in Hoffmann's formula. The iodized compound is soluble 1 in 6.2 of absolute alcohol. If one part of the alcoholic solution is mixed with two parts of the peptone mixture contained in intramine No. 2, 3 c.c. can be repeatedly injected intramuscularly with impunity and without causing pain. It is extremely likely that the ethyl-groups impart anæsthetic properties to the compounds. I have employed both products continuously for several months, and have formed the opinion that the CS₂ compound of di-ethyl-amine is a more powerful therapeutic agent than its iodized body and that both are on the whole more active than intramine. This is probably owing to the greater ease with which the non-metal is dissociated in the CS₂ products. These bodies have the same uses as intramine. They can be employed locally to heal sluggish wounds and in the form of pessaries in cases of chronic cervicitis. They can be injected intravenously and intramuscularly to prevent and combat metallic intoxication. They are particularly useful in certain late syphilitic

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lesions, in chronic gonococcal complications, arthritis of varied ætiology, and in certain chronic dermatoses, especially acne rosacea and vulgaris and seborrhœic dermatitis. Broadly speaking, they are useful in all cases in which fibrosis is occurring and has formed, provided the fibrous tissue has not become completely organized. The compounds are more active and at the same time less toxic than allyl-thio urea (thiosinamine) and its sodium salicylate (fibrolysin) and ethyl-iodide (tiodine) compounds.

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Section of Tropical Diseases and Parasitology.

President — Sir LEONARD ROGERS, C.I.E., M.D., F.R.S., I.M.S.

Eimeria oxyspora, Dobell, 1919, found in a Specimen of Human Fæces in England.

By W. BROUGHTON-ALCOCK, M.B.

(Director, Central Laboratory, Ministry of Pensions),

and J. GORDON THOMSON, M.B., Ch.B.

(Director of Protozoology, London School of Tropical Medicine).

THIS parasite was found in one stool and not on examinations of fæces made seven days previously, and eleven to fifteen days afterwards. It would therefore appear that a further report on the clinical history of the case and the rôle of the *Eimeria oxyspora* as an ætiological factor in any disease producing symptoms should await until repeated examinations are carried out. Moreover the possibility that the oöcysts, in which form the parasite was found, were taken in with some uncooked foodstuffs, and were passing through the intestines, must be ruled out.

The patient came to Dr. Douglas Kirkwood for treatment of digestive disturbance, debility and constipation, and it was at the stool examination he requested that the parasite was unexpectedly found. There had not been any diarrhœa. The patient, aged 30, was born in London, and, save for pleasure visits to the Continent at various short periods and to Malta for two months last year, has not been out of England.

The diet of the patient for some days before the finding of the parasite was light, no meat and only moderate amounts of boiled fish were eaten. The patient would not take milk, butter or obvious fat and this is mentioned because an outstanding character of the fæces on each occasion was the number of fat particles present.

Macroscopically and microscopically no blood or mucus and no pathological exudate were seen in the fæces, and on aerobic culture no organism of pathological significance was isolated.

Entameba coli cysts (many) and *Trichocephalus dispar* ova were found at the four examinations made. The urine showed no abnormality and indican was not present. The total and differential blood counts and hæmoglobin estimation made on two occasions were normal.

Before passing on to a detailed description of the parasite its rarity in human fæces may be noted as outstanding, since, during and following the Great War, the number of microscopic examinations of fæces must have amounted to hundreds of thousands and only in the case cited by Dobell, 1919, in England has the finding of *Eimeria oxyspora* been previously recorded.

The authors wish to proffer their warmest thanks to Dr. Wenyon and Dr. Castellani for their interest and helpful discussions during the progress of the

work, and to Dr. Robertson (Grocers' Research Scholar) for the excellent and accurate camera lucida drawings he has made of the various stages of the oöcysts.

THE MORPHOLOGY OF *Eimeria oxyspora*.

The oöcysts were found in the stools at all stages of development from those containing only a single undifferentiated mass to the fully developed forms with four sporocysts, each containing two sporozoites. The fully differentiated forms were by far the most numerous. The specimens were studied in the fresh state only.

In shape the oöcysts are spherical, and show considerable variation in size. The smallest specimen measured had a diameter of $33.6\ \mu$, and the largest a diameter of $50.6\ \mu$. The average diameter of the oöcysts in this case was about $42.5\ \mu$. The cyst wall, which could be made out in some specimens to be composed of two layers, varies in thickness with each individual, but on the average is about $0.5\ \mu$ thick. The wall of the oöcyst is easily distorted by pressure especially in the young forms.

In the earliest stage of development the contents appeared as a coarsely granular rounded mass separated from the wall of the oöcyst. In its long axis this granular mass measured $27\ \mu$. No nuclear structure could be made out, but even at this early stage there was a certain amount of oöcystic residuum (see Plate I, fig. 1).

In the next stage seen this single granular mass was divided into four masses, each of which were similar in appearance to the parent mass, that is to say coarsely granular and without demonstrable nuclear structure and delimiting wall or membrane. The residual material in the oöcyst is well marked at this stage. In the case of the specimen figured (see Plate I, fig. 2) the average length of the sporoblastic masses was $14.6\ \mu$.

Each of these four sporoblasts has a wall secreted round it (see Plate II, fig. 1) and they begin to take the characteristic shape of the sporocyst, which is elongated, fusiform, and has ends, which, in some instances, are blunt and rounded, whilst in others, but more rarely, they are sharp and pointed. The contents within the sporocyst wall are at first of the coarsely granular nature seen in the sporoblastic stage, but with the formation of the sporozoites the structure begins to change. The sporozoites first appear at either end of the sporocyst as oval bodies which are finely granular in structure and contain several highly refractile particles, some appearing to have a clear area around them (Plate II, fig. 2). No nucleus could at this stage be definitely determined. In addition to the developing sporozoites the sporocyst still contains a considerable quantity of undifferentiated, coarsely granular cytoplasm, and, also, a few apparently spherical masses which look not unlike oil droplets, and which are probably sporocystic residual material. When mature the oöcyst contains four sporocysts with two sporozoites in each and a varying quantity of oöcystic residuum. The average length of the sporocysts is about $28\ \mu$, but they may vary from 25 to $30\ \mu$ in length, while in breadth the average is 6 to $7\ \mu$. The sporozoites are typically sausage-shaped, and, as a general rule, the ends are blunt and rounded, but the anterior end, that farthest from the nucleus, may be produced into a point. They lie in the sporocyst pointing in opposite directions and lying side by side for most of their length, occasionally a portion of one overlapping the other. Thus the anterior end of one sporozoite is close to the posterior end of the other. The nucleus, while situated towards the middle of the body, is rather nearer the posterior than the anterior

end, and is spherical in shape with a large somewhat diffuse karyosome (Plate III, figs. 1 and 2). In parenthesis it may be mentioned that the terms anterior and posterior are used in the same manner as Dobell used them in his original description. In the majority of the specimens peculiar dark, sharp pointed, fusiform bodies could be made out at the posterior end, and, occasionally, these were surrounded by a clearer area or vacuole. The nature of these bodies is unknown. The sporocystic residuum may be small in amount—merely a few granules—or it may be present in masses which measure up to 3 or 4 μ in diameter, and, as mentioned above, its appearance is very suggestive of oil droplets. The sporozoites do not show great differences in length even when there is great variation in the containing sporocysts. In our case the average length of the sporozoites was 19.6 μ . In the width there were much greater differences than in the length, but the average was about 3 μ .

COMPARISON WITH THE OTHER HUMAN EIMERIA.

In his original description of *Eimeria oxyspora*, Dobell, in 1919, laboured under the great disadvantage of being able to find the parasite only in extremely small numbers, and such as he did find were at the stage of complete differentiation. In this case all the stages of development of the oöcyst occurred in the faeces.

It must be noted that Dobell gives about 36 μ as the diameter of the oöcysts found by him, whereas, in this case, the average diameter is 42.5 μ , while they varied between 33.6 and 50.6 μ . The sporocysts in Dobell's case measured from 30 to 32 μ , with a width in the middle of about 7.5 μ , but in this case the average length is 28 μ , the individuals varying from 25 to 30 μ , with a width in the middle of 6 to 7 μ .

This comparison between Dobell's original measurements and those made in the present case is of especial interest in view of the discovery of *Eimeria snijdersi*, Dobell, 1921. Dobell gives as the measurements of *Eimeria snijdersi* the following: Oöcysts 40 to 48 μ , sporocysts in length 20 to 25 μ , width at the middle 7 to 8 μ .

The fact that the dimensions of *Eimeria oxyspora* are subject to such variations, as pointed out above, is extremely significant and renders it possible that *Eimeria snijdersi*, Dobell, 1921, may merely be a variety of this species. In his paper on *Eimeria snijdersi* Dobell says: "Although I was struck, at first, by the resemblance of these oöcysts to those of *Eimeria oxyspora*, it seems clear from their dimensions and those of their spores, that they belong to a distinct species." It is obvious from Dobell's remarks that the main factors which led him to create a new species were the dimensions of the oöcysts and sporocysts which he obtained from material previously fixed and stained. In the type species of *Eimeria oxyspora*, owing to the oöcysts being so few in number and difficult to find, Dobell was only able to give as the diameter of the oöcysts "about 36 μ ." The range of size of the oöcysts and sporocysts of Snijder's case did not correspond with the measurements of the original *Eimeria oxyspora* as described by Dobell and this led him to create a new species. In view of the measurements of the oöcysts and sporocysts in the present case as stated above it seems necessary to raise the question as to whether *Eimeria snijdersi*, Dobell, 1921, is a species distinct from *Eimeria oxyspora*, Dobell, 1919.

It was possible to study the nuclei in many of the fully developed sporozoites in our case, and accurately to fix their positions, points about which

Dobell appeared to have some doubt from a study of the few specimens present in his case, since he says: "The posterior end is rounded and contains an oval body, lying subterminally, which is probably the nucleus." The nucleus is spherical, has a definite karyosome, and, while nearly central, is placed rather nearer to the posterior than to the anterior end. While it is possible to confirm Dobell's statement with regard to the presence of the "two or three very bright and small bodies, fusiform in outline and longitudinally disposed" at the posterior ends of some of the sporozoites, it must be mentioned that they could not be made out in every specimen. Further, while a comparatively clear area in the cytoplasm could be defined "subterminally" in some sporozoites nothing like a nuclear structure could be made out in this situation.

DESCRIPTION OF THE PLATES.

All the specimens were drawn with the camera lucida at a magnification of 2.200 approx. The sizes and various measurements were checked by photomicrographs, which, however, owing to the contents of the oöcysts lying in different focal planes, were quite unsuitable for reproduction.

PLATE I.

FIG. 1.—Shows an oöcyst with the contents at a very early stage of differentiation. Diameter 40.5μ . Long axis of the cytoplasmic mass 27μ , short axis 23.5μ . Larger residual mass in its long axis 2.7μ . Cyst wall 0.5μ . No nuclear structure could be made out.

FIG. 2.—Diameter of the oöcyst 40.4μ . The contents have broken up into four cytoplasmic masses, more or less pyriform in shape, the average length of which is 14.7μ . No nuclear structure is as yet visible. Two definite masses of oöcystic residuum are present.

PLATE II.

FIG. 1.—Diameter of oöcyst 38.4μ . Average length of the sporocyst 28.3μ , with a width in the middle of 7.4μ . In this instance there were three masses of residual material in the oöcyst. The contents of the sporocysts were coarsely granular, and no nuclear structure was visible.

FIG. 2.—Diameter of the oöcyst 39.5μ , and the average length of the sporocysts 29.8μ , with a width in the middle of 6.5μ . The residue in the oöcyst was, in this instance, very small in amount. This shows the sporozoites in an early stage of their formation as oval bodies at either end of the sporocyst, the remainder of the sporocyst being filled with coarsely granular, undifferentiated cytoplasm, and also a certain amount of the highly refractile oöcystic residuum. Within the bodies of the developing sporozoites there were also highly refractile particles, and, as stated in the text, these may simulate very closely a nuclear structure.

PLATE III.

FIG. 1.—Shows an oöcyst fully developed, but of a rather small size. Diameter 36.8μ . Average length of the sporocysts 25.3μ , width 5.4μ . Average length of the sporozoites 19.6μ , width 2μ . The oöcyst contains rather a large amount of residual material. The sporozoites show at their posterior ends the crystal-like, whetstone shaped bodies. Two of the sporocysts show the refractile residual material.

FIG. 2.—Fully differentiated oöcyst with a diameter of 46μ . The average length of the sporocysts 27.5μ , and their width at the middle 6.5μ . The position of the nuclei in the sporozoites is towards the centre, but nearer the posterior than the anterior end. The sporozoites average 19.5μ in length, with a width of 3μ .

PLATE I

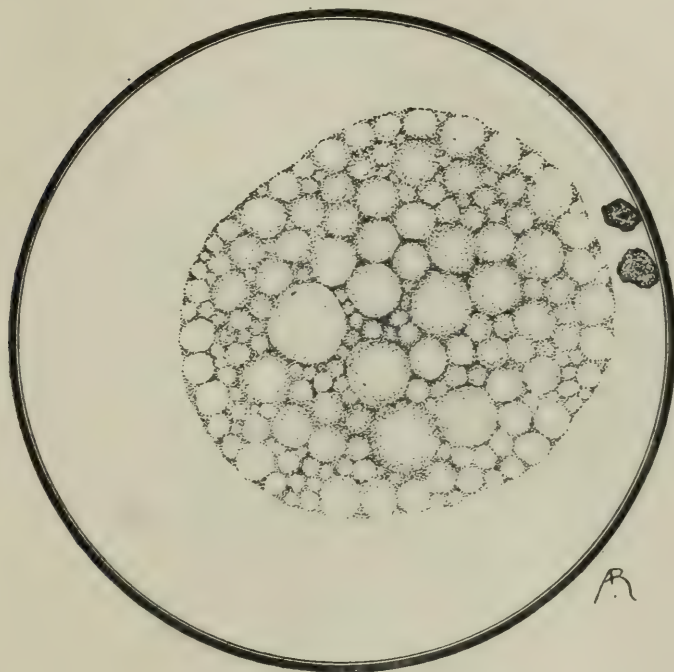


FIG 1.



FIG. 2.

A Robertson del.

0 ————— 10
microns

PLATE II.



FIG. 1.

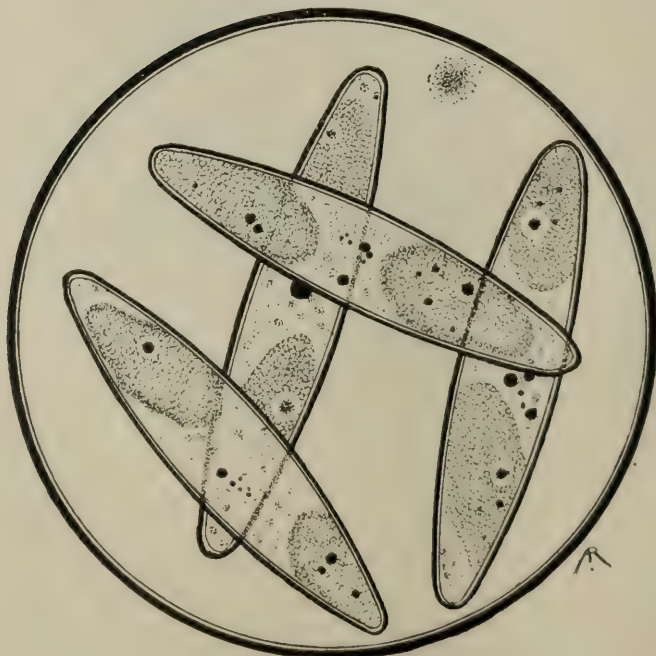


FIG. 2.

A. Robertson, del

0 ————— 10
microns

PLATE III.

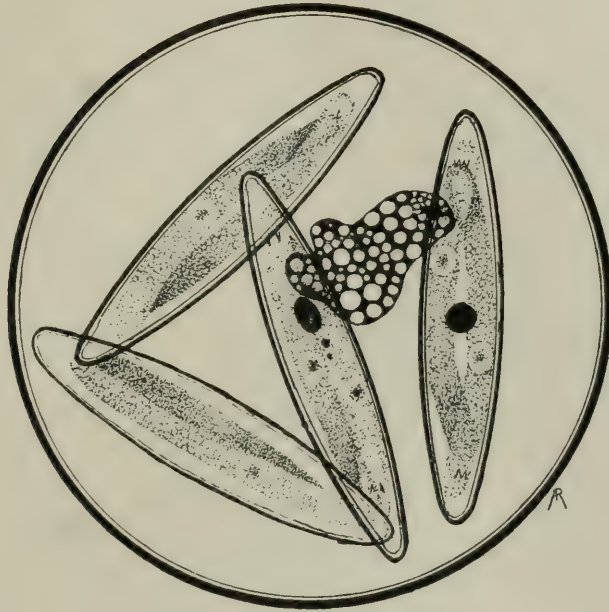


FIG. 1.



FIG. 2.

A. Robertson del.

0 ————— 10
microns

Embadomonas intestinalis (Wenyon and O'Connor), 1917;
Description of the Cysts and Free Forms found in a
Case in England.

By W. BROUGHTON-ALCOCK, M.B.

(Director, Central Laboratory, Ministry of Pensions),

and J. GORDON THOMSON, M.B., Ch.B.

(Director of Protozoology, London School of Tropical Medicine).

CLINICAL OBSERVATIONS.

THE patient, a man aged 54, came under the care of Sir Ronald Ross in October of this year. His case had been treated during the previous eighteen months, first as sprue and later as mucous colitis. The diagnosis of sprue was due to the fact that the patient gave a history of passing loose clay-coloured stools, but that they were not due to sprue was early recognized. A sigmoidoscopic examination revealed no abnormal condition of the mucous membrane as far as it could be seen.

He had been resident in China five years, Japan eighteen, Eastern Siberia two, then again in Japan two years, till his return to England. In 1896, while in China, he had an attack of undetermined dysentery and diarrhoea, which cleared up under treatment with beta-naphthol and bismuth subnitrate. He had no further intestinal trouble till at Vladivostock in 1916, when he passed frequent stools without blood or mucus, and a diarrhoeic condition has persisted since. In March to July of 1919 he was under treatment at the U.S. Naval Hospital, Yokohama, by Captain Raymond Spear, U.S.N., who found *Entamoeba histolytica* in the faeces. A report states recovery from this infection after treatment with emetine and ipecacuanha combined with quinine irrigations. But the general health of the patient remained below par, blood-pressure very low and digestion poor, which was considered due to the amoebic infection. A more suitable climate was recommended, and patient returned to England. The looseness of the bowels however has persisted and two or three soft stools daily were passed.

On October 4 of this year a specimen of the faeces was partly formed and partly soft. The formed part was pale coloured but not characteristic of sprue, and it was such portions seen that led the patient to speak of passing clay-coloured stools. The unformed part of the stool was normal in colour, and this has been the character of each stool examined since. The faeces were acid in reaction, and no macro- or microscopic blood or mucus was seen. There was no pus, and only a moderate number of small epithelial cells. The occult blood test was distinctly positive, but it must be added the patient's diet included meat. The number of *Embadomonas intestinalis* present may be judged from the fact that in any portion of the stool taken and examined under the $\frac{1}{2}$ oil immersion, one to several of the parasites could be seen. No *Entamoeba histolytica* was found at this or later examination, and on aerobic culture no organism of the dysenterica or enterica groups was isolated. A streptococcus in long chained form was seen to be numerous in direct films and on culture. Enterococcus was also present in great numbers.

A soft stool with similar findings was noted on October 7 and 21. The treatment given had been pulv. ipecacuanhæ and acidus tannici at nights.

From November 1 to 16 the stools were formed; *Embadomonas* was rare, and the great majority of the forms cystic. The patient states that the stools have been more formed and only one has been passed *per diem* since adding a morning dose of beta-naphthol to the above treatment.

The important question of pathogenicity to man of this intestinal parasite has to be considered. Unfortunately in this case it is difficult to determine, as there is a definite history of *Entamoeba histolytica* infection, and of its cure in 1919. The condition of the debility and lassitude still persisting may be due to the unrelieved state of diarrhœa consequent upon the irritation due to the great numbers of the parasites present in the intestine. When the stools are formed these are few, a condition so often observed in intestinal infections with other flagellates. It can only be stated that the patient has had a long persistent diarrhœa and the parasite was present in considerable numbers in the loose stool. Animal experiments are being made. An examination of the blood revealed nothing abnormal. The urine revealed no abnormality save a high amount of indican on each occasion of examination. It seems of importance to make known the findings of this parasite in England, though the infection was probably contracted in Eastern Asia: and of epidemiological interest to mark the extension of its endemic areas, since its detection in human fæces has been only reported by Wenyon and O'Connor in Egypt and by Kofoid, Kornhauser and Plate, 1919, and Hogue, 1921, in America. Opportunity to make a complete picture of the detailed structure and measurements of the forms of the parasite and record them by drawings has been taken advantage of, and we have warmly to thank Dr. A. Robertson for the admirable camera lucida sketches he has made. We also wish to thank Dr. Wenyon for his interest and collaboration in our work.

PREVIOUS OBSERVATIONS ON THIS PARASITE.

Wenyon and O'Connor, 1917, first described this parasite as occurring in the stools of two human patients in Alexandria, and they named it *Waskia intestinalis*. The genus *Embadomonas* had, however, been created by Mackinnon in 1911 for a flagellate from the intestines of trichopterous larvæ, and, since the generic diagnosis given by that author so closely resembled that of the *Waskia* of Wenyon and O'Connor, it was necessary to alter the name of the flagellate in the human stools to *Embadomonas intestinalis* (Wenyon and O'Connor), 1917.

Kofoid, Kornhauser and Plate, 1919, reported the presence of this flagellate in the stools of seven soldiers of the U.S.A. Army in New York, three of whom had served in other countries during the war and the remaining four had been on home service in U.S.A.

Hogue, 1921, described this parasite as occurring in the stools of a woman who had suffered from diarrhœa for many years. This observer has also been successful in cultivating the flagellate.

The present case is the first in which this flagellate has been recorded in Great Britain.

DESCRIPTION OF THE FLAGELLATE FORM.

This is a small flagellate, which, when freshly obtained, is very actively motile. When lying flat it is pyriform in shape with a blunt rounded anterior

end tapering to a point at the posterior end. Sideways on, its appearance has been described as bird-like by Wenyon and O'Connor and slipper-like by Miss Mackinnon. These descriptions are distinctly apt, since the presence of the cytostome on the antero-lateral aspect does give an outline to the body very suggestive of a bird or a slipper. It possesses two flagella: the first, directed anteriorly, is long and thin and is the chief organ of propulsion, while the second flagellum projects laterally from the cytostome, is much thicker and stouter than the anterior flagellum, and, in addition to assisting in the capture of food, probably its chief function, also aids in the movement of the organism as a whole. Fig. 1 shows the parasite lying flat, and fig. 2 the appearance of the parasite, as it were in profile. The summation of the driving forces from the two flagella, the one acting anteriorly and the other towards one side, produces a peculiar jerky movement of the body. Since the sideways movement is in the direction towards which the cytostome faces, it will be seen that this is a material aid to the capture and ingestion of the bacteria on which the animal feeds. Further, this method of progression, always provided that the organism does not turn over or otherwise change its direction, would result in the completion of a wide circle. Occasionally the *Embadomonas*, instead of following the method of progression just described performs a jerky and erratic turning movement as well as moving forward.

As stated above the cytostome is situated antero-laterally, or, perhaps it would be better described as being lateral but towards the anterior end. It is usually visible in specimens stained with Heidenhain's iron hæmatoxylin method. Of course it is not visible in every specimen, nor yet are the flagella, one or both of which may be hidden behind the body (*vide* figs. 16 to 19).

The nucleus is spherical and is situated near the anterior end. The karyosome is centrally placed and is large in size. The nuclear membrane is usually distinct and has on or near it two black staining dots, the blepharoplasts, from which the flagella originate, the situation of which is usually on the antero-lateral quadrant nearest the cytostome.

In size the measurements in the present case correspond with those given by Wenyon and O'Connor. In length the minimum was 3.5μ and the maximum 9μ , while the width at the broadest part varied between 1.5 and 6μ . The majority of the organisms were from 5 to 6μ long by 3 to 3.5μ in breadth.

The usual method of division of *Embadomonas intestinalis* is binary fission as is shown in fig. 45, but occasional forms occur which indicate that multiple division may also take place. Thus fig. 43 shows a parasite the nucleus of which has divided into four. In this instance the flagella could not be made out. In fig. 42 it will be seen that the nucleus has divided into five and that from the neighbourhood of three of these daughter nuclei flagella are seen to arise; but that also there are four other flagella passing out from the side, which suggests, in view of the close association between the flagella and the nucleus, that there has been further nuclear division which is not visible.

THE CYSTS.

In the fresh state the cysts of *Embadomonas intestinalis* are minute pear-shaped bodies in which it is impossible to make out the internal structure even when double strength Gram's iodine has been added. It was necessary, therefore, to draw the various specimens from wet fixed preparations stained with Heidenhain's iron hæmatoxylin. Figs. 21 to 31 and fig. 44 are cysts from the present case, but figs. 35 to 41 and also figs. 32, 33 and 34, showing free forms, were drawn from Wenyon and O'Connor's original type specimens.

Just as in the case of the free forms the measurements of the cysts correspond very closely to those given by Wenyon and O'Connor. They vary in length between 4 and 9 μ (approx.) and in width at the broadest part from 2.5 to 3.75 μ .

The contents of the cyst are composed of finely granular, almost homogeneous, cytoplasm and a body which, in the majority of specimens, has a definite, distinctly stained dark outline, and corresponds roughly in shape to a carrot—blunt at one end, and tapering to a point at the other. In the interior of this a dark dot can usually be seen, and often a line running the whole length of the space, although sometimes this line can only be made out for part of the distance (see figs. 22, 23, 24, 26, 35, 37, 39). In other cysts there seem to be two dark dots separated from each other, but occasionally they may be joined by a fine thread of dark staining material, giving an appearance like that of a dumb-bell (see figs. 27, 35, 38, 40 and 41). Fig. 30 shows the two dark dots more widely separated, but this fine line still joins them together.

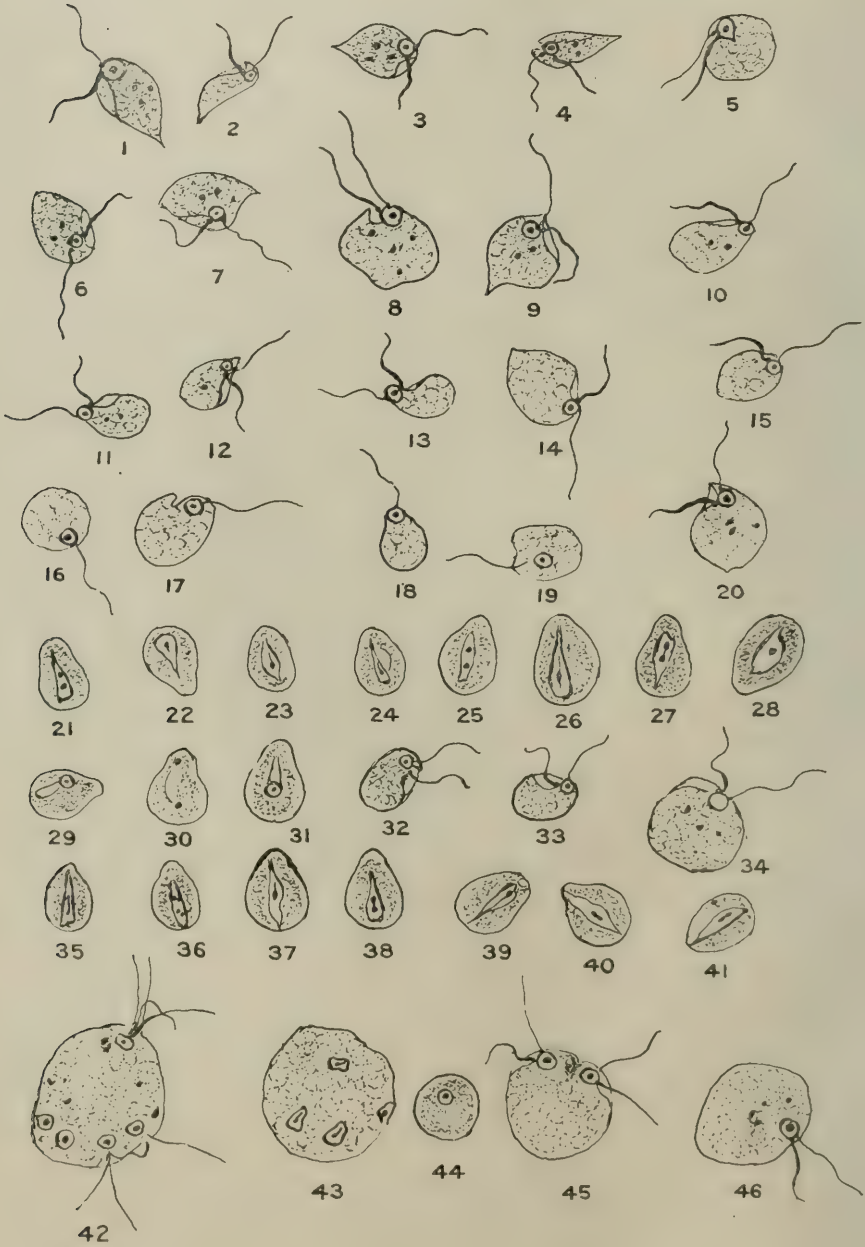
It is an extremely difficult question how to interpret this definite structure so typical of the cysts of this flagellate. Is it an elongated nuclear membrane, the dark dot being the karyosome, or is the elongated space with its siderophilous margin the cytostome?

On studying the cyst described by Mackinnon, 1912, in the type specimen of the genus *Embadomonas*, viz., *Embadomonas agilis*, Mackinnon, from the larvæ of *Trichoptera*, and *Embadomonas alexieffi*, Mackinnon, 1912, from the larvæ of *Tipula*, it seems evident that the elongated oval body in these is the cytostome, the nucleus being a separate body vesicular in type or represented by a number of dark dots with no distinct nuclear membrane. In Mackinnon's cyst specimens the nuclear membrane could be definitely made out in some preparations, with a karyosome in the centre, and lying in close proximity to it the elongated outline of the cytostome. In the cytostome there could also be seen a line running longitudinally which was obviously a flagellum. In the case of *Embadomonas intestinalis*, Wenyon and O'Connor, under consideration here, it is almost certain that the elongated body in the cyst is the cytostome, and the darkly stained dots are part of the nuclear structure, possibly chromatin, while the nuclear membrane in most cases is invisible. The fine line running along the space may be evidence of the flagellum which projects from the cytostome in the free forms. In fig. 31 it will be noticed that the nuclear membrane was distinct around the karyosome, and what possibly was the cytostome was lying beneath it.

Fig. 29 shows a cyst which is extremely like a small *Chilomastix* cyst, but it is possible that this may be an unusual form of the cyst of *Embadomonas*. This figure is of interest because the cyst figured by Dobell and O'Connor, in their book "The Intestinal Protozoa of Man," 1921, closely resembles it, and also a cyst of *Chilomastix mesnili*. Indeed, neither the flagellate nor the cyst of *Embadomonas intestinalis*, as figured by these authors, is characteristic of the type as described by Wenyon and O'Connor, 1917, and certainly is not the typical form as found in the present case.

Fig. 44 from this case is possibly an end-on view of the cyst. Wenyon and O'Connor figure similar specimens in their original description.

12 Broughton-Alcock and Thomson: *Embadoomonas intestinalis*



J. G. THOMSON, del.

10 microns.

DESCRIPTION OF THE PLATE.

All the specimens figured were drawn with the camera lucida at a magnification of 2,200 approx., from wet fixed preparations stained with Heidenhain's iron hæmatoxylin. Figs. 32 to 41 were drawn from the original type specimens of Wenyon and O'Connor.

FIG. 1 shows a typical flagellate.

FIG. 2 illustrates the so-called slipper or bird shape of the side view.

FIGS. 3 to 15, 20.—Varieties of the flagellate forms.

FIGS. 16 to 19.—Flagellates in each of which only one flagellum is visible.

FIGS. 21 to 31.—Cysts.

FIGS. 22, 23, 24 and 26 show the thin line joining the black dot to the end of the carrot-shaped space.

FIGS. 21, 25, 27 and 30 show the (?) karyosome in the process of division.

FIGS. 32 to 41.—Free and encysted forms from Wenyon and O'Connor's original type specimens.

FIG. 12.—Flagellate form which is undergoing multiple division. Five nuclei can be seen, but the four flagella projecting from the upper end suggest further nuclear division which is not visible.

FIG. 43.—Multiple division of the flagellate. Four nuclei can be seen, but no flagella.

FIG. 44.—A cyst of *Embadomonas intestinalis* viewed end-on.

FIG. 45.—Flagellate form, showing binary fission.

FIG. 46.—A large free form, with two flagella.

Observations on the Effect of Tartar Emetic on the Eggs and Miracidia of *Bilharzia hæmatobia*.

By M. KHALIL, M.D., D.P.H., D.T.M.

(From the Department of Helminthology, London School of Tropical Medicine.)

THE therapeutic action of tartar emetic in bilharzia has not yet been explained in scientific terms. The clinical progress of the cases under this treatment is at least comparable to, if not more striking than, the effect of quinine in malaria. The drug is specific in its action not only in bilharzia, but also in kala-azar and oriental sore, diseases with quite different ætiological factors. Its curative value in other diseases is still undetermined.

Dr. Christopherson in his article in the *British Medical Journal*, December 4, 1920, p. 854, claimed that tartar emetic as such is lethal to the adult worms in the portal system and to the ova in the tissues and the urine. He experimented with the eggs and the miracidia *in vitro* to support his claim that tartar emetic is a powerful direct poison which prevents the hatching of normal eggs and kills hatched miracidia. According to Christopherson, 2 gr. of tartar emetic in 6 c.c. of warm water (133° F.) added to $\frac{1}{10}$ c.c. of urine containing living bilharzia eggs, prevented the hatching process altogether. I have repeated this experiment with the same result. It is a fact that tartar emetic in the dilution of 1 in 46 invariably prevents the hatching process, whilst in lower dilutions, with the use of 1 gr. in 6 c.c. (1 in 92), a fair number of eggs are hatched.

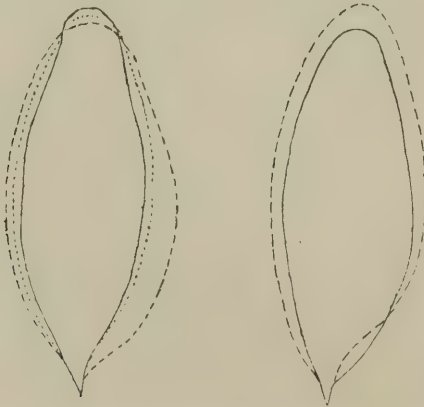
Before any conclusions are drawn from these experiments, the mechanism of hatching under normal conditions ought to be studied. We already know certain facts regarding this point.

(1) To hatch the eggs water has to be added in large amounts. It is necessary to dilute the urine six to ten times.

(2) Warmth hastens hatching.

(3) Hatching occasionally occurs in the bladder in those cases of polyuria in which the salt constituents of the urine are deficient, as in chronic interstitial nephritis.

(4) The process as watched under the microscope begins with distension of the shell with clear fluid. The miracidium turns its head towards the spine, becomes active and revolves around its longitudinal axis. Distension ends in the bursting of the eggshell and the liberation of the miracidium. This is shown clearly in the accompanying camera lucida drawings taken during the process at short intervals.



Camera lucida drawings of eggs of *Schistoma hæmatobium* distending during hatching.

Collectively, these data suggest that the process of hatching depends upon the salt concentration of the fluid in which the eggs are present. The distension of the shell before hatching resembles the distension and the bursting of the bladder of animal membrane containing salt solution when placed in distilled water.

Several experiments were performed with sodium chloride and sodium sulphate in different strengths. These salts were found to prevent hatching of the eggs in greater dilutions than tartar emetic, under the same experimental conditions, temperature 130° F.

	Highest dilution preventing hatching	Ratios	Molecular weights
Sodium chloride, NaCl	0.43 per cent.	1	58.5
Sodium sulphate, Na ₂ SO ₄	1	2.1	142
Tartar emetic, K(SbO) C ₄ H ₄ O ₆	2.2	5.5	323.2

It is a remarkable fact that these dilutions and the molecular weights of the salts used have the same ratio, i.e., are isotonic with each other, having practically equal osmotic pressure.

It was also found that eggs after remaining unhatched for four hours in tartar emetic, 2 gr. in 6 c.c. water, were hatched normally on the addition of more distilled water. These results lead to the conclusions (a) that the hatching of the bilharzia eggs is governed mainly, if not entirely, by the osmotic pressure of the fluid, subject to the physical laws of osmosis. The egg contents are practically isotonic with 0.43 per cent. NaCl solution. (b) That "tartar emetic as used in these experiments acts by virtue of its being a salt and not as the specific drug that kills the bilharzia worms and their eggs inside the body."

The osmotic pressure of the blood and the tissue fluids is equal to 0.75 per cent. saline solution, a strength which *in vitro*, as probably *in vivo*, is the main factor that prevents the eggs hatching in the portal system or during their transit in the tissues. In this saline solution or an isotonic Ringer's solution, the eggs can be stored alive for a considerable time without being hatched.

The osmotic pressure of normal urine varies between wide limits. Starling measures it in terms of lowering of the freezing point as varying between 0.87-2.7 Δ while 1 per cent. saline is 0.6 Δ . This explains why, under normal conditions, the eggs are not hatched in the bladder. In abnormal conditions, as stated above, the osmotic pressure falls very much below this, and we may expect to find that when it is below the pressure equivalent to 0.4 per cent. NaCl hatching takes place in the bladder.

The amount of tartar emetic necessary to cure a case of bilharzia is about 30 gr. Half of this is excreted in the urine. Neglecting the part excreted by the intestine and the bronchial mucous membrane, 15 gr. is the maximum amount present in the body at one time. Probably it never reaches that extent. The average volume of blood in an adult is 4.5 litres, and thus the maximum concentration of tartar emetic in the body is 15 gr. in 4,500 c.c., or 1 in 4,500. If its action is due to the direct effect of the drug, a concentration near that ought to be lethal *in vitro* to the adults and eggs, but this is not the case.

A series of experiments were made to find out the effects of tartar emetic on the hatched miracidia. A concentrated solution in water killed them in thirty minutes. But a concentration greater than 1 in 200 failed to have any effect. Control was always carried out with miracidia in water under the same temperature and age after hatching.

We arrive at the conclusion that:—

(1) The action of tartar emetic in curing bilharzia is not due to its direct effect on the eggs.

(2) The hatching of the eggs is a matter of osmotic concentration of the surrounding medium.

(3) Miracidia are not affected by tartar emetic in a dilution approximating to that used during treatment.

The lethal effect observed by Christopherson was only secured by use of concentrated solution, e.g., 1 in 46 and 1 in 92, and is attributable to the physical effect of the solution rather than to its specific chemical action.

Section of Tropical Diseases and Parasitology.

President — Sir LEONARD ROGERS, C.I.E., M.D., F.R.S., I.M.S.

Thermotropism in Ankylostome Larvæ.

By M. KHALIL, M.D., D.P.H., D.T.M. & H.

(From the Department of Helminthology, London School of Tropical Medicine.)

THE metamorphosed ankylostome larva has been known for a long time to be very sensitive to changes of temperature, its activity depending upon the temperature of the surroundings. It is most active at 37° C., and stops movement altogether below 12° C. Apparently the first to record his observations on the reaction of larvæ to heat was Lutz in 1885. He observed that "ankylostoma larvæ, in spite of their activity, produce slight, if any, change of place, nor can they keep themselves afloat in water by their own efforts. They sink straight to the bottom of a vessel containing water, diverging but a little from the vertical line." With these statements Professor Looss agreed, adding that "in more solid media the writhing movement of the body leads to considerable change of place."

The above statements are found to hold good only if the medium in which the larvæ are present is of uniform temperature throughout. The larvæ are practically motionless when the temperature is below 15° C., especially if cooled suddenly from higher temperature, as often happens in taking a culture from the incubator at 25° C. and examining it at room temperature. Between 20° and 30° C. the larvæ show active wriggling movement and accomplish little change of position.

The condition however is different if a heated point is applied to the vessel containing the larvæ in water. Invariably they become more active and travel directly towards the source of heat. This can easily be verified under the microscope. A number of metamorphosed larvæ are placed in a watch-glass containing water and focussed under a 2 in. lens. The condenser is swung out. The end of a glass rod or a mounted needle heated slightly in the flame of a Bunsen burner, is applied to the bottom of the watch-glass, so that it can be seen through the microscope. The immediate effect is a manifest vigour in the movement of the larvæ. They begin to travel from all directions towards the end of the glass rod, and collect there. Larvæ which have been outside the field come into view and travel actively to the heated point.

When a large number of larvæ are used, the process can be watched with the naked eye as a wriggling opalescent mass which disperses gradually when the hot glass rod is removed, and re-collects when it is re-applied. If the heated point is shifted to another place the mass of larvæ follows it, producing a slightly curved column of active larvæ streaming towards the new point of application.

It is necessary that the glass rod should not be red hot, as in that case the effect of the heat on the larvæ is to induce spasmodic convulsive movements of

great frequency, resulting in no change of position. As the rod cools the movements become less convulsive, and all the larvæ travel towards the point of application as mentioned above.

If the heated point is applied 1 in. below the watch glass, the same effect on the larvæ occurs in a milder form. They become active and travel to a point in the watch-glass that is nearest to the source of heat. It is apparent also that few larvæ in the periphery of the watch-glass are not affected at all.

Encysted larvæ show a wriggling motion, but no change of position. The larvæ collecting round the hot point form a star shape. The individual larva lies radially, its cephalic end pointing to the centre. Occasionally a few larvæ are dislodged from their position by others, but they soon manage to come back to their former position.

If the heated glass rod or needle is held not vertically, but at an angle beneath the watch-glass, the larvæ collect in a line parallel to the direction of the rod.

After any of these experiments the larvæ become motionless for a time, although the temperature may be 20° C. or more. They then become gradually active again.

The above phenomenon is invariably present, not only in *Ancylostoma duodenale* larvæ, but also in the larvæ of *Necator americanus*, *Ancylostoma ceylanicum*, *Strongyloides stercoralis*, *Galoncus permiciosus* of the tiger, *Trichostrongylus douglasi* of the ostrich. It is totally absent in the case of several species of free-living nematodes, and in the larvæ of *Hæmonchus contortus*.

The statement of Lutz and Looss that the larvæ are unable to float in water and support themselves may be true in ordinary circumstances, although occasionally one or two larvæ out of a large number kept in a glass bottle may be seen through a hand lens swimming freely in the lower layers of the fluid. If the medium, however, is not of uniform temperature, the larvæ can swim free in the fluid of their own activity to reach the point of higher temperature.

The following experiment can be easily done to verify this point. A cover-slip upon which a spot is marked with a grease pencil is floated in a watch-glass containing metamorphosed ankylostome larvæ in water. The cover-slip should be about 0.5 to 1 cm. above the bottom of the vessel. The grease pencil mark on the cover-slip is focussed by a 2 in. objective. The larvæ being at the bottom of the dish are out of focus and invisible. The grease mark is now lightly touched by a heated needle or a glass rod which is kept to that position. After a period varying from 30 to 50 seconds, actively swimming larvæ come into the field of the focussed microscope, ultimately reaching the under surface of the cover-slip, crowding round the heated mark. As the cover-slip floats freely on the surface of the fluid there is no possibility of the larvæ having travelled along the side of the vessel. As a matter of fact, by further focussing they can be seen swimming vertically upwards from the bottom of the dish. This experiment demonstrates conclusively the ability of the ankylostome larvæ to swim freely and maintain themselves suspended in water.

In both experiments it is important to discount the possible action of convection currents created by the hot point. The character of the currents was studied by the addition of precipitated fuchsin to the water. Later ankylostome larvæ were also added. This method served well to show the contrast between the behaviour of the living organisms and the suspended particles when exposed to the same action of convection current. It was found that the fine pigment granules are carried to and past the heated spot by the current; no granules

remain even momentarily near the heated point. The ankylostome larvæ however, after travelling to the heated point, remain there in spite of the current.

The convection currents move in circles travelling to and from the heated point. Active ankylostome larvæ are frequently seen travelling against the current to the hot point. Occasionally larvæ become entangled and hampered by flocculi of the pigment, but once freed they continue their journey, frequently with success.

The convection currents are not sufficiently strong in these experiments to carry dead ankylostome larvæ, which are often seen lying motionless on the bottom of the watch-glass unaffected either by heat or by convection currents.

If the fine point of the heated needle is applied to the watch-glass, the convection currents are so weak that there is no visible movement in the pigment granules, yet this is proficient in providing the necessary stimulus to the ankylostome larvæ.

It is probable that the ankylostome larvæ far from the source of heat become stimulated by the rise of temperature through the hot convection currents, and they travel *against* the direction of the current to reach the hottest place. It is evident that this reaction to heat may be the directive influence which stimulates the hookworm larvæ to pierce the human skin, passing from the cooler temperature outside to the warmer inside. This is a new and striking example of that definite and invariable reaction brought out by exposing certain organisms to different temperatures, to which Loeb has applied the term "thermotropism."

Apart from their academical interest, these tests will help to differentiate ankylostome larvæ from the ubiquitous free-living nematode larvæ when examining natural sources of infection such as soil, stream water, mud, &c. So far the apparent morphological identity of the hookworm larvæ with those of certain free forms have frustrated the attempts to locate exactly the principal sources of infection of ankylostomes in nature. By help of the heat test it will be possible to recognize and possibly isolate the larvæ of these parasitic nematodes from free-living ones.

Further, the attraction of ankylostome and other larvæ to heat is apparently a characteristic shared by all larvæ that are skin penetrators, as the list mentioned above shows. The number of different species examined justifies this conclusion, although further test of other larvæ will be of great value. If such additional test holds good it will be a simple matter to decide whether any particular nematode larva is a skin penetrator or not. This will profitably replace the animal experiments now necessary, especially from the point of view that many parasites of man and animals will not infect laboratory animals.

The above studies arose out of an inquiry into the incidence of hookworm in Cornwall under the auspices of the Medical Research Council, and I am indebted to Professor Leiper for the recognition that the reactions observed by me belong to those classified as Thermotropisms, and for suggestions upon which some of the subsequent experiments were based.

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A Simple Method of Experimentation for Skin Infection with Hook-worm Larvæ.

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INTRODUCTION.

IN all, or practically all, previous work done on skin infection with ensheathed larvæ of *Ancylostoma duodenale*, *Necator americanus*, *Strongyloides stercoralis*, *Stephanurus dentatus*, and some other species of nematodes, use has been made of a live animal, either the natural host or some suitable laboratory animal.

Bearing on the reactions and the mechanics of the processes involved, there are the experiments of Herman (1905) with elder pith, into which he found larvæ of *Ancylostoma duodenale* could penetrate leaving their sheath behind them, and the same investigator's work on exsheathment under the influence of certain aniline stains. Lambinet (1902) showed that the larvæ would leave their sheaths when immersed in gelatine solutions of suitable strength, and Looss (1911) confirmed this work and also that of Herman on the effect of stains.

In some work I have recently been doing in the ensheathed larvæ of *Necator americanus* and two nematode parasites from the alimentary canal of the wild rabbit, I was seeking for a suitable method of experimentation which would simulate the condition of the live animal and at the same time do away with its use, and furthermore afford one a means of following the process of skin penetration under the microscope. I finally arrived at the simple technique described in the following note.

The work was done quite independently of Dr. Khalil's experiments described in the preceding paper, in fact I was not aware of his observations until his return from British Guiana early in the autumn. It is therefore additionally interesting to note how intimately this work links up with his.

METHOD.

A young rat or mouse a few days old is taken and killed by chloroforming. The skin from the abdomen and flanks is immediately removed by dissection and is pinned on to a small sheet of cork about $\frac{3}{16}$ in. in thickness, with the epidermis uppermost. In the centre of the cork is a hole about $\frac{1}{2}$ in. in diameter. The cork raft with the attached skin is then floated on the surface of N. saline at 37° C. in a suitable containing vessel. I made use of a cylindrical glass jar about 3 in. high by 2½ in. in diameter. Care is taken to ensure that the saline comes into actual contact with the under surface of the skin. This can be done by allowing the bubble of air to escape from between the cork and the skin.

A drop of water containing the ensheathed larvæ to be experimented upon is placed on the upper surface of the skin. The jar is placed on the stage of a binocular dissecting microscope and by suitable illumination the larvæ can easily be seen within the drop of water. By keeping the jar in the incubator at 37° C. the saline can be kept at body temperature, whilst frequent examina-

tion of the preparation can be made by removal of the jar from the incubator to the microscope stage.

Ensheathed larvæ of *Necator americanus* when treated in this way were found to be very actively motile at 37° C. They could be seen, under the microscope, to be wriggling downwards with their anterior ends pressing against the skin, as though trying to penetrate it. By leaving the stopper out of the jar the drop of water containing the larvæ was allowed to evaporate, and when the depth of liquid became sufficiently shallow the larvæ managed to break out from their sheaths and penetrate the skin.

There is no doubt that the larvæ are stimulated to great activity by the warmth from the saline under the skin; they are in fact much more active at 37° C. than at ordinary laboratory temperatures. In watching them under the microscope one has the distinct impression that they are striving to make their way into the skin. Moreover, the young skin used is so thin as to be practically transparent, and thus allows the transmission of light through it from the mirror of the microscope.

In one experiment of this nature which I performed, I obtained an excellent penetration of *Necator americanus* larvæ into the skin and after fixing the preparation in hot 70 per cent. alcohol I split the skin into two layers. The epidermis and immediately adjacent layer separated quite easily from the dermis and was found to contain a large number of the exsheathed larvæ, whilst on the actual surface of the skin quite a number of empty sheaths were found. The vast majority of the sheaths had been removed before fixation in a drop of distilled water which had been put on the skin and then sucked up again in a pipette.

The epidermis containing the larvæ was cleared in lactophenol and then mounted in this medium on a microscope slide. It forms a very interesting demonstration mount showing the sheaths on the surface of the skin, and the larvæ just inside.

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Section of Tropical Diseases and Parasitology.

President — Sir LEONARD ROGERS, C.I.E., M.D., F.R.S., I.M.S.

The "Blue Bodies" in Leishmaniasis.

By J. B. CHRISTOPHERSON, C.B.E., M.D., F.R.C.P.

THE subject I am bringing forward to-night concerns the fate of the Leishman-Donovan organisms in the human body; it concerns also certain bodies which we have called "blue bodies," and which we have constantly found in all the varieties of leishmaniasis we have met with in the Sudan.

What are these "blue bodies"? Whatever they may be, they appear to be so constant in leishmaniasis as to assist diagnosis in cases where the parasites are scanty.

In 1911, or thereabouts, whilst working at the Khartoum Civil Hospital on cases of suspected oriental sore (which was then beginning to appear, not infrequently amongst the out-patients), we constantly had difficulty in finding the Leishman-Donovan body in the preparations which we made for diagnosis.

In many cases they were very scanty, and patient search was needed to find them, and we came to recognize the presence of certain roundish, blue-staining (extra-cellular) bodies which appeared to us to herald the finding of the Leishman-Donovan bodies themselves. We found the "blue bodies" so often, and they were so frequently the harbingers of the Leishman-Donovan body, that, when we came across them, we considered it worth while to continue longer with the search, the result being almost invariably the finding of the Leishman-Donovan body; and it occurred to us that they might be, or some at least might be, a phase in the development or a step in the degeneration of that organism.

DESCRIPTION OF THE "BLUE BODIES."

If the smear of a sample taken by a capillary tube from the base of an oriental sore is stained with Leishman's stain and examined with a one-twelfth oil immersion, there are found free in the tissue fluid outside the cells, round bodies, varying in number, and varying in size from a third of a red blood corpuscle to about two-thirds of a red cell. Their outline is not very distinct: they take on a uniform or slightly stippled blue colour—they are structureless, they sometimes contain a pink speck, perhaps two, or a thickening at the periphery, suggesting that they might be Leishman-Donovan bodies, which have undergone disintegration and have become structurally undifferentiated rounded bodies. Though some "blue bodies" are larger than others, the variation in size is not in itself evidence against the theory that they are Leishman-Donovan bodies, for the degeneration may be more advanced in some than in others. Yet I think there is no doubt that most of these "blue bodies" are due to the fragmentation of the cytoplasm of the tissue cells—possibly caused by the destruction of the cell by the Leishman-Donovan body,

though there is evidence that some "blue bodies" are the degenerated cystic remnants of the Leishman-Donovan body.

In leishmaniasis the tissue cell is the normal habitat of the Leishman-Donovan body. It appears to thrive and to multiply in the tissue cell, no doubt at the expense of the cell which in the end it destroys, and so is liberated into the tissue fluids or blood. When liberated, the Leishman-Donovan body may find its way either into another tissue cell and continue the process of destruction, or occasionally into a large mono- or a polymorph.

But what is its fate if it remains free in the blood or tissue fluids, as many, perhaps a great many, must? It seems then to disappear, for a Leishman-Donovan body is very rarely found free in the peripheral blood; perhaps it would be more correct to say that the Leishman-Donovan body is found in the blood only after careful search. In fact, the blood appears to be a hostile fluid for the Leishman-Donovan body. If the smear taken from the base of an oriental sore is examined, it will be seen that the Leishman-Donovan bodies which are free in the tissue fluid vary in size, in shape, and in staining reactions. There appear to be steps in a degenerative change. Some are small with distinct tropho-nucleus and rod-body, taking on very little of the blue stain; others are rounder and larger, with tropho-nucleus and rod-body less distinct, the nuclei not staining so well; the cytoplasm on the other hand having a tendency to become vacuolated and to stain a deeper blue.

We find also Leishman-Donovan bodies in a spherical condition where the foregoing points are still more marked, and next—I admit that I am on hypothetical ground now, but I think there is *some* evidence for the hypothesis—next comes the final stage, a round structureless body staining blue, on the periphery of which there is sometimes a thickening or a dot, which suggests to me a fragment of the rod-nucleus, perhaps the least lysible, or last lysed, part of the Leishman-Donovan body.

The bodies I describe have a diameter about equal to the length of a Leishman-Donovan body, and are about one-quarter the size of a red cell—but they vary considerably in size.

One might possibly go a step further, in the theory that the "blue body," or some of the "blue bodies," are degenerated Leishman-Donovan bodies, and ask the question—what becomes of the degenerated Leishman-Donovan body? The appearance of cystic swelling of the body, which I have described, is suggestive of some osmotic change, and it would be easy to believe that it ultimately ruptures, and that the remnant of the Leishman-Donovan body is finally digested by the tissue fluids.

COMMENTS.

We found "blue bodies" in all the forms of leishmaniasis in the Sudan—oriental sore, espundia and kala-azar—we also found them in all the oriental sores we have seen in England.

I am aware that bodies which stain blue, and are similar to some of the "blue bodies" of leishmaniasis, occur in conditions other than leishmaniasis. Professor Dudgeon has told me that he believes that he has seen them in the spleen under ordinary conditions, and my colleague, Dr. Broughton-Alcock, has shown me a Leishman-stained film of liver pus containing bodies which stain blue and are similar to some of the bodies seen in leishmaniasis—these are probably rounded portions of the cytoplasm of tissue cells broken off, and many of the "blue bodies" seen in leishmaniasis are produced in the fragmentation of tissue cells; but my view is that some are degenerated Leishman-Donovan bodies.

THE EFFECT OF ALTERATION IN OSMOTIC PRESSURE ON THE LEISHMAN-DONOVAN BODY.

There is a striking experiment, demonstrating the tendency of the Leishman-Donovan organism, to swell up and become round and disintegrate in the presence of added fluid, and which seems to support, to some extent, what I have said regarding the fate of the Leishman-Donovan body when free in the blood stream or tissue fluid.

If a slide containing a drop from the condensation fluid of a N.N.N. culture of the Leishman-Donovan body from a case of kala-azar be examined microscopically under a cover-glass, flagellates are seen in various stages of development—active and motile. If a drop of water be run under the cover-glass, most of the flagellates lose their flagella and become spherical.

Further, if the slide on which the experiment is done be allowed to dry and then be stained by Leishman's stain, certain round blue bodies—suggestive of the "blue bodies"—are seen. Some have tropho-nuclei, which are diffuse and take the stain badly, the rod-nucleus being a mere speck, and others have no nuclei, except a speck representing the rod.

The property of the Leishman-Donovan body, when free in the tissue fluids, to swell up—and disappear, as it appears to do, owing possibly to an alteration in osmotic pressure around it—is shown practically when a splenic puncture is made with a wet syringe for diagnosis in kala-azar, no normal Leishman-Donovan bodies being found. A susceptibility of the Leishman-Donovan body to alterations in osmotic pressure would explain the difficulty in the case of kala-azar of finding the Leishman-Donovan body in the peripheral blood.

We can picture the tissue cells of the spleen and the medulla of bones in a case of kala-azar, crowded with Leishman-Donovan bodies constantly multiplying and destroying the individual cells. These Leishman-Donovan bodies would, on the inevitable destruction of the invaded tissue cells of the spleen, be found in large quantities in the peripheral blood, were it not for the fact that they are disposed of before they reach the peripheral blood, or when they reach it—for both spleen and bone marrow are nothing more or less than big blood swamps communicating freely and directly with the general circulation by arteries and veins, and there is nothing to bar the way into the general circulation. They are possibly destroyed in the blood by an osmotic change: whether they become "blue bodies" before they are digested, I do not know.

Alteration of osmotic pressure, due to chemical changes taking place in the tissues at death, would also explain the difficulty of finding the Leishman-Donovan body in the tissues after death in kala-azar, when the autopsy is not made soon after death; the tendency of the Leishman-Donovan body to swell up and disappear in adverse conditions is remarkable.

"Coccal" Bodies.—I will refer briefly to appearances in leishmaniasis, which observers¹ have at various times described; the so-called "coccal" bodies. These bodies are intracellular, whereas the "blue body" is never intracellular. McDonagh² has described bodies in oriental sores, which he names *Leucocytozoon leishmania*. "Blue bodies" are not the so-called coccal bodies or the *Leucocytozoon leishmania*.

¹ Balfour and Thompson, *Trans. Soc. Trop. Med.*, January, 1910. Seidelein, *Annals of Trop. Med. and Parasitology*, July 31, 1912. Carter, R. H., *Brit. Med. Journ.*, September 11, 1909. Archibald, *Journ. Roy. Army Med. Corps*, May, 1913.

² McDonagh, *Brit. Journ. of Derm. and Syph.*, May, 1921.

If the facts are as I have outlined, and if the Leishman-Donovan body in the human body is thus affected by chemio-physical conditions around it, we may find, in this direction, the reason why the Leishman-Donovan body is not oftener found in the peripheral blood; the sensibility of the Leishman-Donovan body to chemio-physical conditions around it explains why care must be taken that the syringe shall be free from moisture when making a splenic puncture for the diagnosis of a case of kala-azar, also why the Leishman-Donovan body seems to disappear from the tissues immediately after death in cases of kala-azar; it explains, in fact, the fate of the Leishman-Donovan organism in the human body.

I have brought these observations of the "blue bodies" in leishmaniasis before your notice for comment and opinion, and further, to suggest that when the finding of the typical Leishman-Donovan bodies is not easy, they may encourage further search and act as an adjunct to the clinical history for purposes of diagnosis in cases of kala-azar and in cases of suspected oriental sore.

I am not prepared to say that the "blue bodies" are degenerated Leishman-Donovan bodies, or that they are all fragments of the cytoplasm of the tissue cells destroyed by the Leishman-Donovan bodies. Some are undoubtedly, I think, fragments of cytoplasm of tissue cells, but there are bodies in leishmaniasis films which are round and blue, and which appear to me to be degenerated Leishman-Donovan bodies.

Solitary Abscess of the Liver.

By J. FOREST SMITH.

PROFESSOR DUDGEON has suggested that an account of the solitary abscesses of the liver treated in St. Thomas's Hospital during the last twenty-five years, especially with reference to cases occurring in individuals who have never been abroad, may prove of interest to members of this Section.

Professor Yorke [1], at the annual meeting of the British Medical Association, in 1919, discussed the possibility of amœbic dysentery occurring in this country in epidemic form. He stated that "certain classes at least of the population are heavily infected (that is, show cysts in the fæces), but very few indigenous cases of acute amœbic dysentery have so far been recorded." Wenyon and O'Connor [6], in 1917, stated that though isolated cases of infection have occurred in England, the disease has been rare. Marshall [2] in 1912, recorded one case of amœbic dysentery. Worster-Drought [3] and Rosewarne reported one in 1916, and Laidlaw [4] referred to three others in 1919. In 1921, Young [5] reported one case in a child.

In 1920, a child, aged 5, was admitted to St. Thomas's Hospital with a twenty-four hours' history of abdominal pain, tenesmus, and the passing of blood and mucus *per rectum*. A mass was thought to be palpable in the abdomen, and the diagnosis of intussusception was made. This was not confirmed at operation. The following day the fæces were examined and *Entamœba histolytica* were found in them. Under treatment with emetine-bismuth-iodide the child recovered. He had never been abroad. The possible source of infection was traced to an uncle, living in the house, who had recently returned from war service in the East, where he had contracted dysentery.

This series is divided into two groups. The first consists of twenty-nine cases, in which a history is given of residence in countries in which amœbic

dysentery is endemic. The second group contains three cases. The patients in these cases had never been abroad. The period covered is from 1897 to 1921. (The chief details of Group I are given in tables, shown on the epidiascope.)

GROUP I.

The majority of patients in these cases admitted previous dysentery. Three had had previous treatment for liver abscess, and three more described a previous condition of "inflammation of the liver." Unfortunately, there is no record of emetine treatment before admission. It will be seen that there are no cases in children and only one case in a woman. The age-incidence varies from 21 to 71 years, and the interval between the original attack of dysentery and admission with hepatic abscess from twenty-five years to five weeks.

Dysentery was contracted in India in nine cases, in South Africa in six cases, and in other parts of Africa in two cases. Two cases were infected in Egypt and two others in China. Bermuda, Crete, and Russia supplied one case each. In three cases men had served both in India and in Africa, and in the remaining two cases their history was not obtained. The South African War is held accountable for six cases, and two of these had had hepatic abscess already treated before admission. Of the six cases, five had only been abroad in Africa during that War, the other had served both in India and in Africa, but attributed his dysentery to service in Africa.

Up to the year 1909, there had been fifteen cases with ten deaths, whilst from 1910 to 1921 there were fourteen cases with only one fatal result. This was a case admitted in a collapsed state; he died the following day. Before operation a diagnosis of acute cholecystitis had been made. The surgical treatment of the two periods had not altered, but in twelve of the last fourteen cases emetine or E.B.I. had been given in addition.

Surgical Treatment.—Abdominal incision and drainage were carried out in thirteen cases; transpleural drainage was the method employed in six cases, and resection of rib was done for empyema in two cases. In three cases, both drainage of empyema and abdominal incision were undertaken. One patient died before operation.

Two cases are described as "Resolution by Expectoration." In one of these cases a previous operation had been performed in South Africa for liver abscess. On admission, cough was distressing and the sputum copious. On examination the sputum contained necrotic tissue, in which liver cells were identified. No amœbæ were seen. In the other case, that of a sailor, who denied dysentery, but who had had ulcerative colitis treated in England five years before, the patient coughed up pus, described as "like anchovy sauce." The liver was enlarged, and there were signs of consolidation at the right base. With emetine injections the symptoms rapidly improved. In one fatal case, in which at post-mortem examination a track from an abscess in the right lobe to the right bronchus was shown, amœbæ had been found in the sputum.

The treatment of one case is described as "Exploration." This was that of a soldier, who was admitted with a history of very severe pain in the right lumbar region, made worse on exertion and followed by frequency of micturition. There was some albuminuria and the right loin was tender. The liver was not palpable. Nephrotomy was performed and the kidney found normal. Sixteen days later he died suddenly from pulmonary embolism, and the cause of the pain was discovered, post mortem, to be due to a solitary abscess of the liver. This illustrates the possibility of mistaken diagnosis where the condition is not

suspected, and it is of interest to learn the provisional diagnoses made before the cases were sent into hospital. One case was sent as a cirrhosis, whilst empyema, pleural effusion, and unresolved pneumonia were common errors. The case of the man admitted as an "acute abdomen" has already been mentioned, and two cases came in with a history of repeated negative examination of the sputum for tubercle bacilli.

The right lobe of the liver was the seat of the abscess in twenty-three, and probably in twenty-six cases, the left lobe being involved in three cases only.

Post-mortem Examination.—Of the fatal cases, eleven in all, post-mortem records are available in ten. Ulceration of the colon, either recent or remote, was present in nine cases. The degree of ulceration varied from gross changes to a few discrete ulcers in the cæcum. In one case it is stated that there was no evidence of ulceration in the gut. In three cases the abscess had perforated the diaphragm into the pleura, and in one case into the right bronchus. In another case the abscess had opened into the ascending colon and into the duodenum.

GROUP II.

The second group, occurring in people who have always resided in England, comprises three cases. One of these was a true amœbic abscess, and the probable source of infection can be given. The other two cases are those of solitary abscesses occurring in the liver, the ætiology of which I leave for your consideration.

Case A.—A female, aged 25, admitted in 1920 with a history of ten weeks' pain under the right costal margin, and loss of weight. On examination a large fluctuating mass was palpable in the right lobe of the liver. Fever of a hectic type was present. At operation the abscess was opened and drained. It was solitary and situated in the right lobe. The pus is described as "chocolate" in colour, and was sterile on culture. The patient's husband was an ex-soldier who had had dysentery during the war. The patient had always been resident in England.

The following two cases are submitted for consideration:—

Case B.—A schoolboy, aged 13, who had never been abroad, was admitted to hospital in 1915. He gave a five days' history of pain in the upper abdomen. The only past history was that of an attack of pleurisy fourteen months previously. On examination there was seen to be no fever. A small mass connected with the liver was palpable. At operation a small quantity of yellow odourless pus was evacuated from a solitary abscess in the left lobe. The pus on examination consisted of débris. It was sterile on culture.

Case C.—An hotel porter, aged 26, was admitted in 1899, when he stated that he had never been abroad and had had no previous illness. He gave a three weeks' history of chills, cough, and pain in the upper abdomen. On examination there were signs of a right-sided pleural effusion, and the liver was enlarged 2 in. below the costal margin. Fever of the hectic type was present. Operation consisted of drainage of an empyema: death occurred four weeks afterwards. At the post-mortem examination a solitary abscess with ragged collapsed walls was present in the right lobe. There was no obvious communication between this abscess and the right pleura, which contained some 30 oz. of pus. No reference was made to the condition of the colon.

REFERENCES.

- [1] YORKE, *Brit. Med. Journ.*, 1919, i, p. 451. [2] MARSHALL, *Edin. Med. Journ.*, 1912, p. 229. [3] WORSTER-DROUGHT and ROSEWARNE, *Brit. Med. Journ.*, 1916, i, p. 715. [4] LAIDLAW, *Guy's Hosp. Repts.*, 1919, lxix, p. 149. [5] YOUNG, *Lancet*, 1921, i, p. 122. [6] WENYON and O'CONNOR, "Human Intestinal Protozoa in the Near East," 1917.

The Morphology of the Cercaria of *Schistosomum mansoni* from *Planorbis boissyi* of Egypt.

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(From the Department of Helminthology, London School of Tropical Medicine, in conjunction with Dr. Leiper's Mission to Egypt, December, 1921.)

THE cercaria of *Schistosomum mansoni* was first identified by Leiper during 1915, in *Planorbis boissyi* collected from the neighbourhood of Cairo, Egypt. He recorded the main characteristics of *Schistosomum* cercariæ: (a) The absence of a muscular pharynx; (b) the absence of eye spots; (c) the presence of a forked tail. These characteristics are shared by all species of *Schistosomum* cercariæ known, and they are easily verified in any particular cercaria, without the need for elaborate staining or very high powers of the microscope. He maintained that the identification of any particular species in the first instance is only practically possible by infecting experimental animals and recovering the adults.

Some recent writers such as Faust, Cort and Sewell, lay stress on specific differences in the cercariæ, and Faust has gone so far as to record *Schistosomum mansoni* cercaria from *Physopsis africana* in apparently preserved material sent from South Africa. The cercaria is so small, and its tissues are so delicate, that ordinary methods of preservation and staining are useless in its examination, as they destroy many important details. With the exception of those characters mentioned by Leiper, there is no general agreement with regard to the structure of the cercariæ. Even such competent observers as Cort do not hesitate to revise their observations from time to time.

The anatomy of the cercaria is important in the elucidation of the biological properties and functions of that stage in the development of *Schistosoma*. Specific characteristics are certain to be found peculiar to each species. The material upon which such characteristics are to be based must be specifically diagnosed beforehand by animal experiments.

Material Studied.—The material upon the study of which this paper is based was a large number of infected *Planorbis boissyi*, sent from Egypt by Professor Leiper. They were collected from the same site where *Schistosoma mansoni* cercariæ were first discovered. These snails were brought to London alive. They were put in a cardboard box with layers of filter paper between. This was moistened with water every day during the voyage.

On a previous occasion both *Planorbis boissyi* and *Bulinus contortus* were sent from Egypt by post in a wooden box containing moist cotton-wool between the snails. The snails were kept separated from each other by the cotton. The snails arrived after nine days' transit, and were found to be alive and active when put in water. I am indebted to Dr. M. Erfan, of Kasr-el-Aini Hospital, Cairo, for collecting the snails and forwarding them successfully.

Technique.—For the purpose of collecting cercariæ, each snail was placed in a glass tube, 1 in. by 3 in., half full of tap water, and left overnight. Each tube was then examined with the naked eye or hand lens for cercariæ. These are detected by their wriggling movements. More cercariæ are discharged by the snails when the tubes are kept in an incubator at 22° C. The infected water is poured into a shallow glass capsule that can be examined conveniently under a 2 in. power of the microscope. Cercariæ are picked with a glass pipette and placed on a slide. A cover-slip is gently placed and the edge fixed with

melting wax to facilitate manipulation.) After ten to fifteen minutes the activity of the cercariæ is inhibited sufficiently to allow the use of one-twelfth oil immersion lens for examination. This is probably due to absence of oxygen. *Intra vitam* stains have been tried, using neutral red, methyl green, methylene blue and brilliant green. Methyl green is particularly useful for indicating the termination of the ducts of the peri-acetabular glands. All the details can be easily made out in unstained specimens. Stained paraffin sections of infected *Planorbis boissyi* liver were also available for study. These helped to confirm the observations made on the living cercariæ.

General Appearance of the Cercaria.—The cercaria has a body and a tail. The body contains the organs of the adult in addition to some larval structures, necessitated by the particular mode of life to which it is adapted during that stage. The tail is a purely larval structure, being shed off during the process of penetrating the skin of the final host. The body is cylindrical in shape being a little broader posteriorly in the region of the ventral sucker than anteriorly. The length and breadth vary enormously during life, according to the state of contraction or extension of the cercaria. The normal attitude is to be regarded as that assumed by the cercaria when killed by gentle heat. Within narrow limits this attitude is constant as to size and shape. The tail is longer than the body. Besides being contractile it can be moved freely in all directions. The end of the tail is bifid. The furci are less than one-third the length of the tail. They are motile and can be approximated or separated widely. The body and tail, including the furci, are covered with stout spines directed posteriorly. The only region of the surface free from spines is the space between the openings of the ducts of the peri-acetabular glands.

Measurements.—There is great discrepancy in the records of the measurements of cercariæ given by various observers. This is apparently due to different methods of killing the cercariæ. Alcohol and formalin cause great shrinking. Schaudinn's solution produces relaxation. Heat kills the cercariæ in an apparently more natural condition. Some observers, Manson-Bahr and Fairley especially, measured cercariæ from dissected snails. A high percentage of the cercariæ procured by this method are not fully mature, though alive and active. Other observers omit to mention the conditions under which measurements are taken. The measurements cited here are the average of those of thirty cercariæ voided naturally by different snails and killed by gentle heat:—

	Microns	Iturbe Microns	Faust Microns	Bahr and Fairley Microns
Length of body ...	189 (185-215)	100-130	140	161
Breadth of body ...	73 (65-80)	40-50	60	60
Ventral sucker ...	33 (30-38)	—	—	44
Oral sucker ...	60 (56-66)	—	—	53
Stem of tail ...	250 (215-270)	140-150	200	213
Breadth of tail ...	40 (33-43)	20-25	27	35
Furci ...	75 (69-83)	40-50	50	66

Cuticle.—The cuticle is thin, covered with spines. Directly beneath the cuticle there are two layers of muscle fibres. The external layer is transverse and the inner longitudinal. The cuticle easily ruptures if any slight pressure is applied to the cercariæ. The particular sites of rupture are the oral sucker and at the attachment of the tail. There is a distinct constriction surrounding the anterior part of the body. From its situation it appears to mark the site of attachment of the muscle of the oral sucker to the cuticle.

The Oral Sucker.—The oral sucker is a very large structure in comparison with the size of the body. It occupies practically the anterior third of the body. The circular muscle of that sucker forms its posterior border, and in

optical section is cone shaped, the apex directed posteriorly. There are in addition some longitudinal strands of muscle fibres capable of withdrawing or protruding the lips of the mouth opening. The central part of the sucker is occupied by the oral glands, to be described later. In the middle line runs the œsophagus. On either side is placed the mass composed of the five ducts of the peri-acetabular glands. These glands pierce the conical-shaped muscle of the sucker on either side of the middle line, producing a little indentation in its outline. The ducts are much constricted in this situation, this being due to the action of the muscle. The mouth opening is small oval in shape, being longer along the longitudinal axis of the body than transversely. It is placed more on the anterior surface of the body, the œsophagus opening at its lower pole. On either lip of the mouth are placed the spinous ends of the peri-acetabular gland ducts. These spines were thought by Manson-Bahr and Fairley to compose the armature of the head, and to have no connexion with the gland ducts. They also considered the muscle of the oral sucker with its conical shape to represent pharyngeal pouches, a character which is possessed by quite different groups of trematodes.

The Ventral Sucker.—The ventral sucker is much smaller than the oral sucker. It is placed nearer the posterior end of the body. Its cuticle is covered with spines pointing towards its periphery. Its characteristic feature is its ability for free protrusion above the surface of the body, forming a nipple-like structure. This sucker is much stronger than the oral, and can fix the cercaria securely when the water is disturbed.

The Digestive System.—The œsophagus is a simple tubular structure. There is no muscular pharynx. The mouth opening is a little dilated. It opens on the anterior surface a little below the cephalic extremity. When the oral sucker contracts the mouth becomes nearly terminal. At the posterior end of the œsophagus there is a dilated intestinal cœcum, with an indication of commencing bifurcation. Occasionally granules of debris can be seen in the lumen of the cœcum. The œsophagus is practically half the length of the body.

The Excretory System.—The excretory system consists of flame cells, each of which is joined by an "excretory tubule." Two excretory tubules join, forming a "collecting tubule." The latter join to form the "main excretory duct," which opens into an "excretory vesicle." In addition there is an "excretory canal" in the tail.

The flame cells are small conical structures, visible during life on account of their constantly vibrating cilia producing a current of fluid. Their number is constant in any particular species. There is probably the same number of flame cells in the cercariæ of the different species of the same genus. The number of flame cells in the *Schistosomum mansoni* cercaria has not been agreed upon by any two observers, so far. Iturbe records four pairs of flame cells in the body and one pair in the tail. Faust in his description of *Schistosoma mansoni* cercariæ does not record the number of flame cells. Manson-Bahr and Fairley observed five pairs of flame cells in the body and one pair in the tail, both in the cercariæ of *Schistosomum mansoni* and *Schistosomum hæmatobium*. The number of flame cells in the cercariæ of *Schistosomum mansoni* is constantly three pairs in the body and one pair in the tail. I never found any variations in all the specimens examined. The cell wall of the flame cell has a double outline. It has a higher refractive power to light than the surrounding structures. The position of the flame cells in the body varies slightly, but the general arrangement is constant. They are placed near the cuticle, to the outer side of the internal organs. The most anterior flame cell

is placed midway between the oral and ventral suckers. The middle cell is at the level of or a little anterior to the ventral sucker. The posterior cell is more variable in position, but is confined to the posterior quarter of the body. The tail cells are placed very near the body, and their opening is commonly directed posteriorly. One cell is often, though not constantly, a little lower than the other.

An excretory tubule begins from the apex of each flame cell. The tubule is very fine and more or less coiled, according to the state of contraction or relaxation of the cercaria. They are made visible by their refractability, especially when the cercaria is dying, and they get a little distended with secretion. The ducts from the two anterior cells join to form a "collecting tubule"; the duct of the posterior flame cell joins that of the tail cell of the same side to form another "collecting tubule." The collecting tubules are a little wider than the excretory tubules. The two collecting tubules join to form the "main excretory duct" of the corresponding side. This wide tube curves upwards before proceeding posteriorly to join the duct of the opposite side to form the "excretory vesicle." At the beginning and at the end of the "main excretory duct" there are cilia in constant action producing a noticeable current in the lumen. Their situation is marked by (x) in the accompanying diagram (fig. 1). The appearance of this current probably led Iturbe and Manson-Bahr and Fairley to the wrong conclusion of mistaking this phenomenon for additional flame cells. Careful observation, however, fails to reveal the presence of any flame cells in these positions.

The "excretory vesicle" is placed at the posterior end of the body in the middle line. Posteriorly it joins the excretory canal of the tail. The excretory canal of the tail is a straight canal passing along the centre of the tail to above the furci of the forked tail, where it divides into two ducts, one in each furcus. Each ends in a clear rounded dilatation with an "excretory pore." Near the beginning of the excretory canal of the tail there is an appearance of a little island in the canal, first described by Cort. It was not verified in all the specimens examined.

The Glandular Structures in the Cercaria.—Glands are present around the ventral sucker and in the oral sucker. Those around the ventral sucker have been named differently by various observers. There is no agreement as to the adoption of any one term. Amongst the names proposed are the following: "Cephalic glands," referring to the openings of the ducts at the cephalic end; "salivary glands," "mucin glands," or "poison glands," referring to supposed physiological secretion, according to the interpretation of different observers; "stylet gland" refers to the armature of the duct openings. I think the name "peri-acetabular glands" proposed by Milton is both simple and expressive of the true anatomical position. The glands in the oral sucker were named "head glands," but as this term is easily confused with "cephalic glands," used occasionally for the "peri-acetabular gland," it is proposed to name them "oral glands."

The Peri-acetabular Glands.—These glands overlap each other. Their outline is difficult to make out in many specimens, though occasionally in some cercariae it is quite clear. Probably this is due to the accumulation of secretion in the glands. The same is true of the presence and definition of nuclei. The number of these glands is taken by Faust as a basis of differentiation of the different species of schistosomes. He maintains that their number is constantly three pairs in *Schistosomum hematobium* cercaria, five pairs in *Schistosomum japonicum* cercaria, six pairs in *Schistosomum mansoni* cercaria.

In both *Schistosomum spindalis* and *Schistosomum indicum* cercariæ the number of glands is always five pairs. There is no reliable account of the anatomy of *Schistosomum hæmatobium* cercariæ available. Manson-Bahr and Fairley agree with Faust as to the number of glands in *Schistosomum hæmatobium* and *Schistosomum mansoni* cercariæ, but judging from their statements as to other structures of the cercaria their observations are unreliable. Iturbe records the presence of three pairs of peri-acetabular glands in *Schistosomum mansoni* cercaria.

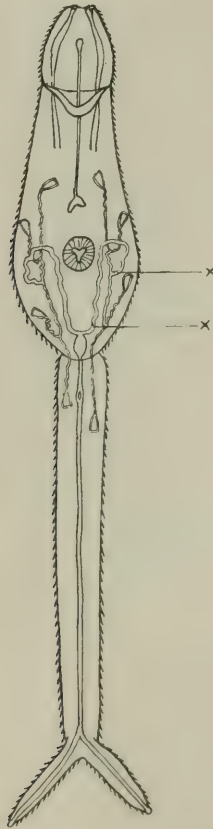


FIG. 1.—Diagrammatic representation of the excretory system of *Schistosomum mansoni* cercaria.

This research has revealed the presence of five pairs of glands only, and the evidence of this finding has been corroborated by the presence of five pairs of ducts and five pairs of spines. These glands are anatomically divisible into two groups: (1) An anterior group composed of one pair of cells on either side. These are large glands with bulbous posterior extremity and tapering anteriorly to fine ducts. They are situated just in front of the ventral sucker. Their protoplasm is very granular. The granules are large, and this gives the glands an appearance which differs from that of the posterior group. Probably their

function also differs. The granular appearance can be seen throughout the length of their ducts. In stained sections these granules disappear, leaving a clear protoplasm. Apparently they are soluble in the reagents used in the process. The nucleus is large, vesicular, and situated in the bulbous portion; there is a large nucleolus. (2) The posterior group of peri-acetabular glands consists of three pairs. They are much smaller glands, situated principally posterior to the ventral sucker. The two anterior glands are situated side by side, and the third posterior to both. Their protoplasm is finely granular. The nuclei are vesicular with large nucleoli.

The ducts from both groups of glands combine to form one bundle. They are compressed to the middle line near the centre of the body by the large cellular structure of the nervous system. The ducts in that situation are practically in contact with the ventral surface. Anterior to this both sets of ducts diverge from the middle line, and they penetrate the corresponding side of the muscle of the oral sucker. They are very much constricted in that situation. In the oral sucker they approach the middle line again, but not to the same extent. They then diverge to open a little distance from each other on either lip of the oral sucker. The two ducts of the anterior group of glands are always situate to the inner side. The five papillæ surmounting the openings are arranged in a semicircular fashion facing that of the opposite side. The ducts of the oral glands open into the concavity.

Faust states that these papillæ are perforated. I fail to detect any opening in these fine papillæ. Upon direct inspection of the mouth the circular openings of the ducts can be definitely seen. Each papilla is a prolonged outer lip of the corresponding duct. Iturbe records the presence of three or four papillæ on either side; Faust, that of six papillæ on either side. Manson-Bahr and Fairley record sixteen papillæ, having no connexion with the gland ducts. The gland ducts are said to open into a hypothetical pharyngeal pouch. Lutz alludes to the presence of six or more papillæ.

The Oral Glands.—The presence of oral glands was first recorded by the Japanese observers. Cort describes one oral gland in *Schistosomum japonicum* larvæ. No allusion was made as regards their presence in the other human schistosoma cercariæ. Both in stained sections and in the living cercariæ, a mass of nuclei occupy the space between the two sets of ducts in the oral sucker. Distinct outline of separate glands cannot be made out. There are, however, four large nuclei near the posterior end of the sucker. There are also four openings of ducts, one pair on either side in the concavity of the duct papillæ. For these reasons it is assumed that there are four oral glands. Their duct openings are apparently unarmed with papillæ.

The Nervous System.—There is a large mass of cells surrounding the œsophagus and pressing the gland ducts to the middle line. From its relation to the œsophagus this mass of cells is assumed to be the nervous system. The nuclei of the cells composing this mass only appear by means of staining or in sections. They stain violet when methyl green is used. The only other cells staining in the same manner are the genital cells.

The Genital Cells.—These are represented by a mass of cells lying posterior to the ventral sucker. It is not visible in the unstained cercariæ. The nuclei are very small, stain deeply, and are crowded together. The number of nuclei exceeds twenty.

The Ground Tissue.—The organs described above are crowded together. The very small interstices are filled with ground tissue consisting of large cells with large feebly staining nuclei. These nuclei are situate at considerable distance from each other.

The Tail.—The cuticle of the tail resembles that of the body as well as its spines and musculature. The cross-section of the tail is round. Its attachment to the body is very fragile, and it is easily detached. Its posterior is slightly narrower than its anterior end. The stem of the tail is longer than the body. The furci of the bifurcation are less than one-third the tail length. They can be moved in all directions, approximated or lying at right angles to the body. In the latter position their flattened surfaces lie anteriorly and posteriorly. In mounted specimens the furci lie flat on the surface of the slide, and thus are twisted. This gives them the false appearance of being constricted at their junction with the stem of the tail. When the tail is separated from the body, its movements continue for a short time—not as long as in the case of *Fasciola hepatica* cercariæ. The movements of the flame cells cease much sooner than the muscular contraction.

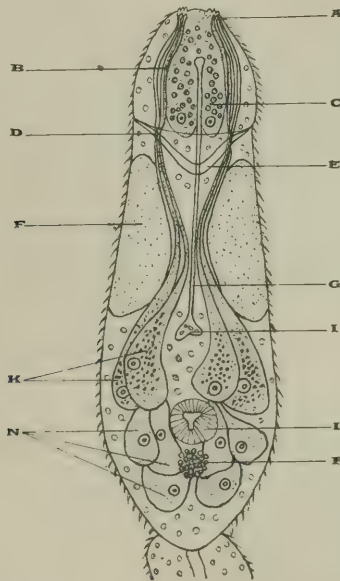


FIG. 2.—Camera-lucida drawing of *Schistosomum hæmatobium* cercaria. ($\times 770$ diameters.) A, spinous openings of peri-acetabular glands ducts; B, ducts of anterior group of peri-acetabular glands, with granular contents; C, oral glands; D, constricted part in the peri-acetabular glands ducts; E, muscle of oral sucker; F, nervous system; G, cesophagus; I, intestinal caeca; K, anterior group of peri-acetabular glands; L, ventral sucker; N, posterior group of peri-acetabular glands; P, cells of the genital system.

Activity of the Cercaria.—In water the cercariæ swim easily. The movement is accomplished by bending irregularly from side to side. The tail commonly precedes the body. The furci are most useful when the surface of the water is reached. They stretch at right angles to the body and tail, which hang vertically. The flat surfaces of the furci lie on the surface of the water, and in that position can support the cercaria indefinitely. When the surface of the water is disturbed the cercaria becomes active at once. It could not be discovered that it travels to the seat of disturbance, but further observation regarding this point is necessary. A fact worth mentioning is that cercariæ in

a tube at rest, although floating on the top, do not come out with the first portion of water poured. Most of them are procured in the last portion. This is apparently due to the cercariæ travelling rapidly to the bottom of the vessel when there is an attempt to pour out the water.

During flotation the action of the body, and especially of the suckers, is not much in evidence. This is quite the opposite when a solid surface is near by. The cercaria fixes itself by the ventral sucker quite efficiently. It elongates its body to nearly double its normal length, exploring the surrounding surface in different directions. The tail is in constant movement sideways. The cercaria can then fix its oral sucker to the surface while releasing its ventral sucker, contracting its body, and thus advancing appreciably. Although comparatively bigger the oral sucker is much weaker as regards the power to fix itself. Its mechanism is also different. The lips of the oral sucker can be retracted or extended to a very great extent. The openings of the ducts can be retracted to practically the centre of the sucker. During extension the openings of the ducts become the most anterior structure of the body. This action is produced by the contraction of the circular muscle pushing the contents of the sucker forwards. At the same time this results in compressing the ducts, and thus emptying their secretion, which streams out in small globules which flow together, forming large spherical globules. This modification of the oral sucker, although not efficient as a prehensile organ, is probably better suited for penetrating the definitive host.

I am indebted to Professor R. T. Leiper for the material examined and for several suggestions during its study. I have to thank Miss Le Bas, of University College, for bringing the snails alive from Egypt.

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Section of Tropical Diseases and Parasitology.

President — Sir LEONARD ROGERS, C.I.E., M.D., F.R.S., I.M.S.

DISCUSSION ON AMŒBIC DYSENTERY.

Professor LEONARD S. DUDGEON

made some brief remarks on certain points of interest concerning amœbic dysentery, and infections caused by the *Entamoeba histolytica*. He referred to certain data from the days of the Gallipoli campaign up to the end of 1918 in the East and from then onwards up to the present time in London. He considered that one of the most interesting points was the comparative infrequency of hepatic abscess in this disease, although amœbic hepatitis was of greater frequency. Sir Leonard Rogers' statistics of the number of cases of hepatic abscess and dysentery admitted to the British Hospitals in India during a period of ten years up to 1910, showed a proportion of one amœbic abscess to seven cases of dysentery. Professor Dudgeon suggested that this comparative infrequency in the war zones for 1915-19 in which he worked was probably due to the liberal use of emetine during the War. The most important type of histolytica infection from the military aspect, and now also in civil life in England, was the patient suffering from chronic diarrhœa alternating with constipation, provoked by alterations in diet, heavy work, alterations in temperature and many other causes. Such cases were continually applying for medical treatment. His opinion based on experience with the double iodide of bismuth and emetine was to the effect that the action of this drug in such cases was very disappointing. No one would deny that the drug was of quite undoubted value but opinions expressed as to its value in the early days had been exaggerated. Numerous cases of chronic diarrhœa with *Entamoeba histolytica* cysts in the fœces had received several courses of this drug which had been given by various people in the most advantageous manner but without success. It would be very important to hear the opinions of various workers that evening on their experience with this drug in such cases. The last question which he considered worthy of considerable discussion was the presence of the cystic form of the *Entamoeba histolytica* in those who had not suffered from any intestinal trouble. Figures obtained from sampling the general population had been given, which varied from about 2 per cent. upwards, but quite recently Scott, working in Jamaica, had examined 102 cases who denied any intestinal trouble, and found a total number of fifteen or 14·7 per cent. who were passing *histolytica* cysts. Scott acted on the recommendations of Wenyon and Dobell and made various examinations of each case, in fact nine in all. Professor Dudgeon pointed out that statements of hospital patients that they had never suffered from any intestinal trouble must be accepted with very great caution as one knew from experience that not infrequently such statements were erroneous when fully tested. It was

[December 5, 1921.]

well known that an attack of diarrhœa in the East which might last only a few hours, and was soon forgotten, might be due to the action of Flexner's bacillus. It was shown during the War that cases in which there was no history of any intestinal disturbance had a chronic ulceration of the large intestine due to the *Entamœba histolytica*. It was possible that the wider use of the sigmoidoscope, as recommended by Manson-Bahr and Greig and, if fully confirmed, the advantages in the diagnosis of amœbic cases of the presence of Charcot-Leyden crystals in the stools, as suggested by the work of Major Acton, I.M.S., and Dr. J. Gordon Thomson, might help to throw further light on this question.

Dr. P. H. MANSON-BAHR

dwelt on the difficulty in the treatment and diagnosis of the chronic case, whose course was more often marked by alternating diarrhœa and constipation. The presence of cysts of *Entamœba histolytica* in the stools could only be verified in about 30 per cent. of cases. In a further 40 per cent. diagnosis was supplied by the use of the sigmoidoscope, and in the remainder it was based on the course and history of the case. Mention was made of the insidious onset and long spells of latency occasionally displayed by amœbic infection. Emetine-bismuth iodide was undoubtedly of value in the early stages, and was capable of curing about 50 per cent. at that period, but when the disease became chronic it had proved a failure. He emphasized his preference for the use of emetine alone in the acute phase, followed later by emetine-bismuth iodide. Earlier administration of the double iodide might prove harmful owing to the risk of its irritating action on the acutely inflamed bowel leading to serious intestinal hæmorrhage. Recovery depended much on the patient's general resistance to infection being maintained, and was occasionally aided by rectal injections of eusol. Emetine-bismuth iodide was best given in gelatine cachets, in 3-gr. doses every night, for a course of 36 gr. Abstinence from food for two hours previous to each dose was advisable, and intolerance to the drug was occasionally avoided by giving 10 minims of tincture of opium, or, as advocated by Sir Leonard Rogers, by the addition of 5 gr. of tannic acid to the iodide. Ipecacuanha given on the lines advised by Sir Patrick Manson at times succeeded when emetine-bismuth iodide failed, but treatment of the chronic stage of amœbic dysentery still left much to be desired. Following the course of the double iodide treatment in a favourable case, careful attention in the matter of food and the gradual return to full diet were of much importance.

Sir LEONARD ROGERS (President)

recommended Deek's bismuth carbonate treatment for the relief of cases which emetine failed to cure, the number of which was relatively small in proportion to the large total of acute amœbic dysentery. Many cases in India, vaguely diagnosed as chronic diarrhœa or constipation, were found post mortem to be due to amœbic infection. He attributed the reduction in the number of cases of liver abscess in India to the use of emetine in the treatment of dysentery. Combination of tannic acid and emetine-bismuth iodide had certainly checked the unpleasant effect of the latter, and after the course of emetine-bismuth iodide he had found courses of ipecacuanha in doses of 15 to 30 gr. of value.

Surgeon Rear-Admiral Sir PERCY BASSETT-SMITH

stated that the general debility due to recurring diarrhoea and constipation in the chronic stage of the disease diminished the prospect of recovery. Emetine and emetine-bismuth iodide treatment met with success in the acute cases, but later on the results were less favourable. Emetine-bismuth iodide at times proved so unpleasant to patients that they would not take it. He had found the combined use of ipecacuanha and tannic acid very helpful in chronic cases, but under present conditions the difficulty of their obtaining suitable diet was a serious problem. He mentioned the results of the examination of stools of normal persons for *Entamoeba histolytica* cysts, and stated that Bayliss had found 2 per cent. of 400 persons examined at Haslar to be cyst carriers who had never been out of England and had no history of any intestinal disorder. Owing to improved sanitation, the risk of chronic amœbic carriers spreading infection in England was negligible.

Dr. ARTHUR POWELL

said he could confirm Professor Dudgeon's statement that past histories of dysentery or its absence were often unreliable. For instance, he had performed autopsies on six patients who died of liver abscess who said they had never had dysentery. In all six he had found either active ulceration or scars in the colon.

He could also corroborate Dr. Manson-Bahr's and the President's (Sir Leonard Rogers) statement that the addition of tannin to ipecacuanha prevented vomiting. About 1916-17 he gave some hundreds of doses of ipecacuanha along with tannin without causing either nausea or vomiting; but he had found no benefit result. It then occurred to him that tannin was an antidote to, and incompatible with, most alkaloids.

He found tannin precipitated all alkaloids from ipecacuanha and he came to the conclusion that giving ipecacuanha with tannin was practically giving *ipecacuanha sine emetina*, in other words, *ipecacuanha sine bono*.

Dilute HCl redissolves emetine hydrochloride when precipitated by tannin, but extremely weak solutions of sodium carbonate or any alkali, such as pancreatic fluid, promptly reprecipitated it.

Dr. J. C. BAKER

described a few cases of amœbic dysentery which had apparently been relieved by the use of chenopodium oil.

Sir G. S. BUCHANAN

asked to what extent amœbic dysentery occurred in Great Britain. He thought that little reliance could be placed on history of freedom from illness of persons reported as cyst carriers. He disapproved of such cases being entitled to inclusion under the term amœbic dysentery carriers.

Dr. MANSON-BAHR

did not regard chenopodium oil as of any real value in the treatment of amœbic dysentery. It was exceptional to find amœbic abscess of the liver without post-mortem evidence of some, however minute, ulceration of the bowel, which, as Professor Dudgeon had pointed out, might exist without any symptoms and be found in persons dying as the result of accident. He referred also to instances in his experience of conditions of apparent rude health, yet associated with chronic amœbic lesions of the bowel. He had little doubt that amœbic dysentery existed both in Great Britain and France.

Section of Tropical Diseases and Parasitology.

President — Sir LEONARD ROGERS, C.I.E., M.D., F.R.S., I.M.S.

Acute and Subacute Tonsillomycoses.

By ALDO CASTELLANI, C.M.G., M.D., MACKENZIE DOUGLAS, M.D.,
and T. THOMSON.

SOME years ago one of us called attention to the frequency of tonsillar affections of mycotic origin in tropical countries. During the war we noted that such conditions were comparatively frequent in various parts of the Continent and the near East. Since 1918 we have been on the look-out for these affections in this country and we have come to the conclusion that they are not at all rare. Clinically they may be classified into two groups: (1) the chronic forms; (2) the acute and subacute forms.

The chronic types are represented by the various forms of actinomycosis (nocardiasis, streptothricosis) and pseudo-actinomycosis, but these conditions, though rare, have been known for years and therefore we will not touch upon them, except to note that in the Tropics we have observed a case of tonsillomycosis with black granules due to *Nocardia nigra* (Castellani 1912); and we would also call attention to the comparative frequency of a *chronic granular nocardiasis* of the crypts, which is at times the starting-point of tonsillar calculi. The affection, which is not new, but which is little known, runs a chronic course and is not painful. The patient often does not come to consult the doctor because of sore throat, but because of the unpleasant odour of the breath. Yet on examining the throat small whitish-yellowish spots will be seen on the tonsils; these spots are in reality the surface portion of the granules contained in the crypts, and may be extracted with more or less ease. These bodies when crushed emit a very offensive odour; under the microscope they are sometimes seen to consist of masses of nocardia-like organisms, at other times of masses of leptothrix; in certain cases both nocardial fungi and leptothrix are seen, as well as various bacteria, and even protozoa such as amœbæ and flagellates. The *Nocardia* fungi present are very difficult to grow. After several years the masses in the crypts may become calcified, and real calculi may be formed, which sometimes form the starting point of some very severe inflammation.

ACUTE TONSILLOMYCOSES.

Very little attention has so far been paid to acute and subacute tonsillomycoses, though they are of practical importance as they may often simulate diphtheria. The following is an ætiological classification of these acute and subacute types:—

(1)	Due to fungi of the genus <i>Monilia</i>	Tonsillo-moniliasis
(2)	" " " " <i>Saccharomyces</i>	Tonsillo-saccharomyces
(3)	" " " " <i>Cryptococcus</i>	Tonsillo-cryptococcosis
(4)	" " " " <i>Oidium</i>	Tonsillo-oidiosis
(5)	" " " " <i>Hemispora</i>	Tonsillo-hemisporosis

Without entering into too many botanical details, it may be of practical advantage to give briefly certain characters of the above genera.

Genus Monilia, Persoon, 1797.—The original definition by Persoon is "*Stipitata aut effusa byssoidea, fila moniliformis articulata*," and Saccardo and other botanists state that these fungi are characterized by the sporophores being simple or sub-simple, and producing by constriction at their extremities a chain of large lemon-shaped conidia, often provided with a disjunction apparatus. No asci and no ascospores are present. The general tendency at the present time, however, thanks to the work of Pinoy, is to extend the term "*Monilia*" so as to include all those organisms of the family *Oösporaceæ* Saccardo 1886, the vegetative body (thallus) of which in its parasitic life (*in situ*, in the lesions) appears as a mass of mycelial threads and free budding forms, some of the mycelial filaments being long and branched, and of rather large size, and often presenting arthrospores. In the saprophytic life (cultures on the usual solid laboratory media) mostly roundish or oval budding yeast-like cells are seen, while mycelial filaments are very scarce or absent, and when present they are rather short and consist only of a few articles. *Monilia* fungi very often ferment glucose and other carbohydrates with production of gas. Therefore, from a practical point of view these fungi are characterized principally by the following features: In their parasitic life in the human lesions the vegetable body (thallus) is composed of mycelial threads of rather large size showing arthrospores and numerous free, oval, or roundish budding yeast-like elements; in cultures, especially on solid media, mostly roundish or oval budding cells are seen, while mycelial filaments are scarce or absent. Asci and ascospores are absent.

For practical purposes monilias may be conveniently classified according to some of their biochemical characteristics as follows:—

(1)	Production of gas in glucose only	<i>Monilia balcanica</i> Cast. group
(2)	Gas produced in glucose and levulose only ...	<i>Monilia krusei</i> Cast. group
(3)	Gas produced in glucose, levulose and maltose	<i>Monilia pinoy</i> Cast. group
(4)	Gas produced in glucose, levulose, maltose and galactose	<i>Monilia metalondinensis</i> Cast. group
(5)	Gas produced in glucose, levulose, maltose and galactose	<i>Monilia tropicalis</i> Cast. group
(6)	Gas produced in glucose, levulose and saccharose	<i>Monilia guillermondi</i> Cast. group
(7)	Gas produced in glucose, levulose, saccharose, galactose and inulin	<i>Monilia macedoniensis</i> Cast. group
(8)	Gas produced in dextrin in addition to other sugars	<i>Monilia pseudolondinensis</i> Cast. group
(9)	Gas produced in lactose in addition to other sugars	<i>Monilia pseudotropicalis</i> Cast. group
(10)	Absence of gas fermentation in any sugar ...	<i>Monilia zeylanica</i> Cast. group

A more detailed classification of the genus *Monilia* may be found in Castellani and Chalmers' "Manual of Tropical Medicine," p. 1079.

Saccharomyces, Meyen.—The characters of these fungi are identical with those of the genus *Monilia*, except that asci and ascophores are present in old cultures.

Genus Oidium, Link (*sensu stricto*).—This genus is morphologically closely allied to *Monilia*, but mycelial threads are much more abundant both in the lesions and in the cultures, and budding yeast-like cells are rare. Fungi of this

genus may occasionally induce an acid fermentation, but never produce gas. There is no doubt that the original thrush fungus, *Oidium albicans* Robin, 1895, is not an oidium but a monilia.

Genus Hemispora, Villemin.—The mycelium is very abundant. Some mycelial hyphæ become differentiated, forming terminal ampulliform structures called "protoconidia." The protoconidium after a time divides into several segments called "deuteroconidia," which are the true reproduction spores.

CLINICAL SYMPTOMS.

Tonsillomoniliasis.—The onset is generally abrupt with general malaise, fever, and difficulty in swallowing. On inspection of the throat, in most cases, the tonsils are seen to be covered with creamy white patches which at times extend to the soft palate, the pharynx and the larynx. Diphtheria is often suspected, but the microscopical and cultural examination of the patches clears up the diagnosis. One of us, however, has seen cases of mixed infection, diphtheria and moniliasis in the Tropics. The prognosis is generally favourable but not always. The treatment consists in local applications of glycerine of borax, and, most efficacious, a carbolic or a chlorine spray. Small doses of phenazone or aspirin may be given internally.

Illustrative Cases.

Case I.—History: On August 5, 1921, Pensioner N. suffered from tonsillitis with temperature 102° F., rapid pulse and prostration. On the tonsils and fauces there was a white, easily detached, membrane. Neither in the direct smear nor on the culture were diphtheria bacilli found. In the direct smear, made at the bedside, were a large number of monilia. From cultures on Löffler's medium and glucose-agar, *Monilia* was grown at 37° C., and isolated in pure culture. The patient made a good recovery after the exhibition of a chlorine gargle. No antitoxin was given.

Biological reactions: The monilia isolated gave the following reaction: (1) glucose, A. and G.; (2) lævulose, A. and G.; (3) maltose, A. and G.; (4) galactose, A. and G.; (5) saccharose, *nil*; (6) lactose, *nil*; (7) inulin, *nil*; (8) litmus milk, *nil*. The *Monilia* biologically corresponds to *Monilia metalondinensis* (Castellani, 1916).

Intravenous inoculation of isolated *Monilia*: An emulsion of the *Monilia* was made from a glucose-agar slope and 1 c.c. of a white opaque emulsion was injected into an ear vein of a rabbit. In five days' time the animal died and cultures of the *Monilia* were obtained from the heart blood, lungs, liver, spleen and kidney.

Naked-eye appearance of organs: In the lungs there were areas of congestion but there was no pneumonia. Both kidneys were greatly enlarged. The capsules stripped readily and showed the surface white and granular. On section the cortex was whitish. This was due to white specks, none larger than a pin's head, closely set together. There were a few discrete specks in the medulla which otherwise showed little change to the naked eye. There was no change of note to the naked eye in any other of the organs.

Microscopic appearance of kidney: Microscopic sections showed the *Monilia* scattered throughout the substance of the kidney, but mostly in the cortex. A few lay singly but for the most part they were small groups and surrounded by small round cells. The glomeruli were not greatly affected. The convoluted and other tubules were distinctly degenerated and many of them crushed owing to the presence of groups of *Monilia*. The collecting tubules were less affected, but in their lumen were many red blood corpuscles and small white cells. The interstitial tissue was not increased. Between the tubules, however, especially in the medulla, there was a considerable amount of homogeneous matrix, staining pink with eosin. In the medulla the groups of *Monilia* were confined mainly to the periphery. In the clusters there was no caseation, no increase of fibrous tissue and there were no giant cells present.

The organism isolated from the heart blood of this rabbit was injected in a similar manner into another and in the same manner the second rabbit died in five days. At the autopsy the naked-eye appearances of the organs was in every way identical with those described above, as were also the microscopic pictures of the kidneys. The same *Monilia* was isolated from its heart blood and from the organs.

Case II.—Singhalese girl, aged about 11, admitted to the Infectious Diseases Hospital of Colombo with the diagnosis of diphtheria. There were white patches on the tonsils, uvula, and soft palate. The temperature was rather high (102° F.); the pulse frequent and of low pressure. There was swelling of the angle of the jaw. The child developed symptoms of broncho-pneumonia and died three days after admission. (Anti-diphtheria serum was given twice by the physician in charge of the hospital.) The microscopical and bacteriological examination of the patches for the Klebs-Löffler bacillus, carried out with the usual technique, serum-media, &c., being used, remained negative. No bacteria of any kind were seen in the specimens directly taken from the patches, but numerous mycelial and conidial elements of a fungus were present. On serum and glycerine agar media no colonies of diphtheria or other bacteria. The fungus had all the biochemical characters of *Monilia tropicalis*, Cast.

Case III.—A young European lady, aged 22, became suddenly ill with sore throat at one of the Colombo hotels. Her medical attendant suspected diphtheria, and called one of us to see her in consultation. When examined her temperature was 101° F., pulse 98; she complained of difficulty and pain in swallowing, both tonsils and uvula were covered with white creamy patches. Preparations made from the patches revealed an enormous number of yeast-like organisms and a few cocci, while no bacilli of any kind were present. This, of course, was sufficient to exclude diphtheria. On glucose agar tubes a fungus grew in pure culture with all the characters of a *Monilia*. This *Monilia* rendered milk slightly acid, and then decolorized it completely; it did not liquefy serum or gelatine. On serum it induced a narrow zone of black discoloration all round the growth.

Tonsillo-oidiosis.—The clinical symptoms are identical with those observed in tonsillo-moniliasis. The oïdium so far found in these cases is *Oidium matalense* Castellani.

Illustrative Case.—European, aged 25, has been several years in Ceylon. In November, 1914, was taken ill with severe sore throat, difficulty in swallowing, and high fever (104° F.). When seen by one of us, twelve hours after onset, both tonsils were covered with a white exudation, but not the uvula. The microscopical and bacteriological examination showed absence of the diphtheria bacillus, while a fungus was grown with the botanical characters of an oïdium. Further investigation showed it to be very similar to *Oidium matalense*.

Tonsillo-hemisporosis.—The clinical symptoms are somewhat different from tonsillo-moniliasis. The onset is as acute, but the general and local symptoms do not disappear so rapidly, the patches are not creamy, they are greyish brown, or even greyish-yellowish, and are very resistant to treatment. The fungus found is a *Hemispora*, usually *Hemispora rugosa* Cast.

Case I.—European planter, admitted to the Kandy Hospital on May 2, 1913. The illness had started two weeks previously. Temperature on admission 101° F. Complained of severe pains in throat and difficulty in swallowing. Flushed face; felt extremely weak and exhausted. Voice thick and nasal. Swallowing painful and difficult. Fluid regurgitated through nostrils. There was a profuse flow of saliva. The muscles of the neck were rigid, submaxillary glands enlarged and painful. The patient was unable to open the mouth wide. Tongue thickly coated and dry; soft palate swollen. Greyish membranes were present on the left tonsil, left anterior pillar, and soft palate. Diphtheria antitoxin (2,000 units) injected the same day into the flank and spray of hydr. perox. prescribed. During the next four days the general condition improved, but the white-greyish membranes in the cleft between the left tonsil and the

	(+)Inose	Invertulose	Maltose	Galactose	Saccharose	Lactose	Mannite	Dulcite	Dextrin	Raffinose	Arabinose	Adonite	Inulin	Sorbite	Starch	(Glycerine	Inosite	Salicine	Amygdalin	Isodulcite	Erythrite	Gelatine	Serum	Litmus milk	Colour of growth on glucose agar
<i>Monilia ceylanica</i> , Cast. ...	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	Yellowish
<i>balanica</i> , Cast. ...	G	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	White
<i>parabalanica</i> , Cast. ...	G	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	:
<i>krusci</i> , Cast. ...	G	G	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	:
<i>parabrusci</i> , Cast. ...	G	G	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	:
<i>panaji</i> , Cast. ...	G	G	G	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	:
<i>noharrai</i> , Cast. ...	G	G	G	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	:
<i>madagandimensis</i> , Cast. ...	G	G	G	G	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	:
<i>alba</i> , Cast. ...	G	G	G	G	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	:
<i>albicans</i> , Robin. ...	G	G	G	G	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	+	0	0	:
<i>tropicatis</i> , Cast. ...	G	G	G	G	G	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	:
<i>neotropicalis</i> , Cast. ...	G	G	G	G	G	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	:
<i>rhoi</i> , Cast. ...	G	G	0	G	G	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	:
<i>guillemondi</i> , Cast. ...	G	G	0	0	G	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	:
<i>macedoniensis</i> , Cast. ...	G	G	0	G	G	0	0	0	0	0	0	0	G	0	0	0	0	0	0	0	0	0	0	0	:
<i>iserebaniensis</i> , Cast. ...	G	G	0	G	G	0	0	0	0	0	0	0	G	0	0	0	0	0	0	0	0	0	0	0	:
<i>parachalmeri</i> , Cast. ...	G	G	0	G	G	0	0	0	0	0	0	0	G	0	0	0	0	0	0	0	0	0	0	0	:

G = gas; 0 = absence of gas; C = presence of clot; + = positive; s = slight.

left anterior pillar were still very evident. Nine days after admission there was still a small whitish patch visible, but the patient felt quite well and was discharged the following day. In smears made from swabs taken for examination no bacilli were seen, a few cocci were present, and numerous large mycelial segments of a fungus. Serum tubes and glycerine agar tubes were inoculated as usual, and gave the presence of mycelial threads, also several sugar agars. The serum and glycerine agar tubes did not show any growth of the diphtheria bacillus; instead there was growth of a fungus with a peculiar crinkled surface which one of us had found previously in 1909 in a case of bronchitis. Being uncertain of its classification it was placed temporarily in the genus *Monilia*, and called *Monilia rugosa*. It was later sent to Professor Pinoy, of the Pasteur Institute, who, after a long botanical investigation, came to the conclusion that the organism belonged to the genus *Hemispora*. The correct name of the fungus became therefore *Hemispora rugosa* Cast.

Case II.—Mrs. N. B. came to consult one of us in September last year. She was complaining of sore throat which had begun three weeks ago and she had been treated with various gargles. There was a large greyish-yellowish patch on the right tonsil. The microscopical and cultural examination revealed presence of a *Hemispora*. The local condition healed after repeated applications of iodine. It is interesting to note that soon after, she developed signs of bronchitis and the same fungus was found in the sputum.

CONCLUSIONS.

Acute and subacute affections of the tonsils of mycotic origin are frequently met with in the Tropics, and are not at all rare on the Continent of Europe and in this country. Most of the cases we have observed were due to fungi of the genus *Monilia*, a few to fungi of the genus *Oidium* and of the genus *Hemispora*.

Cases of acute tonsillomycosis generally terminate favourably, but not constantly. These conditions have a certain practical importance, as they may simulate diphtheria, and are not infrequently treated as such.

A Report on the Treatment of Ten Cases of Kala-azar by Sodium Acetyl-para-aminophenyl-stibiato ("Stibenyl").

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IN 1916 Caronia treated four cases of kala-azar with an organic compound of antimony, acetyl-para-aminophenyl-stibiato of soda, with apparent cure of three of the cases by intramuscular injections of the drug, and in 1920 Manson-Bahr reported encouraging results with this preparation in a case of kala-azar and in one of trypanosomiasis. Messrs. Allen and Hanbury put it on the market in ampoules ready for use under the name of "stibenyl." As it was reported to contain a larger proportion of antimony than the potassium and sodium tartrate salts, and to be considerably less toxic than the latter for animals, while the intramuscular method of administration was a great convenience, it was first tried in India by Major Mackie. In a letter to the *Indian Medical Gazette* in February, 1921, Mackie pointed out that great care was necessary in its use, while in May of the same year Manson-Bahr recorded severe symptoms following its use, but still considered it might prove useful

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in kala-azar. In June, 1921, Forsyth also reported that his experience with the drug had not been very encouraging, and in the *Journal of the Royal Army Medical Corps* for August last (1921), Gatt reported a case ending fatally under this treatment.

In the present year three cases under Mackie and seven treated by myself are recorded in detail. Two sets of Allen and Hanburys' ampoules were employed, the last being used very shortly after reaching India in as fresh condition as possible. Six cases were treated with the first batch of ampoules; diarrhoea, vomiting, and abdominal pain were marked features which supervened, and in two cases death took place with collapse within one to two days of the third dose. In two the treatment was abandoned, and in two apparent recovery took place. The last four cases were treated with the fresh batch of ampoules, which was not quite so toxic, but the drug proved ineffective against the disease, as in two cases after a full course the Leishman-Donovan parasites were still obtained by spleen puncture, and were also numerous in the spleen of one patient who died with high fever and collapse after the ninth injection. In the fourth the treatment had to be abandoned on account of toxic symptoms after the second and fifth doses, but the patient eventually recovered under sodium antimony tartrate treatment.

Thus, apart from the serious toxic symptoms, in two cases an amount of "stibenyl," the antimony content of which was quite equal to curative amounts in the potassium and sodium tartrates, not only failed to cure but seemed to exhibit the infection absolutely undiminished; so the result of the attempt at treatment in these ten cases was not encouraging. Possibly the drug had a cumulative action, as it was noticed that an intolerance seemed to develop; this was shown by the shorter intervals after the later doses in which the toxic symptoms developed. The conclusions arrived at are: (1) that the use of "stibenyl" in the treatment of kala-azar was not very successful in the hands of the author and of others in India; (2) that there is some factor to be taken into consideration other than that of the relative toxicity of this organic compound of antimony; and that this probably applies to other complicated organic compounds of antimony, which should therefore be used with the utmost caution; and (3) that the curative effect of an antimony compound is not in direct proportion to its antimony content.

The Earliest Account of Pellagra in Egypt from the Writings of an Egyptian Physician, A.D. 1846.

Translated from the Original by M. KHALIL, M.D., D.P.H.,
D.T.M. & H.

IN his review of the history of pellagra in Egypt in "Medical Diseases of Egypt," the late Dr. Sandwith made a casual reference¹ to an account of the disease by "Ahmed Ibn Hassan El-Rashidy." He appears to have been under the impression that the publication of this physician was a translation from a French source. On consulting the catalogue of the Oriental Library of the British Museum, I find several Arabic books by El-Rashidy which were published in Cairo. Of these, two were translated from the French—one dealing with "Diseases of the Eye," the other with "Gynæcology and

¹ I am indebted to Dr. Sambon for drawing my attention to this reference.

Obstetrics." Two were his own work: one is on "Diseases of Children," and the other is a first volume which treats of "Skin Diseases." The latter publication contains an account of pellagra. It is probable that this account is based on his personal observations, for he advises a method of treatment called "Hommosa," which is in vogue amongst quacks in Egypt to-day, although unknown to Western Medicine. It is not a specific treatment for pellagra, but is used as a counter-irritant in many diseases. A small incision is made near the site of the disease, if localized, and a "hommosa" (pea) is inserted to prevent the wound from healing. The occasional good results are attributed by some modern physicians, who never advocate its use, to bacteria in the wound acting as a vaccine; by others to its counter-irritant effects, and possibly to its action as a "fixation abscess," advocated in some cases of puerperal infections.

In addition to the account of pellagra, there is a short note on "Mal de la Rosa," which the author regarded as a distinct disease with which he was, however, personally unacquainted.

The following is a translation of this earliest account of pellagra in Egypt:—

PELLAGRA.

Syn.—Poverty disease; circular erythema; nervous erythema; chronic erythema, &c.

It is a disease of the digestive organs with disturbances of the nervous system. Rarely there is disturbance of the nervous system alone. The skin affection is not always present. The back of the hands and of the feet, the legs and the forearms, all round the neck and to a less extent the face, are affected. The extent depends on what parts are exposed to the sun.

There are three kinds of skin lesions:—

(1) Rosy redness or slightly dark with cracks, the superficial layer may fall off and the skin under it appears brilliant red and slightly œdematous. There is prickly sensation, heat and discomfort in the area, increased by exposure to the sun.

(2) Irregular bullæ containing yellowish or reddish serum, which on drying forms fine scales.

(3) Thickening of the skin with yellow or brown colour, dry and rough, resembling scales of fish. There is no redness or pain.

Early in the disease the skin change is scarcely apparent and is considered by the patient to be the result of exposure to the sun; protection from the sun causes this change to disappear. The skin lesion normally disappears at the end of summer; it returns at the beginning of the spring every year for several years.

The tongue is dirty or red at the sides. There is a burning sensation in the mouth and pharynx, thirst, dyspepsia, flatulence, emaciation, pain in the abdomen, loss of appetite, nausea and occasionally vomiting. The pain in the abdomen is excited by pressure, or begins spontaneously. Diarrhœa is commonly present but sometimes constipation is severe. There is giddiness, headache and the patient hates work. The symptoms are, however, very irregular.

Severe cases, not necessarily accompanied with severe skin lesion, sometimes with no skin lesion at all, are particularly seen in the children of those who have died from the disease. Their symptoms, besides those already mentioned, are exfoliation of the mucous membrane of the lips, painful

micturition, conjunctivitis especially in the spring, muscular pains and nervous symptoms, as hysteria, &c. Mania and melancholia are common.

The disease may be accompanied by psoriasis, pityriasis, leprosy, erysipelas, itch, acne, eezema, In old cases hemiplegia may occur.

Pathology.—The skin is thickened six to eight times the normal. It is dark and friable. The nerve terminals are thicker than normal and, if cut, serum oozes from them. The membranes of the brain are congested with black blood; sometimes they are inflamed. (Magnifying apparatus was used.)

The disease is not infectious. It is present in villages among poor working people. It is commoner in women than in men, rare in children and old people. It is said to occur more frequently in hysterical people, especially the poor and the unhappy. It affects the children of those affected soon after birth and thus there is a predisposition to the disease.

The disease is said to be caused by eating maize, much salt in the food, filthy water, insanitary houses, foul air, overwork and starvation. The skin lesion is due to exposure to the sun.

Differential Diagnosis.—From erythema, acute and chronic: There is no limitation to the parts exposed to the sun, no intestinal or nervous symptoms. The same serves to differentiate it from lupus and, moreover, the patches of lupus are round and heal in the centre and spread at the edge. Acro-dynia is a very similar disease, which caused an outbreak in Paris in 1828.

Prognosis.—Fatal pellagra resembles ordinary fever with incontinence, mania, convulsions, emaciation and death.

Treatment.—The patient should leave his surroundings and change his work and habits. The poorer people affected unfortunately cannot do this. Protect patient from the sun and give him good food, especially milk. At the onset venesection and purgatives. Warm baths and leeches are good. For the diarrhoea give opium, rice water and anodyne enemias. For the brain and cord symptoms use blisters, seton over the neck and the back, and "hommosa." For delirium, apply cold applications to the head, also apply leeches and employ venesection. For shivering, use sulphur-bath, either warm or cold.

Mal de la Rosa found in Spain is allied to pellagra, but the skin eruption leaves marks resembling scar tissue in appearance and depressed below the skin level. Disturbances in the head and upper half of the body occur and the patient cannot stand. The nervous symptoms are less pronounced than in pellagra, but there is loss of the sense of touch, followed by redness and ascites.

Treatment: Antimony preparations and balsams, but my information on this disease is incomplete.

The volume containing the above account was printed in Cairo in 1846, i.e., the year before the publication of Pruner's record of Pellagra in Egypt, which has hitherto been regarded as the earliest reference to the disease in Egypt. I have given as literal a translation as possible of El-Rashidy's account, but it is a long one, occupying seven pages of text and I have, therefore, confined my translation to statements of special interest.

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Notes on Two Cases reported as Yellow Fever.

By E. WILKINSON, F.R.C.S., Lieut.-Col. I.M.S.(Retd.).

DURING the year 1920, two cases of yellow fever were reported in this country. I happened to be concerned with both and, so far as I have been able to ascertain, no similar occurrence has been reported since the outbreak of yellow fever at Swansea, in 1865, a full account of which was given by the late Dr. (afterwards Sir) George Buchanan in the Eighth Report of the Medical Officer of the Privy Council for 1865.

One of the cases was reported in January and the other in November, therefore you will be prepared to hear that upon investigation neither proved to be a case of yellow fever. When I add that both the patients were seamen who had just arrived from the West Coast of Africa, you may surmise that if their disease was not yellow fever, it was probably malaria. Both were in fact cases of subtertian malaria and one ended fatally. Although they thus lack the importance which would naturally attach to true cases of yellow fever occurring in this country, they both present certain points of interest.

The first case was that of an apprentice seaman, aged 19, who, on January 19, was put ashore at Dover from the ss. *Simoom*, which had left Bathurst, West Coast of Africa on January 5. He was apparently in a dying condition and he died in hospital the next day.

The Port Medical Officer who diagnosed this case as one of yellow fever reported it by telegram to the Ministry of Health, adding that the vessel had proceeded to Rochester. Two of us were immediately instructed to make investigations; my colleague, Colonel James, went to Dover and I to Rochester for this purpose. Colonel James made a post-mortem examination of the body: he could not find anything suggestive of yellow fever, but he found numerous subtertian malaria parasites in the spleen and therefore concluded that the patient died of acute malaria. Meanwhile, my inquiries on board the vessel at Rochester had led me to the same conclusion. For I found two more of the crew to be suffering from fever with which they had just been attacked and which I had no hesitation in diagnosing as malaria. I took specimens of their blood to the Ministry's laboratory, where both showed heavy infection with subtertian ring forms.

The apprentice who died at Dover had only been ill four days, having first complained of not feeling well on January 16: that is, eleven days after the ss. *Simoom* had left Bathurst. The vessel's log shows that his temperature on the first day was normal; on the second day it was 100 F., but he was "showing signs of malaria." He was accordingly given a dose of 10 gr. of quinine at once, and another dose every five hours "according to the Medical Guide." Next day his temperature was again normal but he was very weak and ill and would not take food. The master seems to have done everything he could for the lad. On the 18th he altered his course for Dartmouth to get medical treatment for him, but he was unable to make the port owing to fog: he therefore proceeded up Channel sending a wireless message to Dover for medical assistance. The vessel reached Dover early in the morning of the 19th, and a doctor came on board at once and sent the patient ashore. He

died in hospital the following morning in a condition of coma with a temperature of 107° F.

On the day on which the apprentice was taken ill the Chief Officer of the *Simoom* had died and was buried at sea. He also had been ill for four days only, having been taken ill on January 10, but had remained at work until the next day, when he reported sick, complaining of pains in the stomach. The following day his temperature was just over 99° F.; on the 13th he felt better and tried to keep his watch, but was looking so ill that the master sent him below. The next day he was very weak with a temperature of only 95° F. and had difficulty in passing water, which the Captain thought was due to a previous stricture. Two days later he was very weak indeed, his temperature was normal but he would take no food and he died at 4 o'clock the next morning. He had had no quinine.

Probably this case was also one of malaria, for in its short course, with great weakness and anorexia, it resembles that of the apprentice in whose case also no high temperature was recorded while he was on board ship. As the temperature appears to have been taken only once daily, quite possibly the febrile period escaped notice.

The ship's company of the *Simoom* consisted in all of twenty-six persons, Europeans, and all British except one. They had all been on board during the outward and homeward voyage and had been up the River Gambia as far as McCarthy Island for sixteen days, where it was noted that mosquitoes had been very troublesome on board. Besides the two men in whose blood malaria parasites were found, several other members of the crew told me that while at Bathurst they had suffered from chills. One said that he had been ill there three or four days with chills and headache and once his temperature was taken by a doctor at Bathurst and was found to be 103° F.; the doctor treated him with quinine. The climate of Gambia is described in a gazetteer as "fairly healthy during the dry months." It would seem that the *Simoom* was not there at that time of the year, for at least a quarter of the crew were attacked with malaria, to which one of them succumbed, the death of another having been also probably due to this disease. No case of yellow fever is reported to have occurred in Gambia since 1917, when one death occurred.

The second case reported as one of yellow fever has an added point of interest attaching to it as it was at one time suspected to be one of plague before it was finally demonstrated to be, like the first, a case of malaria. The patient was admitted to the Guardians' Infirmary at Swansea on November 1. He was then extremely ill; had a temperature of 101° F. and he was jaundiced. His illness was diagnosed as yellow fever and he was placed in a separate ward. There was considerable delay in information relating to this case reaching the Ministry of Health, but on its receipt I was instructed to make inquiries. By this time the patient had been transferred to a general ward, where I saw him. He was then sitting up in bed, but was looking ill and anæmic. The skin of his face was slightly yellow but there was no yellow tinge on the rest of the body or on the conjunctiva. His tongue was rather dirty and his pulse 116, hard and bounding. His temperature, however, was normal. Both the liver and spleen were enlarged, the edge of the spleen being palpable about a finger's breadth below the margin of the ribs. A bubo about 3 in. in diameter which was red, brawny and very painful and tender, occupied Scarpa's triangle in the right groin. No separate gland could be recognized in the swelling and no definite fluctuation could be obtained. The appearance of the bubo, however, so strongly indicated the presence of pus that it was incised

under a local anæsthetic and about an ounce of thick pus was evacuated to the great relief of the patient. In the left groin a femoral gland was enlarged to the size of a large broad bean; it was soft and there was no inflammation round it; none of the other superficial glands was palpably enlarged. On both legs, calves, thighs, buttocks and loins were dark dry scabs of boils. There were also some on the back of the neck and a few on the back. The patient said he had had the boils several weeks previously but that the swelling in the right groin only began about a fortnight before I saw him, as a slightly hard lump. It had not been painful or tender, but it had gradually become bigger as well as painful and tender a few days back when it was painted with iodine. The temperature chart of the earlier portion of the patient's stay in the Infirmary had been mislaid, but from November 12 it showed that he had had a rigor on that date, when his temperature was 100° F., as it also was on the 13th; it fell next day to 99° F. and since then it has either been normal or sub-normal. On November 5, the patient's serum-reaction had been tested against members of the enteric group, but with negative results. A similar result had attended a search for spirochætes in the blood and urine.

The patient told me that he had been a fireman on board ss. *Clemenceau*, which he had joined at Sharpness, on September 16. The vessel after taking in bunker coal at Cardiff sailed on September 23, in ballast to Dakar, which she reached on the eleventh day. She stayed there a little over three weeks and took in cargo of monkey nuts from barges. She remained in stream all the time about a quarter of a mile from the shore and none of the crew were allowed to land; mosquitoes had, however, been very troublesome on board. The *Clemenceau* left Dakar on October 16, and reached Plymouth fourteen days later, that is October 30. Nobody was ill at Dakar but on the second day out one of the trimmers was taken ill with fever, and a day or two later the chief engineer and the patient himself were taken ill. The patient did not complain for two days, but after that he was too weak to lift a shovel, so he "went sick." He had headaches, cold chills, and sweatings every day. He had no pain but was very thirsty and could not take anything but water and milk. He was sick three or four times, his vomit was yellow and he turned yellow in a few days. The ship's company consisted of twenty-nine persons, and of these, besides the patient, five were taken ill on the voyage home from Dakar, all with the same kind of illness and all were treated with quinine. He left the *Clemenceau* the day she reached Plymouth, with another man who had also had fever, who came as far as Cardiff. The chief engineer was still very ill and a fireman was so ill he had to be taken to hospital at Plymouth on a stretcher. A greaser went to his home at Shields from Plymouth and another man went to Hull. Both of these men had had fever.

The patient's history and condition were so strongly suggestive of malaria that I expressed the opinion that this was what he was suffering from on admission to the infirmary. There remained his bubo to be considered. Having had so much to do with plague I naturally think of this disease when I see a bubo. This one was, in fact, very like a plague bubo, as I pointed out to the medical men who were with me. I said, however, that I did not consider there was any real suspicion of plague, as the manner in which the bubo began was not at all like what ordinarily occurs in plague and, moreover, inasmuch as it had not appeared until several days after the patient was admitted to the infirmary it was exceedingly unlikely that it could be plague even had it occurred among the rats on board the patient's ship. The only way in which infection could

have been so greatly delayed would be by means of a bite of an infected flea, inflicted after the patient's admission to the infirmary. As however the patient had had a bath on admission and was wearing the infirmary clothes this seemed entirely improbable; moreover, the condition of the man's legs which, it will be remembered, were encrusted with the scabs of boils, was sufficient to account for the presence of the bubo. I took blood films from the patient's finger tips and collected some pus from his bubo, as it was opened, in a sterile test tube. Both the blood films and the pus were immediately examined by Dr. Sladden, pathologist to the Swansea General Hospital, and myself, but we could not find any malaria parasites in the blood and the only organisms visible in the pus were staphylococci. Specimens of the blood and of the pus were also examined at the Ministry's laboratory. Again no malaria parasites were found in the blood but the pus was reported to contain, besides numerous cocci, masses of bi-polar-staining bacilli, many of which were indistinguishable from *Bacillus pestis*. I was shown microscopical specimens but was not satisfied that the organisms were true *pestis*. The next day it was reported that the culture also resembled *Bacillus pestis*, though again the appearance was not quite typical. Further cultures however showed that the organism was not *Bacillus pestis*, as did also the result of animal inoculation. Before these negative results were obtained however there was sufficient presumption of the existence of *Bacillus pestis* to indicate the wisdom of making further inquiries regarding the ss. *Clemenceau* and the ship's company, and this was accordingly done. A telegram was immediately sent to Dr. Williams, the Port Medical Officer at Plymouth, and he replied that the *Clemenceau* had reached Plymouth on October 30, having put in there owing to sickness among the crew. There were then seven cases of illness on board, all of typical malaria. Three of the patients were so ill as to require removal to the general hospital for treatment; their blood was examined there, in each case the malaria parasite was found. They were treated with intravenous injections of quinine and two were subsequently discharged convalescent. The third died in hospital with pneumonic symptoms, and his death was certified as having been due to malaria and pneumonia. The other four were well enough to proceed to their homes, and inquiries made there, while they gave no hint of plague, showed that two of the men had suffered further attacks of fever during which the parasites of subtertian malaria were found in their blood. Dr. Williams confirmed the statement of the patient I had seen, that none of the crew were allowed to go ashore while on the West Coast of Africa, the vessel lying in the open roadstead and being loaded from lighters. Dr. Williams had made inquiries regarding sickness and mortality among rats during the voyage but could obtain no evidence of this. He added that the vessel was regularly employed in the West African trade, of the risks of which the commander was fully alive.

There had been no record of the occurrence of plague or yellow fever at Dakar during 1920.

The great interest of this second case to me was the discovery in the bubo of a pestis-like organism. This is the first occasion on which I have encountered such a thing and I have hitherto considered it to be almost an axiom that if a pestis-like organism were obtained from a human being the case was undoubtedly one of plague. This, of course, does not apply to other animals for the *Bacillus pestis* very closely resembles members of the *Pasteurella* group and can only be positively differentiated from them by both cultural and animal experiments. As already seen these experiments finally negative the conclu-

sion that the organism was *Bacillus pestis*. The question as to what it really was requires settlement.

From the further experiments that were conducted in the Ministry's laboratory it would appear that there was a mixed infection, one of the organisms giving the reaction of *Bacillus proteus* and the other being assigned to the *Bacillus coli* group. Dr. Sladden, who has also made cultures from the pus from the patient's bubo, found in the primary culture colonies of a bacillus which were very suggestive of *Bacillus pestis* and the bacilli seen in stained films showed some polar staining. No involution forms were, however, found on salt-agar slopes and there was no stalactite formation in broth. Animal inoculation was negative after thirty hours, and when the animal was killed on the fourth day all the organisms were found to be healthy and no bacillus could be recovered. Dr. Sladden took cultures from the crusts of some of the boils on the patient's legs and from the healing bubo, but this gave negative results.

When I first saw the patient I was so convinced that he was or had recently been suffering from malarial fever that I warned the medical officer in charge of the likelihood of a further attack, and it is satisfactory, from the point of view of clearing up the diagnosis, to know that the patient had another attack of fever on December 6, when subtertian malaria parasites were found in his blood both by Dr. Sladden and at the Ministry's laboratory.

Thus both the cases reported as yellow fever were in the long run proved definitely to be cases of malaria.

Apart from other points they are interesting as showing that Europeans visiting West Africa are still in great danger of contracting malaria. Of the *Simoom's* crew of twenty-six three persons were found by the discovery of malaria parasites in their blood or spleen to have suffered from malaria, and one of these died. Another death on the voyage home was also most probably attributable to malaria, and the symptoms of several others of the crew indicated that they had also suffered from it. It will also be remembered that the crew of the ss. *Clemenceau* had not been allowed ashore while at Dakar, but, as has been seen, no less than seven of the twenty-nine were attacked with malaria on their voyage home and one died.

Section of Tropical Diseases and Parasitology.

President — Sir LEONARD ROGERS, C.I.E., M.D., F.R.S., I.M.S.

Dysentery in the West Indies.

By J. S. ANDERSON, B.Sc., M.D.

(*London School of Tropical Medicine.*)

THE Colonies comprising the British West Indies present a rich mine of material which has been comparatively little explored by the workers in tropical medicine and sanitation. In a recent expedition organized by the London School of Tropical Medicine and led by Professor Leiper, I had an opportunity of studying the health and sanitary conditions of various communities in the Caribbean zone, and to me the most striking feature in the annual reports of the respective Chief Medical Officers, was the high rate of mortality and morbidity attributable to dysentery and allied intestinal disorders.

The members of the Commission travelled separately and one or other of them called at Barbados, Trinidad, Grenada, St. Vincent and St. Lucia in the Windward Islands, and Antigua in the Leeward group. Headquarters were established in Georgetown, the capital of British Guiana and there we met with every facility and kindness that could possibly contribute to the success of our investigation.

The report of the Surgeon-General for British Guiana shows that in 1919, out of a total of 12,000 deaths, 112 were due to enteric fever, but undefined intestinal disorders (over one year) were responsible for 1,050 deaths. In the City of Georgetown, the corresponding figures were forty-eight deaths from enteric fever and 217 from intestinal disorders. In the two largest medical districts of Demerara with a population of 50,000, lying adjacent to Georgetown, the average mortality for the past seven years was as follows: Dysentery, 46; enteric fever, 6; other intestinal disorders, 89. Deaths from all causes, 1,124.

The Surgeon-General for Trinidad and Tobago reports that in 1920, the deaths in the Colonial and District Hospitals were as follows: Dysentery, 135; enteric fever, 191; diarrhœa and enteritis, 84. Deaths from all causes, 1,875.

In the island of St. Vincent, with a population of 54,000, the statistics for 1920 are limited, but one table gives the following details of deaths: Dysentery, 24; enteric fever, 2; diarrhœa and enteritis, 84. Deaths from all causes, 594.

In Grenada, population 75,000, the Colonial Surgeon's report for 1920 gives: Dysentery, 99; enteric fever, 37; diarrhœa and enteritis, 304. Deaths from all causes, 1,318.

The island of Jamaica is the most populous of the West Indian Colonies, and the figures for its capital, Kingstown, may be taken as representative:

Dysentery, 85; enteric fever, 130; diarrhoea and enteritis, 222. Deaths from all causes, 2,040.

Barbados has acquired some reputation as a health resort but has not published any vital statistics since 1913.

Striking a rough average from the above data, it will be seen that true dysentery accounts for about 5 per cent., and intestinal diseases for over 20 per cent. of the total deaths in the West Indies. These figures are sufficient to indicate that in terms of mortality and morbidity, dysentery and its allied diseases must take rank as a grave factor in the depletion of the various colonies, and that there is urgent need for a careful scrutiny of the equipment provided for their diagnosis, prevention, and treatment.

The facilities for laboratory diagnosis are extremely limited and with the possible exceptions of the capital towns in Trinidad, Jamaica and Demerara, there is no centre for microscopical examination. It is a Utopian maxim that no medical man should enter the Tropics without his microscope, but the average medical officer in the West Indies has little leisure, and the methods of cytological examination have not been widely published. The diagnosis, therefore, in the vast majority of cases is based entirely on clinical grounds.

In some parts of South America the three main types of dysentery are found—viz.: (1) Bacillary—due to the *Bacillus dysenterix*—Shiga or Flexner Y group; (2) Protozoal—due to the *Entamoeba histolytica*, or to the malarial plasmodia; (3) Helminthic—due to *Schistosoma mansoni*.

In the British West Indies conditions suitable for the development of schistosomiasis do not seem to be present and malarial dysentery is comparatively rare owing to the widespread use of quinine. The question of diagnosis is therefore practically limited to the two diseases, amœbiasis and bacillary dysentery. In the absence of laboratory facilities, the differential diagnosis of the type is usually based on the response to emetine, and the medical men of widest experience consider amœbic dysentery to be decidedly more prevalent than bacillary. In many cases, especially amongst the poor, medical advice is not sought or is unavailable during an acute attack; the dietary is often limited and unsuitable, and a large proportion of infections take on a chronic character. The general health is thus reduced, the path is opened up for other tropical diseases and the result is a high morbidity rate throughout the plantations, with serious loss to industry.

Complications usually found in the East are singularly uncommon in the West Indies. Exact data are very difficult to procure but in a residence of nine months I saw only one case of perforating amœbic ulcer and two cases of liver abscess. The arthritic complications of bacillary dysentery are very seldom seen.

The agencies by which the disease is spread are not difficult to trace. In a tropical climate, with abundant moisture, luxuriant vegetation, and haphazard methods of sewage disposal, flies are ever prevalent. In the country districts large numbers of labourers, chiefly blacks and coolies, defæcate on the banks of the canals and streams which provide the drinking water supply for the neighbouring hamlets and villages. In the same water supply the housewife washes her vegetables and the milkman his utensils.

In municipal areas the problem of sewage disposal is too often a matter of acute controversy. Probably the best picture of urban conditions is contained in the report of the Colonial Secretary for British Guiana printed in 1919.

“The system of sewage disposal in Georgetown, consisting as it does of cess-tanks and midden-pits is, in the words of Dr. K. S. Wise, not only a disgusting anachronism,

but is also a serious menace to the health of the city. The midden-pits are repulsive and hardly approachable, the night-soil remaining open to the air in a semi-fluid state; they are the hunting-ground of rats and the breeding-places of flies and other vermin. The rise of ground water in wet weather brings all the foul gases to the surface polluting the atmosphere: and ultimately it drifts the contents of the midden-pits out on the surface of the premises where it floats, settles, and finally dries. The cess-tanks are perhaps not so obvious but equally insanitary and dangerous. The midden-pits and cess-tanks are periodically emptied by evacuation. This process, as also the subsequent journey of the material through the city, is a nuisance only too obvious to those who use the streets. It is thus evident that a large quantity of sewage is never removed from the premises. The ground is polluted with filth, the air is permeated with sewage-dust; rats hunt in the pits; flies breed there; both enter adjacent houses and foul exposed food. What wonder that filth-borne diseases are so common."

Since that date small septic tanks have been installed in most of the public and Government buildings, but for the greater part of the city inhabited by the masses, the picture remains unchanged.

British Guiana is probably unique in the difficulty of its sanitary problems because the inhabited portion of the colony is on an average about 4 ft. below the level of high tide. Most of the other colonies are of volcanic origin and there is little or no difficulty in obtaining good drainage slopes.

All the colonies have the advantage of up-to-date medical advice, but when the sanitarian advances a comprehensive scheme of improvement the administrator points to the unenlightened state of the inhabitants, and the financier to the falling returns from sugar, cocoa, and rice. Prevention, therefore, apart from a few centres of municipal enterprise, still remains in the realm of pious hopes.

The treatment of dysentery also leaves something to be desired, and until means are initiated for early and accurate diagnosis, this must remain unsatisfactory. Living amidst luxuriant tropical vegetation, the inhabitants have a number of popular remedies, each with some local reputation and each with an element of scientific justification. Ipecacuanha flowers by every wayside, and infusions of the plant were used empirically long before the birth of the "British Pharmacopœia." The bark and the wood of various trees and shrubs have a more or less tribal repute, but the virtue of these infusions probably lies in their astringent properties. One of the forestry officers sent me a preparation called "dakamballi," which has a certain reputation amongst the Carib and other aboriginal Indians, as a cure for dysentery. It is prepared from the fruit of a tree, *Aldina insignis*, and is mixed with milk or water to form a paste. The Wellcome Research Bureau kindly made an analysis of the substance and reported that it was practically a pure starch. The therapeutic effect of this "dakamballi" starch was tried on six cases of dysentery in the wards of Georgetown Hospital. It was found to be similar in its action to arrowroot, and apart from its high starch content, it could not be regarded as having any specific action on any type of dysentery. As a food suited to all kinds of intestinal disorder such as diarrhœa, enteritis, and dysentery, it possesses undoubted value, and it might form a useful adjunct to the diet of the Indians, especially in times of famine when other farinaceous preparations are not available. In the towns and on the plantations, medical advice is within easy reach, and the treatment is, of course, on modern lines. Acute cases are treated, as a rule, with hypodermic injections of emetine and saline aperients, a combination which has the merit of being orthodox whichever way the diagnosis swings. Sometimes a medical man will give one or two injections of emetine only, and if no improvement results he will diagnose bacillary

dysentery and prescribe sulphate of soda. Anti-dysenteric serum was not used in any of the colonies visited; either the cost was regarded as prohibitive or doubts were expressed as to its keeping properties. The diet prescribed is usually milk and that even under the most hygienic conditions is not satisfactory; in a community where there is little or no control over the quality and contents of the milk supplied, it becomes a positive danger. In the majority of cases the medical man has to deal with a patient who has strong opinions of his own with regard to diet. Under such conditions, the wonder is not that so many of the immigrants to these colonies die of dysentery and allied diseases but that so many escape.

If this brief sketch should arouse any interest in the medical and sanitary problems of the West Indies, it will have served its purpose. Locally there is no lack of workers endowed with vision and enthusiasm, but there still remain three clamant needs: (1) A little encouragement and intelligent support from the home Government; (2) a closer association between the insular medical services and the tropical schools of England; and (3) an extension of the educational facilities for the rising Colonial generation.

Some Observations on Tuberculosis in India.

By ARTHUR POWELL, M.B.

FROM the year 1888 to 1899 tuberculosis was exceedingly rare in my practice in Assam, although a large proportion of the population were immigrants from Madras, the North-West Provinces, Central India, Chota Nagpur, and other distant country districts.

In 1901 I went to Bombay City, and was at once startled by the prevalence of tuberculosis there. This incidence is scarcely shadowed by the municipal mortality returns, for the population of the city is largely a floating one, recruited from the Konkan, the Deccan, or further afield. These immigrants come to the city for work, and when disabled by a chronic disease creep home like wounded animals to die in their native place. Some years ago I found that of 600 city patients who had then died, more than 500 had died outside the city. In another batch of eighty-three, the subject of inquiry by the Commissioner of Police—three died in the city, eighty died in the country. In all tubercle had originated while serving in the city.

Secondly, the mortality returns are necessarily not scientifically accurate, many of the inhabitants being treated throughout their illness by unqualified and often ignorant, practitioners. The "cause of death" is frequently recorded on the verbal statement of the friends of the corpse to the uneducated peon or gate-keeper of the burial ground or burning ghat. Some prominent symptom, such as "Tap"—i.e., "Fever"—is given and registered. The Urdu and Mahratta names for tuberculosis are seldom used and scarcely understood by the coolie of Bombay. When I have asked the relatives of a man dead of tuberculosis what was the cause of death, in nine out of ten cases the answer was "fever."

It is significant that in the Bombay City mortality returns, up to a few years ago—possibly still—there were two headings, "Remittent Fever" and "Malaria." A very considerable number of deaths from tuberculosis are recorded as "remittent fever."

I noted over one period that the mortality returns showed that the proportion of deaths from "remittent fever" to those from tuberculosis was fifty-five to two, while in my own cases at that period the proportion was more than completely reversed, the deaths from malaria being *nil*, from tuberculosis forty-three. Even making due allowance for the fact that hospital treatment for malaria is much more efficient than that which the mill-hand or coolie generally receives, the contrast of these figures is absurd.

During the years 1901 to 1919 the Bombay City Police were under my care. The strength of the force ranged from 2,300 to 3,000. In that period 582 policemen died of tuberculosis *verified bacteriologically* during life. In addition, especially in the earlier years, a large number died of an acute broncho-pneumonia, no tubercle bacilli being detected during life, but in many acute miliary tuberculosis was found post-mortem. These were cases of "galloping consumption" exhibiting a rapidity I had hitherto not dreamed of.

My experience of mill-hands, coolies, and domestic servants was that among them the mortality was equally great. I may instance that in nineteen years I had in my employ eight coachmen or chauffeurs. Five of these died of tuberculosis.

Von Pirquet's test, as applied to a European, has little clinical value unless it be negative. Most adult Europeans give a positive reaction, and this, generally, is mere evidence of the dormant "weed-patch" which autopsy has shown that most Europeans possess. Between 1912 and 1918 I applied this test to 1,532 recruits—new arrivals in the city. Eighty-seven, i.e., 5.67 per cent., gave a positive reaction. It is interesting to note that of these an undue proportion were Pathans or Panjabis. Of ten healthy European controls seven (70 per cent.) were positive.

You will remember the very similar results obtained by Russian surgeons among the Kalmucks. Formerly tuberculosis was unknown among certain Kalmuck tribes, while it was rife in the Russian Army. Several hundred Kalmucks were recruited, and von Pirquet's test applied. Almost all gave a negative reaction. Later they were drafted into divisions in which tubercle was prevalent. Tuberculosis smote the Kalmuck recruits swiftly, acutely, and almost wiped them out. Like the Bombay recruit, they had no immunity, hereditary or acquired—the disease was in all cases acute; recovery was almost unknown.

The Immigrants to the City arrive Uninfected.—I have examined over 12,000 young men, mostly recruits, newly arrived from the country, paying very special attention to the condition of their lungs. The number in whom I detected pulmonary disease was extremely small. All civil surgeons and other practitioners in the country districts have confirmed my observation that in former years tubercle was most rare in the districts, but is now much more common, chiefly in those returning from the city and in their households.

Rarity of Old or Healed Tubercular Lesions in Autopsies on Indians.—Pathologists in England, America, France, and Germany are agreed that the great majority of European adults show post-mortem evidence of old or dormant tubercular lesions. My own records show 72 per cent. for Europeans dying from all causes in India. These were generally "picked" men, specially selected for service abroad. In England, France, and Germany, figures of 75, 80, 85, 90, and even 96 per cent. have been recorded. The statement of an eminent German pathologist: "Am Ende hat jedermann ein Bischen Tuberculose," has become a proverb approximately true for Europeans, but wholly contrary to fact if applied to Indians. Omitting those dying of active

tuberculosis, out of over 8,000 autopsies on Indians, performed by myself, in which I have noted the condition of the lungs, in only 185 were healed or chronic tubercular lesions found—a proportion of 2·3 per cent. Fibroid phthisis was rarely seen. Most of these autopsies were medico-legal cases of sudden death, performed at the Morgue.

Tuberculosis in the Indian is much more acute, and more rapid, than in the European: Recovery is the exception.—I think we can safely say that the great majority of Indian patients die within a year of their first symptoms. Of my own cases, the majority died in less than eight months after the time the tubercle bacillus was first detected. In the way of statistics I offer the following: About thirteen years ago the Surgeon-General took me to task for “unnecessarily invaliding policemen alleged to be suffering from tuberculosis.” An inquiry was instituted by the Commissioner of Police into the subsequent history of all those invalided during the two years from two to four years previous to the date of inquiry. One was reported well, five ill, four could not be traced; seventy-six dead out of the total of eighty-six. Personally I am inclined to believe that the pensioner who “cannot be traced” is, indeed, dead beyond hope of resurrection. Excluding the “cured” case, the longest period of survival was three years, three months.

The First Symptom of Tuberculosis in a Majority of Indians is Fever—not Cough.—In a paper read before the Bombay Medical and Physical Society in 1903, published in the *Indian Medical Gazette*, February and March, 1904, I said: “I think it is a safe estimate to say that nine-tenths of the cases of fever in Bombay which last ten days or more are due to either tubercle, enteric or abscess.” In the subsequent discussion I offered to lay “six to four” odds that when enteric and sepsis had been excluded in Indians, the cause of the fever would be found to be tubercle. My subsequent experience is that I was shamefully “cramping the odds.” The police force, in particular, supplied me with an excellent field for observation in this matter. All constables when sick are obliged to remain in hospital. Our records show hundreds of cases admitted for “fever,” or “P.U.O.,” in which the Enteric group, Malaria and Sepsis, as well as Plague, Relapsing Fever, &c., were excluded by laboratory as well as by bedside tests. This fever is generally quotidian, intermittent, and may be as high as in malaria. Sweats and rigors may or may not be present. There is no special characteristic beyond the fact that gastric disturbance is rare, and complete rest often reduces the temperature. Convinced of the truth of my theory, I and my assistants made the most careful examination of the chest, seldom finding any physical signs of tubercle. Later, cough would set in and the sputum show tubercle bacilli. Though my colleagues were equally unsuccessful, I became ashamed of my want of skill, and spent over a year in study at the special hospitals of England, Germany, and Switzerland. I there learned the value of X-rays in diagnosis, and especially how readily the inexperienced can misinterpret what they see.

The First Physical Sign in Indians is often a Deposit at the Hilum of the Lung when Fever is the only Symptom present.—On my return to Bombay, with the ungrudging aid of Dr. S. B. Nayak, radiologist to our Medical College, and of the radiologists of the military hospitals, we were able to diagnose definitely many cases in the early stage when no other pulmonary sign or symptom was present.

“Low Fever” is often Incipient Tuberculosis.—I base this opinion on the fact that many Indian cases of low fever subsequently die of unmistakable tuberculosis. The majority of European cases cease work, take a holiday to the

hills or proceed on a sea voyage to Europe or Australia and recover completely, as we are justified in expecting the European with incipient tubercle to do. In four Europeans suffering from low fever, the X-rays showed very suggestive shadows. In three these shadows were greatly reduced on their return after six to twelve months' leave in Europe. In the fourth case definite pulmonary tuberculosis became established a year later. Some of you may remember that a distinguished Indian medical officer some years ago announced that low fever was tuberculosis of the urinary organs. His bacteriological colleague endorsed—possibly originated—this error by saying that he found tubercle bacilli in the urine of all cases. It is not for me to explain this error of observation, but I would suggest he was dealing with another acid-fast bacillus, the *Bacillus smegmæ*.

Hæmoptysis is relatively Uncommon among Indians.—In my paper in 1903, already quoted, I state that hæmoptysis “is very rare among natives in the earlier stages and in the majority of cases is never seen at any stage—of ninety-two natives only four gave any history of blood-spitting.” In about 1,000 subsequent cases a little over 5 per cent. had hæmoptysis up to the time they were invalidated or sent to the country. Through the kindness of the officials of several English, German and Swiss hospitals and sanatoria, I was able to read the histories of 504 European cases. Hæmoptysis was reported *before admission* in 264 cases, about 53 per cent. Osler records 60 to 80 per cent. This striking difference is due to racial, not climatic factors, as the majority of my European patients in India had early hæmoptysis. The chewing of betel nut may somewhat discount my figures, as betel juice may easily disguise the presence of blood in the expectoration. Hæmoptysis is not a very common cause of “sudden” death in the streets of London or Paris. In Bombay it is very rare. Rupture of an aortic aneurysm is very common. In the Morgue I found it twenty-seven times more frequent than death from tubercular hæmoptysis.

To judge by my cases tubercle bacilli are readily found in a much higher proportion of Indian than of European cases of tuberculosis. Indians of all ages are equally attacked. Acute forms are much more common in aged and middle-aged Indians than in old and middle-aged Europeans.

In Adult Indians Acute Tubercular Broncho-pneumonitis is very Frequent, though in Europe it mostly occurs in Children.—I cannot give you the relative proportion of tubercular to other cases of broncho-pneumonia in my practice as the true character of many was only recognized *post mortem*. Large numbers of such cases are brought to the Morgue by the police.

Many apparently Typical Cases of Lobar Pneumonia in Indians are Tubercular.—Pneumonia is the most common cause of death among the City Police in hospital. Some of these cases—clinically typical, fatal pneumonia—are in reality fulminating pulmonary tuberculosis. I plead guilty to first recognizing this fact through an accident. An apparently healthy constable aged 25, who never had a day's illness during his three years' service, was seized with rigor while on duty. He came at once to hospital. A sharp pain at the base of the right lung was found, due to a pleuritic rub. Two days later this was replaced by crepitation and dullness. All the signs and symptoms of a right basal pneumonia with slight implication of the pleura were present. On the fifth day he died comatose. His pupils were very contracted. This sign in association with the coma gave rise to a suspicion of opium poison. An autopsy was performed. No poison was detected. The right pleura, dotted with miliary tubercles, contained about 10 oz. of sanious serum. A mass of tuber-

cular glands was adherent to the root of the right lung. The inner coat of a large branch of the pulmonary vein adherent to the glands, was dotted with minute miliary tubercles. The whole of the right lower lobe was solid, sank in water and was in a condition of red hepatization, except that there were numerous islets of almost a normal colour. The brain and meninges showed no change beyond an excess of serum. This case was a revelation to me though it may be no new thing to most of my audience. Subsequently I recognized several similar conditions at the Morgue in bodies found dead by the police in the streets. I may add that ambulant lobar pneumonia is a very common cause of "sudden death." One is at first amazed that advanced cases should be able to continue at their work.

Bovine Tuberculosis is practically Non-existent in Bombay.—I have been so informed by four successive principals of the Veterinary College, three of whom said they never saw a case in Bombay. Two Inspectors-General of the Civil Veterinary Department informed me that tuberculosis scarcely existed among cattle in India. I have myself inspected hundreds of live cattle and examined hundreds of slaughtered cattle in my search for animal parasites and never saw even a suspicious case of tubercle. Few Indians drink unboiled milk. Butter is boiled and used as "ghi." In the light of these facts it is much to be regretted that the most prominent exhibits in the Museum of the Bombay Anti-Tuberculosis League and Sanitary Institute are exotic preparations and drawings of tuberculosis in cows, with warnings against the use of tubercular beef. Two-thirds of the population are Hindus. This is drawing a herring across the trail. It is as futile as warning against frost-bite in Bombay. The absence of bovine tubercle may be a factor in the acuteness of human tuberculosis in India. Every adult in England has probably swallowed millions of bovine tubercle bacilli and gained either immunity or a slight and temporary infection.

These experiments and observations show that in the Indian we have an organism with little or no acquired or hereditary immunity to tubercle, in whom the bacillus can flourish like seed in a virgin soil.

Causes of the Prevalence of Tuberculosis.—The Indian compares favourably with many races in cleanliness as far as personal ablution is concerned, but his expectorating habits are disgustingly evident to the least observant. The passages, walls, floors and stairways of almost every bank, office and public building in Bombay are fulsomely defiled with copious expectoration. I have seen an Australian lady land at the Gate of India eagerly anticipating a coral strand, become violently sick at sight of the reality—a pavement slippery with copious sputum. In my hospital, notices prohibiting promiscuous spitting under pain of fine or other punishment are affixed to every wall of every ward. A spittoon is placed at each side of each bed, and a spitting cup on each table or locker, yet the patients will spit on the bedding, walls and floor. A special pleasure is derived from expectorating from the upper balconies and windows; the prohibition of this pastime is resented as gratuitous "zulum" (oppression or injustice). The Bombay citizen, as all motorists know, will insist on walking in the centre of the road. We cannot blame him, as to walk on the footpath is to run the gauntlet of a shower of sputum and pan-stained saliva. When one enters the hut of a coolie suffering from any pulmonary disease, one finds the bedding, walls and floor rough-cast with sputum which the patient projects promiscuously. If the patient be a child or too feeble to clear his mouth, the mother, wife, or other attendant, picks the sputum from the patient's lips with her fingers, which she then flicks off anywhere, wiping her fingers on her *sari*¹

¹ A woman's dress.

or on the blanket. As she is cook-general, contamination of the family food is a certainty. The blanket as a rule is common property to the household, some members using it at night, others, who work in the mills or other night shifts, using it by day.

The Mosquito now comes in as an Important Factor in the Spread of Tubercle.—The well-to-do man uses a mosquito net which on an average encloses about 70 cubic ft. of stagnant and repeatedly inhaled air. The poor man covers his head with a blanket. The hotter and more stifling the weather, the more numerous and active are the mosquitoes. Those who have not seen the people sleeping on the pavement with their head and face completely covered by a blanket to keep off the mosquitoes, have probably seen the notorious photographs reproduced in French and German papers alleging that these rows of sleeping natives were victims dead of plague. At least a quarter of a million citizens sleep in the hot weather with their respiration thus impeded. Remove the blanket and you will find the coolie drenched in sweat, as a result of the high temperature combined with partial asphyxiation. When one reflects that the little air that reaches the coolie's lungs has to filter through a blanket often stiffened by tubercular sputum, the marvel is that any escape infection. Get rid of the mosquito and the coolie will sleep with his face uncovered. It is almost as important to check the breeding of *Culicine* as of *Anopheline* mosquitoes.

At most seasons of the year the air of Bombay is laden with dust in which the expectorating customs of the people ensure the presence of an appreciable proportion of dried sputum. The side-walks are fouled with the faeces of dysenteric and tubercular children. Many of the best residential quarters have no water-closets and are only provided with commodes. The character of the dust may be gauged by the fact that on three occasions when my examination of blood films for malaria was interrupted, on resuming my search I found an egg of *Trichocephalus dispar* on the dusty slide. Dust is seldom allowed to settle but is constantly stirred up by municipal sweepers brushing some of it to the edge of the path whence it is again scattered by the wind. After some time the sweepings are placed in an open cart which is left at the roadside for a period varying from a few hours to a few days for dogs, cats, crows, kites and children to investigate. Ultimately a pair of bullocks is harnessed to each cart. The load being largely composed of dried dust, dried dung, used toilet and other paper, is absurdly light, even at the start, so the driver urges the bullocks to their utmost speed. The roads are bumpy, the carts without lids or springs. It is difficult to conceive any device except a giant pepper-castor better adapted than these galloping carts for re-sprinkling the dust over the city. Throughout the day strings of fifty or more carts may be dimly seen through their halo of dust, careering through the streets to a dumping ground about four miles from the centre of the Fort. This refuse should be sterilized by burning at numerous depots or incinerators. In this way one cart would do all the work and none of the mischief which ten now do. Lids should be placed on all carts. Removal should take place at night when the streets are comparatively empty. Motor lorries would obviate the stabling of thousands of bullocks in the city.

The European housemaid wipes dust off books and furniture, the Bombay *hammal*¹ never does so. He flicks it into the air with flaps of his duster so that the dust in the air is repeatedly renewed. A few minutes after he has

¹ Generally, a porter; in Bombay City, a "male housemaid."

performed this task you can trace letters and figures with your finger in the dust settled on a table.

Overcrowding is unavoidable in a city built like Bombay on a small island, but many buildings, such as jails, almshouses, orphanages, *pinjrapols*¹ and sanatoria, which occupy valuable space, would be better outside the city. The Anti-Tuberculosis League founded its sanatorium about three years ago in a densely populated part of the city.

Government has during the past fifteen years built good, sanitary quarters for most of the police, yet the ingrained dislike of the Indian for sunlight and fresh air, combined with the *purdah* spirit, has neutralized these sanitary efforts. These well-built quarters look as dirty and disreputable as any coolie *chawl*.² Open verandahs or balconies were constructed on each floor, but the occupants have enclosed them by boards, sheets of tin, mats, and old pieces of sacking, so that sunlight and air are excluded.

The high rents prevailing in the city tempt the constable to increase his income by taking in as many lodgers as can find sleeping space. He bluffs his officers by describing the lodgers as relatives.

¹ Jains and many Hindus have religious scruples against taking the life of any animal, even of a mosquito, a flea or a louse. The pious and wealthy endow certain enclosures or collections of cages known as "pinjrapols," in which animals suffering from old age, incurable diseases or wounds, are enclosed till death ends their misery.

² A large, poor-class tenement house.

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SECTION OF UROLOGY.

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Section of Urology.

President—Sir PETER FREYER, K.C.B., M.D., M.Ch.

Genito-urinary Symptoms in Acute Appendicitis.

By V. ZACHARY COPE, F.R.C.S.

THE simulation of appendicitis by urinary disease is well known: for example, a case of right-sided pyelitis, or stone in the right ureter, has been operated on with a pre-operative diagnosis of appendicitis. The converse mistake is more rarely made, but though in the majority of cases of appendicitis the diagnosis is clear, and any genito-urinary symptoms are subsidiary, it is certain that when such symptoms arise mistakes may be made even after careful examination.

The following symptoms, which generally point to disease of the genital or urinary organs, may occur in acute appendicitis:—

- (1) Frequency of micturition.
- (2) Pain on micturition.
- (3) Retention of urine or difficulty in urination.
- (4) Tenderness on pressure in the right erector-costal angle.
- (5) Pain in the right, left testicle, or both testicles.
- (6) Retraction of the testicle.

The first three of these seldom give rise to any confusion, though they are of great significance in diagnosis.

Frequency of urination is not uncommon, and usually results from direct irritation of the bladder by the peri-appendicular inflammation. When the appendix lies in the pelvis there is usually no rigidity of the abdominal wall, and the combination of hypogastric pain, a lax abdomen and frequency of micturition might mislead a cursory observer into thinking the bladder the source of trouble. One of my patients suffering from appendicitis, who had frequency of urination, stated that he felt as if the bladder were full, but there was no pain on performing the act.

Retention of urine, which is said by Deaver to be due to irritation of the bladder sphincter, is uncommon, but difficulty in micturition is an occasional complaint.

Pain on micturition is quite common, and may accompany inflammation of an appendix situated either behind the cæcum, in the iliac fossa, at the right brim of the pelvis or deep in the pelvis. The appendix need not be perforated for this symptom to be produced. The reason for this pain is most likely to be sought in the proximity of the inflamed appendix or of inflammatory exudate either to the pelvis of the kidney, the ureter, or the bladder.

Sometimes frequency of micturition is combined with pain on urination and tenderness in the right loin, and then the symptoms may closely resemble those of pyelitis.

2 Cope: *Genito-urinary Symptoms in Acute Appendicitis*

The differential diagnosis is usually easy if the urine be examined carefully and the following points compared as in the following parallel table:—

ACUTE PYELITIS.	APPENDICITIS.
Initial rigor common.	Initial rigor unusual.
Temperature 103° F. or more.	Temperature seldom so high as 103° F.
Abdominal muscles usually lax.	Abdominal muscles sometimes rigid.
Pus or bacteria in urine.	Urine normal.

Tenderness in the right erector-costal angle is frequent in disease of the right kidney or perinephric tissues, but in cases of appendicitis also there is commonly pain in the same region. This does not always signify a retrocaecal appendix, for the pain can sometimes be elicited in patients whose appendix is not lying on the quadratus lumborum. It is quite likely that such pain is due to a hyperaesthesia of the posterior area supplied by the tenth dorsal spinal nerve, which on clinical grounds I believe to be the segment which supplies the appendix.

When a perforated appendix lies upon the quadratus lumborum pain is much greater on pressure under the rib-margin posteriorly. The simulation of an acute perinephric abscess is sometimes very close, and, indeed, a perinephric abscess may be a consequence of perforative appendicitis when the appendix is retrocaecal. Unless the history is noted very carefully, the observer may take such an abscess to be of renal origin. The mistake is more likely to be made if any other genito-urinary symptoms are present.

Testicular Symptoms in Appendicitis.—So far as I know, testicular symptoms in appendicitis have only been casually noticed in even the most exhaustive treatises on appendicular disease. Most writers do not seem to be aware of its occurrence, important though it is, for wrong diagnoses may result from lack of appreciation of the symptom.

Testicular pain or discomfort occurs in probably about 5 per cent. of cases of appendicitis in the male. The pain is often not so severe as the general abdominal pain, and the patient may not complain of it unless directly questioned on the point. It is either of a dull aching or of a sharp shooting nature.

CASES OF APPENDICITIS WITH TESTICULAR SYMPTOMS.

Case I.—Medical man, aged 27, known to have mobile right kidney, seized one morning with rather vague pain in hypogastrium and dull pain in testicles. No vomiting, but acute loss of appetite and nausea. Flatulence and discomfort experienced in right renal region. Patient thought he was suffering from Dietl's crisis; this opinion was shared by surgeon who saw case within eight hours of onset. After rest in bed symptoms abated, abdominal belt with special renal pad ordered, and patient allowed to go away to seaside. A week later, patient noticed rather tender lump in right iliac region and felt renal pad irksome. Thinking kidney might have become displaced returned to town, where his temperature was found to be 101° F. Soon after, large appendicular abscess was successfully drained. Patient later extruded concretion from sinus left after drainage. When appendicectomy was undertaken a month later, only stump of appendix was found attached to caecum.

Case II.—B. S., male, aged about 30, taken with vague right-sided abdominal pain May 12, 1914. During night pain became much worse and he vomited. When I saw him at 6.30 a.m. on 13th, pain found on pressure at right erector-costal angle, and some rigidity of anterior abdominal muscles on right side. Complained that pain went down into right testicle and that the day before urine had been dark coloured. Provisionally, I diagnosed renal colic, gave morphine, and arranged for X-ray photograph to be taken. Radiogram showed shadow just external to line of ureter which radiologist said

was probably calcareous gland. In view of symptoms I thought this might be stone in ureter, and after cystoscopy explored right ureter by open incision. Ureter quite normal, but on opening peritoneum at anterior part of wound I removed acutely inflamed appendix, non-adherent and unperforated, with a calcified gland size of hazel nut embedded in appendicular mesentery.

Case III.—Mr. G. History of five days' illness with abdominal pain, vomiting, fever and abdominal distension. No trouble with micturition, but had some pain in *left* testicle. I opened large pelvic abscess on sixth day of illness.

Case IV (not under my care).—H. C., aged 26. History of abdominal pain, first epigastric, then right iliac. Pain also radiated to right testicle. Neither vomiting nor nausea. Two days later, pain still in right iliac fossa and right testicle. Operation revealed gangrenous appendix in right iliac fossa.

Case V.—Mr. D., aged 58, at 11 a.m. on morning of February 20 this year, felt severe pain in left testicle. For an hour and a half this pain was unaccompanied by any other symptoms, but at 12.30 p.m. there ensued generalized abdominal pain. Later, pain settled in right iliac region. There was anorexia but no nausea, nor vomiting. Two days later when I saw him there was definite evidence of abscess formation in right iliac fossa. I opened abscess, and removed perforated appendix lying on ileo-psoas muscle. Concretion in appendix and appendicular mesenteric vessels thrombosed.

In all these cases the testicles appeared normal on examination.

I have seen at least two other cases, but these five serve to illustrate the main facts, which may be summarized as follows:—

- (1) Pain in either right or left testicle, or in both testicles, may accompany acute appendicitis. This pain is due to the appendicular disease and is not dependent on any disease of the genito-urinary system.
- (2) Though the testicular pain is easily associated with perforation of the appendix it may accompany appendicitis without perforation (Case II).
- (3) The testicular pain may precede the onset of the abdominal pain by at least an hour or two, though this is unusual.
- (4) The duration of the pain may be short or as long as two days.

Cause of the Pain.—Whilst it is tempting to suggest that direct irritation of the sympathetic nerves accompanying the spermatic artery may account for the testicular pain, yet the fact that the pain may be on the left side and may occur with an unperforated appendix negatives that explanation for at least some of the cases.

The sympathetic nerve supply of the testicle is from the tenth spinal cord segment, whilst fibres from the first lumbar go to the cord and cremaster. There is clinical evidence that the appendix is supplied chiefly from the tenth spinal cord segment. May not the testicular pain be explained most easily as a pure referred pain, since the appendix and testicles appear to be supplied by the same cord-segment?

When a pelvic abscess has formed, possibly direct irritation of the vasa deferentia or of the seminal vesicles might cause testicular pain.

Retraction of the Testicle is occasionally noted in appendicitis. This may be due to irritation of the genito-crural nerve causing a contraction of the cremaster.

As evidence of the importance of the testicular symptoms in appendicitis, one need only refer to the first two cases, in which renal conditions were diagnosed in consequence of the pain in the testicle. In one of these the correct diagnosis was not made till a large abscess was detected a week after the onset of the illness.

A New Posterior Urethroscope.

By J. SWIFT JOLY, F.R.C.S.

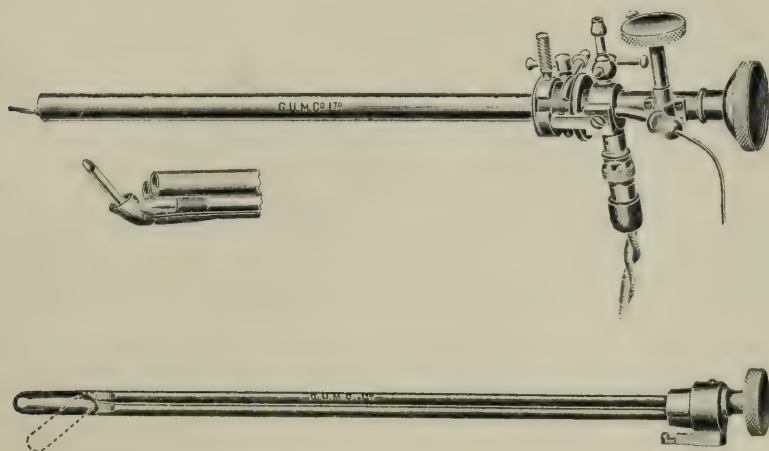
THIS instrument is a modification of the Geiringe urethroscope, but the modifications incorporated in the design are so numerous and important that it is practically a new instrument.

It consists of a straight tube, 18 cm. long, provided with an obturator, to enable it to be passed easily. The inner end of the obturator is rounded off, and projects about $1\frac{1}{2}$ cm. beyond the tube. By means of a concealed screw, actuated by a milled head in the handle, the projecting portion of the obturator can be tilted up through any angle, up to one of 35° , with the axis of the tube. This variable tilt is of great service, as it can be adjusted to each patient, and enables the instrument to be passed with the minimum feeling of discomfort. The handle of the obturator also carries a catch which engages with an interrupted screw on the expanded outer part of the urethroscope tube. When the obturator is in place, a slight lateral movement of a milled arm attached to the interrupted screw locks it firmly in position. This type of interrupted screw lock has the advantages of simplicity, durability, and ease of manipulation, and as any wear is automatically taken up, it always holds the two parts of the instrument firmly together.

The urethroscope tubes are made in two sizes—22 Charrière for observation only, and 26 for the operating instrument. As the same telescope fits both instruments, a combination set has been made up.

The operating urethroscope will be described first. When the obturator has been removed, a male fitting consisting of four smaller tubes is inserted in its place, and locked by means of the interrupted screw described above. These tubes are the telescope, the light holder, the water conduit, and the instrument holder. The three latter are soldered together, and lie above and to either side of the telescope, which is simply held in position by friction, so that it can be removed and used with the observation instrument. The telescope is of the straight variety, and is focused for near objects. The image is fully corrected, and in spite of the small size of the telescope (12 Charrière) it is bright and sharply defined. The lens system is of the usual type, giving two reversals of the image in its passage down the tube, and the makers are to be congratulated in producing such a large field of view and such a bright image through so small a telescope. The light carrier consists of a small electric bulb mounted on the end of a special tube carrying the insulated wire. The bulb is brought right down to the end of the urethroscope tube, and is only just outside the field of vision of the telescope. The light is thrown directly forward, so that the portion of the urethral wall under observation receives the maximum illumination. The bulbs are mounted in a small holder, which screws on to the end of the light carrier, so that they can be changed in a few seconds. On the opposite side of the telescope is the water conduit. Water is by far the most satisfactory medium for distending the posterior urethra during observation, and this part of the instrument has been very carefully designed, as most of the failures in posterior urethroscopy are due to defective water circulation. In this instrument the water conduits are unusually large, and the incoming stream is directed against the portion

of the urethral wall under observation. On account of this a clear picture can be obtained even in cases where the mucous membrane is inflamed and bleeds at the slightest touch. The incoming water is carried by a long tube down to the end of the telescope, and the amount of the flow is regulated by a tap in the handle of the instrument. The return flow travels back between the main tube and the inner fitting. The outlet is also controlled by a tap. By this means the flow of water can be controlled to a nicety by the operator. As the external lens of the telescope is plane, the image is just as clear under air distension as under water. If the surgeon prefers the former medium, the bellows can be attached to the inlet tap, while the outlet is closed. The operating attachment consists of a tube sufficiently large to carry an instrument of the calibre of a No. 6 Charrière easily. The terminal part of this tube is hinged, and its position is controlled by the large milled head in the handle of the urethroscope. This hinged portion of the tube takes the place



of the Albarran beak in directing the instrument, but has decided advantages over the older method, as the instrument is always held securely and cannot slip out of the field of vision, as so often happens when the Albarran beak is used. In addition, the movement imparted to the instrument is positive in two directions, while that imparted by the Albarran beak is only positive in one. The extreme end of the hinged portion of the instrument carrier is bevelled off like the mouthpiece of a flute, and as it just comes into the field of vision of the telescope the instrument is always under control of the surgeon's eye. This operating attachment was primarily designed for use with a fulguration electrode, but can be used with any flexible instrument.

The observation urethroscope is similar to the operating pattern as far as the telescope, the light holder, and the water conduit are concerned, but the main tube is only 22 Charrière. This is, I believe, the smallest posterior urethroscope on the market. A detailed description of the instrument is unnecessary, as it follows the lines of the operating instrument, and all its essential parts have already been explained.

The following advantages are claimed for this urethroscope: (1) It is light, handy, and practical: (2) it gives good illumination and excellent definition:

(3) the improved irrigation system allows a thorough examination of the posterior urethra, even under the most disadvantageous circumstances ; (4) the technique of minor operations, such as fulguration of papillomata, &c., in the posterior urethra, is very much simplified ; (5) it is aseptic, as the main tube and the obturator, which are the only parts of the instrument to touch the urethral wall, can be sterilized by boiling.

This instrument is made by the Genito-Urinary Manufacturing Co., 66, Margaret Street, W.1, who deserve the greatest credit for the exact and careful workmanship they have put into it.

Section of Urology.

President—Sir THOMAS HORDER, F.R.C.P.

DISCUSSION ON TESTS OF RENAL FUNCTION.

Mr. JOHN EVERIDGE.

(ABSTRACT.)

SINCE it was arranged to hold this discussion, I have endeavoured to observe the co-relation existing between the clinical aspect and such results as certain of the renal function tests have yielded. The tests which have particularly been brought into use are three: (1) Dye excretion; (2) forced urea excretion; (3) urea retention (blood urea).

The total number of tests carried out at King's College Hospital is in the neighbourhood of 600, of which the majority were in cases of a surgical nature.

(1) *Dye Excretion.*

Two dyes are in constant use, phenol phthalein and indigo-carmin. By the former, a quantitative, and by the latter, a qualitative estimation of the renal function is attempted. Indigo-carmin is never used for a test of total function, its rôle being to give some idea of the relative working of each kidney. Phenol phthalein lends itself to a more accurate estimation, but difficulty of technique, e.g., estimation of percentages in higher dilutions, embarrassment from urinary pigments, &c., and the secreto-inhibitory effect of ureteral catheterization, deprive it, in our opinion, of the foremost position among the tests.

In a series of cases where the figure of *total* renal function was required as a preliminary investigation before operation on the lower urinary tract (chiefly prostatectomies), the percentage of accurate results may be given as 66. In one case in which the result was inaccurate, the numbers were given as: (a) "Colour came through in five minutes"; (b) "36 per cent. in the first hour." This patient died three days after the second stage of a two-stage prostatectomy, and at the post-mortem very little renal tissue was present, there being generalized degeneration of the tubules, and fibrosis of the interstitial tissue.

In tests for *relative* kidney value it may be said that in 54 per cent. of cases the tests were accurate, so far as they fell into line with clinical phenomena and the alternative renal function tests. In 30 per cent. the test results were doubtful, i.e., either the latent period (time elapsing until pigment appeared in the urine) was correct, and the excretion in the first and second hours incorrect, or vice versa. And in 15 per cent. the test results were apparently quite misleading.

In one case of unilateral hydronephrosis, which I proved to exist by pyelography and at a subsequent operation, the colour came through the affected kidney in ten minutes, while from the opposite kidney, which all clinical features tended to show was normal, no dye made its appearance in the first half-hour.

(2) *Urea Concentration Test.*

This is a delicate test, and appears to demonstrate adequately increasing or decreasing renal efficiency in a remarkable way. At King's College Hospital we have come to put increasing reliance on it, and to regard it as an essential part of our armamentarium.

There is a class of case, some 6 per cent. of our series, often puzzling, namely, that in which a high concentration of urea in the urine occurs with advanced deficiency, where the urea appears to leak through the kidney, or is forced, as it were, by the high "pressure head" of the overcharged blood, as MacLean has suggested (*Lancet*, June 19, 1920). The necessity therefore arises for doing a blood urea estimation at the same time, even, perhaps, while the urea concentration test is being carried on, in order that precisely the same conditions of metabolism and environment may obtain in both, due allowance being made in the latter case for the excess urea in the blood resulting from the meal.

In a case of mixed nephritis, upon which my senior colleague, Sir John Thomson Walker, performed double decapsulation, we obtained the following figures, which demonstrate how the urea concentration in the urine varies according to the blood urea:—

	Urea concentration	Blood urea
January 24, 1921	1.9	87½
February 8, 1921	1.4	66
April 30, 1921	1.24	62
June 3, 1921	1.83	240

In this case, as in others we have observed, it is of interest to record that the clinical condition varied rather as the urea concentration figure than as the blood urea. Our figures show too, that this class of case is a better operation risk than would have been imagined from a mere examination of the blood urea alone.

Three examples of cases upon whom I operated with satisfactory results, may be cited, all of them being feeble old men:—

Operation	Blood urea	Urea concentration
Prostatectomy	57	1.93
Prostatectomy	59	2.26
Suprapubic lithotomy (secondary to prostatectomy three years before)	131	2.14

Where the urea concentration figure is very low it appears dangerous to carry out even the most trivial intervention in the urinary tract. Thus in one case, admitted to hospital for severe chronic cystitis, the urea concentration was 1.29. I cystoscoped under local anaesthesia, which worked very well, and the patient was in no way distressed. He died two days later in typical uraemia, and at the post-mortem one kidney was found to be completely destroyed, and the other to contain numerous caseous deposits.

Regarding the *technique*, we withhold all fluids for twelve hours before the test, and give half a pint of water immediately after the draught. We now collect specimens of urine at the end of each of the three hours following the meal, for, in quite a large percentage of cases the highest concentration occurs in the third hour; further, diuresis which is promoted by the urea, as a rule has passed off by the third hour.

Although the test is not infallible, yet provided that most careful precautions are taken in the collection of all urine passed, and that the test is controlled by a blood urea estimation, it appears to warrant a very considerable degree of confidence. It is unfortunate that it cannot be carried out in many cases in which there are digestive disturbance, vomiting, &c.: or with accuracy where a suprapubic drainage has been established.

Has any Fellow yet introduced the urea intravenously instead of by the mouth, as McGasky suggested several years ago?

A final point to note is the increased urine urea concentration after nephrectomy, well exhibited by the urea meal test. In some cases the maximum occurs

in the first week and then subsides to the region of the normal or pre-operation level. In others the maximum does not arrive until the third week. These well-known facts are generally ascribed to an over-action of the sound kidney resulting from the removal of the inhibiting effect of the diseased kidney. Possibly, however, the phenomenon may be pathological and comparable to the conclusions arrived at by Rose Bradford, in his experiments on nephrectomized dogs. In the *Journal of Physiology*, 1899, it will be remembered, he pointed out that after removal of one kidney, shortly followed by the removal of half the remaining kidney, a diuresis occurred and the output of urea was increased. This was ascribed to excessive nitrogenous waste, possibly from loss of an internal secretion of the kidneys. The animals died in a week. Evidence supplied by the other function tests would be of value. I have not been able to demonstrate increased functional activity after nephrectomy by the phtalein test. Miller and Cabot (quoted by Brasch in *Surgery, Gynæcology and Obstetrics*, 1917, v, p. 29) have observed, in rare cases, an increase in phtalein excretion after operations other than those on the urinary tract. In the majority a diminution is found. Have those working on the diastase content test, or tests other than the urea concentration test, evidence to bring forward that the increased urea output is a true expression of increased functional activity?

(3) *Blood Urea Estimation.*

Blood Urea Estimation.—With regard to the value of this test, it has been amply proved that, in apparently any individual, the blood urea is a quantity variable through wide limits according to environment and metabolism in health and disease. Our attention was drawn to this by noting the marked increment in the blood urea during the few hours after the urea meal administered for the urea concentration test. To give a few examples:—

	Urea concentration	Blood-urea	
		Before urea meal	After urea meal
(1) Chronic nephritis ...	1.83 ...	67 ...	105
(2) No urinary disease ...	4 per cent. ...	27 ...	66
(3) Carcinoma of the bladder ...	2.8 ...	34 ...	70
(4) Vesical calculus, probably chronic interstitial nephritis	2.4 ...	73 ...	131
(5) Adenoma of prostate ...	2.26 ...	44 ...	59
(6) Hypernephroma of kidney (before nephrectomy) ...	2.66 ...	92 ...	133

(in milligrammes per 100 c.c. blood)

When alluding to the urea concentration test, I referred to the fact that a high urine urea may occur with a high blood urea, and it follows then, that either test may be misleading, unless controlled by the other. In other words the two tests are interdependent and complementary, and perhaps we should go further and regard them as two parts of one test. This precaution is not so much required where the blood urea is very high, anything over 130 mgr. ; but in those cases in which it is found to be 40 to 90 mgr., and the majority of candidates for prostatectomy fall into this group, the combination test will provide a far more accurate assessment of renal efficiency.

It may, therefore, be said, that the blood urea estimation fulfils two important functions:—

(1) As an adjunct to the urea concentration test by eliminating an abnormally high "pressure head" of blood urea, which may be causing a high urine urea.

(2) In distinguishing uramia from diseases which present clinical features indistinguishable by the ordinary bedside examinations.

It does not appear that in practice, conditions of life can be so accurately arranged as to enable us to stabilize in *any one patient* his blood urea content in order that the changes through narrower limits can be depended upon to give reliable information as to the finer variations in renal function. I am inclined to think, therefore, that the blood urea estimation, as a separate entity, practically only decides between very efficient and very deficient renal function.

Mr. J. SWIFT JULY.

(ABSTRACT.)

The "urea concentration test" has been very extensively used at St. Peter's Hospital during the last two years, but I shall only consider the results obtained in cases submitted to it since July 1, 1921.

(1) *Enlarged Prostate.*

In four months sixty-four observations were made on cases of enlarged prostate; of these twenty-six gave a concentration of 2 per cent. or over, eighteen of from 1.5 to 1.9, thirteen of from 1 to 1.4, and seven under 1 per cent.

(1) Taking the twenty-six cases giving a concentration of over 2 per cent.: The prostate was removed in nineteen, with two deaths. Suprapubic cystotomy was done on the remaining seven with one death. One death after prostatectomy was due to pyelonephritis, the other to shock. The death after the cystotomy was in a case admitted suffering from pyelonephritis, with a widely oscillating temperature. Before operation he gave a urea concentration of 3 per cent.

(2) Eighteen cases giving a concentration of between 1.5 and 1.9 per cent.: Prostate removed in thirteen cases, with one death. Suprapubic cystotomy in five cases. The death was not due to renal deficiency.

(3) Thirteen cases giving a concentration between 1 and 1.4 per cent.: Five prostatectomies with one death, due to pyelonephritis. Six suprapubic cystotomies. Two cases, no operation.

(4) Seven cases giving a concentration under 1 per cent.: Four prostatectomies with no death. Two cystotomies. One case, no operation.

From these figures it is very difficult to draw any conclusion, but it is obvious that prostatectomy can be performed safely with a urea concentration of very much less than 2 per cent. Of the nine cases giving a concentration of less than 1.5 per cent., four showed definite signs of pyelonephritis during convalescence, one of them dying of this complication. My opinion is that with a concentration of 1.5 per cent. or over there is little risk of renal failure after a prostatectomy, but there are a considerable number of cases in which the prostate can be safely removed, the patients giving a much smaller percentage concentration than this.

In addition to observing the urea concentration, the quantity of urine passed in the second hour after the urea draught was carefully noted, and the amount of urea passed was also calculated. It was found that all the cases that did badly, in spite of giving a high urea concentration, gave a very poor diuresis; while those that did well, in spite of a low concentration, gave a good diuresis. For example, the only case that died of polynephritis with a concentration of over 2 per cent. gave the following figures: Concentration, 2.7 per cent.; quantity, 1 oz.: amount of urea passed in the second hour, 12 gr.: while the following are typical examples of cases doing well in spite of a low concentration:—

Percentage	Amount of urine	Grains of urea
0.9	10 oz.	39
0.75	8 oz.	27

On account of figures such as these I now attach much more weight to the amount of urea passed in the second hour than to the actual percentage, and I think that if 20 gr. or upwards are excreted during this time the prognosis after operation is good.

However, if the test is used in the manner I have just described, there are certain precautions that must be observed. The patient must be on a known diet, and no food or drink allowed during the duration of the test. At St. Peter's Hospital the patients are not tested until they have been on the hospital diet for forty-eight hours. The patient is given a standard breakfast at 8 a.m., he has the urea draught at 9.30, and the specimen is collected by means of an in-dwelling catheter between 10.30 and 11.30.

Lastly, where this test appears to be in opposition to the clinical picture presented by the patient, we have always put more faith in clinical observation than in the result of any test of the renal function.

(2) *Diseases of the Kidneys.*

In these cases the urine has been drained off by catheterization of both ureters during the second hour after the urea draught has been administered. In most cases the quantity of urine passed has not been measured, as the catheters have only been left in position long enough to obtain a satisfactory sample.

In cases of renal calculus there has always been a distinct lowering of the concentration on the diseased side. With a small stone in the renal pelvis this is but slight, a typical case being: Diseased side, 2 per cent.; healthy side, 2'4 per cent. The power of concentration diminishes with the amount of destruction to the kidney.

Hydronephrosis.—Three typical examples:—

Case I: (a) Infected Hydronephrosis.—Diseased side: Quantity, 13 c.c.; percentage, 0'3; microscope, much pus. Healthy side: Quantity, 34 c.c.; percentage, 3'6. microscope, some blood, no pus.

Case II.—Diseased side, 0'65 per cent.; healthy side, 2 per cent.

Case III.—Diseased side, 1'2 per cent.; healthy side, 2'4 per cent.

(b) *Renal Tubercle.*—In these cases only the healthy ureter has been catheterized, and in every unilateral case the healthy kidney gave a concentration of over 2 per cent.

In healthy cases where both ureters have been simultaneously catheterized, a marked difference of the diuresis from the two kidneys has not been noticed, so that I think the urea concentration gives a fair idea of the functional activity of the two organs.

Professor H. MACLEAN

(St. Thomas's Hospital).

(ABSTRACT.)

In endeavouring to come to some conclusion as to the best methods for estimating renal efficiency, it is perhaps sometimes forgotten that given efficient methods, the simpler the procedure the more helpful is it likely to prove. With plenty of time and a well equipped modern laboratory, it is quite possible to arrive at a definite conclusion as to the functional activity of the kidneys in every case without exception. Few, however, are the cases in which an elaborate investigation is required, and the more experience one has in estimating renal efficiency the more obvious does it become that lengthy, elaborate and complex procedures are not required.

Directly or indirectly I have tested altogether over 14,000 patients for renal efficiency; a certain number of these cases were associated with genito-urinary complications, but the majority were subjects who had contracted nephritis in the Army, and who came under my notice in connexion with work done for the Ministry of Pensions. As the result of this experience it appears to me that the following tests are quite sufficient to determine the condition of the kidneys in the great majority of patients.

- (1) Estimation of blood urea.
- (2) Urea concentration test (MacLean and de Wesselow).
- (3) Relation of blood urea concentration to concentration of urea in urine.

Besides these tests many others have been employed from time to time, and I have always made use of the diastatic test as well, but though this test is of some value in a general way, especially in cases of interstitial nephritis, it has not proved helpful in my hands when dealing with patients whose renal efficiency depended on genito-urinary lesions such as stricture or enlarged prostate. Indeed, for such cases the test might be discarded altogether. At one time or another I have employed practically all the known tests for renal inefficiency as well as many modifications that have never been published, but have found the majority of them sadly lacking. I have now come to the conclusion that the three tests suggested above are all that is required, at any rate for the class of cases under discussion to-night. It is far better to take up a certain number of tests and learn to appreciate their limits and their possibilities rather than flutter about from test to test without really ascertaining what the results mean or how they should be correctly interpreted. Again, it is most important to remember that no single test is *per se* of much value in all cases, and when there is any doubt, all the tests done must be considered in relation to each other. At the same time the clinical condition of the patient must never be forgotten in arriving at a conclusion.

(1) *Estimation of Blood Urea.*

In estimating the blood urea, it is important to have some standard as to the average normal value. The tendency of the American school is to place the average concentration of blood urea at about 20 or 25 mgr. per 100 c.c. blood. This may be all right in the case of young individuals, but it is far from representing the average in subjects well advanced in years. Frequently a man aged 65 or 70 with quite normal kidneys for his age will show a blood urea concentration of 40 to 45 mgr. per 100 c.c. or more, and perhaps one might even take 50 mgr. as the maximum in such cases. Since the majority of patients suffering from such conditions as enlarged prostate and other genito-urinary lesions are old men, this observation is very important. When the blood urea reaches 75 to 80 mgr. per 100 c.c., then, *under ordinary conditions*, it is of itself direct evidence of renal inefficiency, but values of 50 to 60 mgr. or so may or may not be of much significance and must be interpreted in the light of the other tests to be discussed, i.e., the urea concentration test and the urea concentration factor.

Again, blood urea depends largely on the amount of food taken, and a patient with dangerously inefficient kidneys may show quite a low blood urea. In a patient under my care suffering from interstitial nephritis and showing signs of uræmia, the blood urea was reduced from 180 mgr. to 30 mgr. per 100 c.c., yet the patient died of uræmia. This result proves incidentally that urea has nothing to do with uræmia. The blood urea concentration is merely an index of renal efficiency: urea in the body in large excess of the normal seems to have little or no deleterious action.

In this connexion it must not be forgotten that various conditions besides renal inefficiency will produce an increase of blood urea. If, for instance, the heart is acting badly the blood urea may be comparatively high, but this is easily explained by the fact that the blood pressure is not sufficient to produce the normal amount of urine. The kidneys may be healthy, but one of the factors necessary for excretion is inoperative, and so urea and other products are retained. In such conditions as very severe diarrhoea, it is not uncommon to find large amounts of urea in the blood. Here, again, the mechanism is obvious, most of the body fluid being passed by the bowels and not by the kidneys. In such cases the blood urea alone is a very false guide, but if we estimate the amount of urea in a sample of urine it will be found to be high in direct contrast with what we find in renal deficiency. Various other extrarenal factors produce the same result. The importance or otherwise of increased blood urea is easily ascertained by correlating the result with the other two tests discussed below. When blood urea concentration alone is relied on as evidence of renal inefficiency it is quite possible to make very gross mistakes, for an apparent reduction in blood urea after, say, a preliminary cystostomy with drainage, might actually be dependent on a change of diet and the renal condition might be really worse than it was before.

(2) *Urea Concentration Test.*

The urea concentration test should be done in all cases if the local conditions are such that samples of urine can be obtained. The test is so simple that it can be carried out by any practitioner and frequently no other test is necessary. The patient empties his bladder and receives immediately afterwards by mouth 15 grm. of urea dissolved in about 100 c.c. of water. If possible, no fluid should be taken six to ten hours before the test. After one hour the bladder is emptied and the sample measured and preserved for urea estimation. The same process is repeated in another hour (two hours after receiving the urea) and if necessary a third sample may be taken at the end of a third hour. Generally the second sample shows the maximum concentration of urea, and if this is over 2 per cent. the kidneys may be taken to be fairly efficient. If the urea in any sample exceeds 2.5 per cent. or so, it may be taken for granted that the kidneys are efficient for all practical purposes and that the blood urea is normal in amount. In such a case no further investigation is necessary. If the concentration is in the region of 2 per cent. there is also little chance of increased blood urea concentration being present, or if there is, it is not marked and is of little account. It is, therefore, possible to state in a general way that when the urea concentration test gives a result over 2 per cent. the kidneys are fairly efficient; the higher the concentration of urea the better is the renal condition. When the urea concentration test gives a figure decidedly below 2 per cent. the kidneys are not likely to be quite efficient, and when the result is in the region of 1.5 to 1 per cent. or lower it may be taken for granted that a considerable defect exists, especially in cases with values in the region of 1 per cent. It is very important however to note that values below 2 per cent. may be caused by diuresis, so that, on the whole, not more than 120 to 130 c.c. of urine should be passed during the hour the specimen examined is being secreted. If more than this is passed, and if the concentration is nearly up to 2 per cent., a certain allowance may be made, but if the concentration is lower than this (say 1 to 1.5 per cent.) and diuresis has been a marked feature, the test should be repeated. If when made a second time the test fails to give a specimen of urine containing up to 2 per cent. urea, it may be assumed that the renal condition is not very satisfactory.

(3) *Urea Concentration Factor.*

One of the principal functions of the kidney is to produce a fluid which contains very much more urea than is contained in an equal volume of blood. Thus, 100 c.c. of the blood of a normal young individual contains say 20 mgr. urea, while 100 c.c. of the urine will contain probably about 1,600 mgr. (i.e., 1.6 per cent. urea). It is obvious that in such a case the concentration in the urine is eighty times the concentration in the blood. This factor (referred to as the urea concentration factor) is a most important one. In obtaining the factor it is best to empty the bladder and then after an hour to pass water. Immediately after passing urine a sample of blood is taken. Theoretically, it would be better to take the blood about the middle of the hour, as this would allow for possible variations of the urea in the blood, but, unfortunately, in nervous patients especially, the slight operation necessary to obtain blood may cause a diuresis and so upset the result. The urea concentration factor is obtained from the formula—

$$\frac{\text{milligrams of urea in 100 c.c. urine}}{\text{milligrams of urea in 100 c.c. blood}}$$

and should normally be from about 60 to 80. When the renal efficiency is somewhat impaired it may be only 30 to 40; if below 20 the condition is generally bad, and if below 10 it is usually severe. I have seen a factor of rather less than 2 in a very severe case immediately before death.

Various modifications of this factor in connexion with the urea concentration test have been worked out in my laboratory and American workers have published some observations bearing on this point. In my experience, however, the ordinary urea concentration factor as described here, when used in conjunction with the other tests, is all that is necessary, and prevents certain complications inherent in a correlation of blood urea estimation with the urea concentration test.

Besides the above, the phenol red test is in extensive use, but has not proved very successful in my hands. It is easy to get results, but difficult to be certain that these results are at all correct. No doubt under certain conditions it is very useful, but in genito-urinary cases where blood is so often present in the urine the test cannot be used. With care no doubt it may frequently be useful, but it is certainly more difficult to carry out satisfactorily than the other tests mentioned, and when these tests are done the phenol red method is unnecessary.

The three tests described have been used as a routine method on all cases of genito-urinary surgical conditions at St. Thomas's Hospital. The results have been quite satisfactory and several of the statements made above are based on the investigation of these cases. When the renal condition was passed as "efficient" no post-operative death took place as the result of renal inefficiency. In the very few cases that ended fatally the causes of death were found post mortem to be general peritonitis, perforated gastric ulcer, pneumonia and acute septic kidneys. Where the renal condition was inefficient or very bad the necessary operative interference was frequently followed by fatal results; in very bad cases even a cystostomy under gas and oxygen, or some such anæsthetic, frequently proved fatal. As Mr. Nitch will deal with this aspect of the subject I need not refer further to it but it is obvious that certain very definite rules should be followed in all cases requiring, say, enucleation of the prostate or any major operation when the kidneys are found to be inefficient. With care and skilful treatment many lives that would otherwise be lost can be saved, and apparently hopeless cases from the renal point of view

may frequently be improved to such an extent that an operation which would at an earlier stage have proved fatal can be undertaken with satisfactory results. I cannot, however, emphasize too strongly what I have frequently stated before, that any patient with 100 mgr. or so of urea per 100 c.c. blood should never be allowed to undergo a major operation with a general anaesthetic. Under such conditions the great majority of patients die and the few exceptions only emphasize the rule. Even in bad renal cases, however, such minor procedures as cystostomy and drainage may be carried out successfully but here it appears to me that much depends on the skill of the operator.

Mr. CYRIL A. R. NITCH.

(ABSTRACT.)

The only tests that I have employed in the last three years are the four following, viz.: (1) The percentage of urea in the total amount of urine passed in twenty-four hours by a patient on a normal hospital diet. (2) The urea concentration in the urine collected at the end of the second and third hour after a dose of 15 gm. of urea. (3) The concentration of urea in the blood. (4) The clinical signs of the renal function.

In a healthy adult, on an ordinary diet, the normal figures for the respective tests are as follows:—

(1) Percentage of urea in total quantity of urine secreted in twenty-four hours = 1.5 per cent.

(2) The concentration of urea in the urine, after a dose of 15 gm. of urea in 100 c.c. of water, should be about 2 per cent. in the second and third hour.

(3) Blood urea: there should be from 20 to 50 mgr. of urea in 100 c.c. of blood. The lower figure applies to the young, the higher one to the old.

It is important to realize at the outset that any single one of these tests may be misleading, but when taken in conjunction they yield a very reliable indication of the function of the kidney. In fact a few days ago I showed Professor MacLean a tabular statement of over a hundred cases subjected to operation, and after reading the results of the tests, he gave an accurate forecast of the issue in every case.

In order to emphasize the importance of basing an opinion on the combined results, I will now briefly point out some of the fallacies of the individual tests.

(1) The percentage of urea excreted in twenty-four hours of course depends entirely on the diet. One in which proteids predominate will yield a higher percentage of urea than one consisting mainly of carbohydrates. It has been found that about 1.5 per cent. is the normal with the ordinary full diet of a hospital.

(2) The urea concentration in the urine collected at the end of the second and third hour, after a dose of 15 gm. of urea in 100 c.c. of water. Normally, about 120 c.c. of urine should be secreted in each hour and should contain over 2 per cent. of urea. If the kidneys are healthy, the percentage usually increases slightly in the second hour.

Occasionally, and particularly in the absence of polyuria, there is pronounced diuresis after the dose of urea and the percentage excreted is lower than normal. This may be safely attributed to the diuresis and the percentage of urea taken at a higher figure, or the difficulty may be overcome by administering a second dose of urea, for example:—

One of my cases, a male, aged 77, with an enlarged prostate, without polyuria, secreted 95 c.c. of urine containing 1.8 per cent. of urea in the second hour and 1.0 c.c.

of urine containing 1·8 per cent. of urea in the third hour. The latter figure probably represents over 2 per cent. of urea if the diuresis be taken into account. The blood urea in this case was 42 mgr. per 100 c.c., and the clinical signs of the renal function were fair. The prostate was removed in two stages and the patient made an uninterrupted recovery.

(3) *The Percentage of Urea in the Blood.*—With an ordinary hospital full diet, under normal conditions, 100 c.c. of blood should contain from 20 to 40 mgr. of urea. If the nitrogenous food is greatly increased, the quantity of urea in the blood will be somewhat increased; and, conversely, Professor MacLean has shown that by giving a diet consisting solely of carbohydrates, to a case of interstitial nephritis, an abnormally high blood urea can be reduced rapidly to normal, e.g., a female with 180 mgr. of urea per 100 c.c. of blood was fed on carbohydrates only, and the blood urea was reduced to 30 mgr. in less than three weeks. In spite of the reduction, she died of uræmia when this figure was reached.

(4) The clinical signs of a defective renal function are, in my opinion, most valuable, but here as elsewhere, an ounce of experience is worth a pound of theory. The opinion formed by different observers will vary and where one will give a bad prognosis, another may take a less serious view.

Freedom from thirst, a moist tongue and a good appetite speak for themselves, but when a patient has a typical clayey complexion and a "beefy" tongue and complains of either thirst or disinclination for food, especially meat, I do not allow any chemical test to influence me in favour of an operation. On the other hand, if the clinical condition is good and the tests are below standard, I should pin my faith to the clinical signs.

The following is a case in point:—

A male, aged 74, with an enlarged prostate and 12 oz. of residual urine. No cystitis. Clinically, the renal function was bad. Owing to sudden retention, the usual tests could not be carried out and the bladder was drained suprapubically. He was uræmic for two days and, at one time, I did not think he would recover, but a week after drainage he improved rapidly. His tongue became moist and clean, he was able to enjoy an ordinary diet and the secretion of urine rose to 50 oz. *per diem*. Though the urea concentration, three weeks after drainage, was only 1·7 per cent. in the second and third hour, I considered that the favourable clinical signs justified prostatectomy. He made a rapid and uninterrupted recovery. Seven months later, the urea concentration was 2·05 per cent. and 2·5 per cent. in the second and third hour, and the patient was in excellent health.

The diastase test has been performed as a routine procedure in all my cases, but the results have been so variable that I have ceased to place any reliance on it. The normal is said to vary from 6 to 20, but in some cases with a large percentage of urea in the blood, the index figure is high and in others, with a normal quantity of blood urea, the figure is low.

These remarks are based on a series of 118 cases comprising urethral stricture, lithiasis, diseases and neoplasms of the prostate, bladder and kidneys. Eighty-nine were operated on and twelve died (a mortality of 13·6 per cent.) This figure is somewhat misleading as many of the cases were operated on more than once, such as those with enlarged prostate, in which a preliminary suprapubic drainage was successful. In seven cases death followed suprapubic drainage for acute retention due to enlarged prostate. The remaining five were due respectively to pneumonia (two), shock (one), and ascending pyelonephritis (two). None of these were due to failure of the tests.

There are two factors which are largely concerned in lowering post-operative mortality in diseases of the urinary system. One is rapidity in

operating and, in desperate cases, doing only what is absolutely necessary to relieve urgent symptoms: and the other is the ability of the surgeon to judge the clinical picture and draw correct deductions from the chemical tests.

In drawing up this report, I have purposely included all the cases admitted to St. Thomas's Hospital in the last eighteen months, in which the renal function was estimated. I think the low mortality adds additional weight to the value of the tests when it is realized that some of the operators have not made a special study of urinary diseases.

In cases of unilateral surgical disease of the kidney, where it is necessary to estimate the function of the opposite kidney before a possible nephrectomy, the urea concentration test can be carried out by catheterizing the ureter two and a half hours after a dose of urea, provided that a minimum of 2 c.c. of urine be obtained. I have employed this test in this manner in all my renal cases and up to the present I have not had cause to regret it. An estimation of the blood urea, or of the percentage of urea in a twenty-four hour specimen in these cases is valueless and is often misleading, as the diseased kidney may be doing a large proportion of the work, or even all, should it happen to be a solitary one.

Conclusions.

- (1) The twenty-four hour urea percentage, the urea concentration and the blood urea tests are simple and efficient.
- (2) They are harmless to the patient.
- (3) They can be applied to every variety of urinary disease.
- (4) They give more accurate information than any other test, or tests, at present in use.
- (5) Three, or two, of the tests are of greater value than one alone.
- (6) The clinical signs of the renal function are of great importance in conjunction with the tests and should never be overlooked.

Mr. FRANK KIDD.

(ABSTRACT.)

[Mr. Frank Kidd gave a summary of the various tests that have been employed during the last thirty years for estimating the reserve power of the kidneys. Only a small number of these had proved to be of practical clinical use. Such tests were chiefly required in estimating the power of one kidney when considering an operation on the other, and in estimating the power of both kidneys when operating on the prostate and bladder.]

He continued: Tests can be time tests pure and simple, or more elaborate quantitative tests. Time tests are found to be far more reliable in practice than quantitative tests and they are more easily carried out by the clinician.

(I) *Nephrectomy.—Catheterization of Ureters.*

Principles to be kept in mind when considering the problem of nephrectomy and applying tests to the urines separated from each kidney by ureteric catheters:—

(1) There is no method yet arrived at by means of which the surgeon can be absolutely certain that he has collected all the urine in a given time from each kidney. For that reason alone I doubt if any *quantitative* tests applied to ureteric catheter specimens are worth the trouble, as they are bound to lack accuracy.

(2) The mere passage of a ureteric catheter upsets the *quantitative* renal

balance for a shorter or longer time, which varies with each individual and for each kidney, but does not upset the time-values. Ureteric catheterization produces temporary anuria, oliguria or polyuria in a degree which varies even in the two kidneys being tested, and influences the water more than the solids. Again we see that quantitative tests are shown to be fallacious. Fortunately the time-values are not upset. Some portion of the dye or drug injected will come through within ten to fifteen minutes if the kidney is healthy, unless reflex anuria is prolonged for more than this time, which it seldom, if ever, is.

(3) Anæsthetics and the fear and dread of examination produce similar fallacies.

(4) Seeing, then, that quantitative tests as applied to ureteric catheter specimens are likely to be fallacious, and since it is in the highest degree unwise to leave ureteric catheters *in situ* for more than a few minutes at a time (I cannot too strongly emphasize this point), I therefore most earnestly warn students not to apply quantitative tests which necessitate leaving ureteric catheters in place for an hour or several hours (urea-concentration test, experimental polyuria, &c.). The prolonged in-dwelling of ureteric catheters sets up a ureteritis which may cause intense pain and infect or damage a previously healthy kidney.

(5) Finally, disease of one kidney (especially neoplasm and hydronephrosis) may so depress the function of the other that we should be unwise always to refuse nephrectomy simply because of doubtful functional tests. If the blood-urea is satisfactory and if other clinical factors point the way, it is better to give the patient the benefit of the doubt if his disease of one kidney is likely to prove fatal if left without operation, and if there is reasonable evidence that another kidney is anatomically present and is not pathologically altered, even though it may not show up perfectly when considered functionally.

(6) In certain cases of ureteric catheterization do not forget that much time can be saved by injecting a drug or a dye intravenously rather than intramuscularly.

Summing up, I would advise the following tests as of value in the case of ureteric catheterization when considering nephrectomy:—

(1) *Employ time tests, pure and simple, rather than quantitative tests.* The time of the first appearance of indigo or phthalein is the important matter, not its amount. Time tests obviate the necessity for measuring the exact quantity of urine passed in a given time by each kidney.

(2) Instantaneous simultaneous correlation tests applied to the blood and each urine may be of value in doubtful cases. The electrical resistance test applied to a drop of blood serum taken while the ureteric catheters are *in situ* and applied also to a drop of urine taken from each kidney at the same time I have found of considerable value. A few drops of fluid only are required in this test as compared with the large quantity needed for cryoscopy and blood-urea hæmo-renal indices. This is why it is to be preferred in practice.

(3) The percentage of urea taken from the separated urines has often appeared to me of value, though in theory it should not be accurate. It is at any rate of greater accuracy than attempts to measure the total quantity of urea passed in a certain time.

(4) Pathological and anatomical considerations (pyelography) should not be overlooked, and in practice the problem is usually solved satisfactorily by correlating these with functional considerations, for example, the question as to whether the urine from the poor kidney contains blood, pus, and bacteria, as compared with the urine from the good kidney, which is of good colour and

clear of pathological elements and gives a good time test. Pyelography is usefully related with the surgical anatomy, and is often of more value than the tests concerned with surgical physiology.

The chief uses of these ureteric catheter tests are in the consideration of nephrectomy in cases of tuberculosis, neoplasm, stone, and hydronephrosis.

If both kidneys are diseased (e.g., in tuberculosis), the blood urea test may be expected to warn us of this in doubtful cases—a fact well worth remembering.

Finally, I would rather put my trust in the indigo test than in all the others. It can be employed by the surgeon himself at the bedside, and is therefore superior to any laboratory test. I have used it for fifteen years, and I can recall only two or three instances of its failure.

(II) *Prostatectomy.*

In this operation the surgeon wants to be able to detect loss of function in both kidneys considered together, as a result of back pressure before any of the ordinary clinical signs of back pressure have appeared. Here he has no need to separate the urines from each kidney before applying the tests. For more than ten years I have been doing prostatectomy in two stages in cases in which the renal function tests are deficient, and have thus been enabled to save many lives by operation that would otherwise have shortly succumbed to the dangers of catheter life. *In obvious cases with giant atonic painless bladder and clear clinical signs of back pressure, on no account whatever should a catheter be passed for the purpose of carrying out tests.* In such cases a catheter may kill in forty-eight hours. Be wise; simply drain the bladder with a Pezzer tube and leave the urethra alone. Suprapubic drainage presents far less risk than catheter drainage. Tests can be carried out later through the Pezzer tube without passing a catheter. In doubtful cases I find "time tests" of great value and easy to carry out. In doubtful cases only a catheter is tied into the bladder and the urine is drawn off every quarter of an hour for one hour. I employ indigo-carmin and I expect a moderate degree of the blue to appear in the second specimen and a deep blue coloration to appear in the last two specimens. If this does not happen it is seldom, if ever, wise to operate in one stage. The phthalein test is of equal value as a time test in these cases, but I doubt if its quantitative relations are of such accuracy as has been claimed by its originators.

In these cases, too, the blood urea test is excellent, but it does not show up as early, nor is it so delicate as the dye tests. It is a good auxiliary test in doubtful cases. I have seen the blood urea test become normal after drainage of the bladder long before the indigo test has returned to normal and long before it is really safe to perform the second stage of the operation.

In the majority of prostatic cases the kidneys regain sufficient functioning power after the first stage of the operation in ten to fourteen days to enable the second stage to be carried out; but in exceptional cases they may take four to six months to recover sufficiently. It is always better to wait too long than to operate too early.

Conclusions.

Ideal tests of renal function should be as simple as possible, and in the last result should be such as can be carried out by the clinician himself. All tests should be searchingly scrutinized in combined work between the clinician and the laboratory worker. But eventually the tests to be chiefly adopted in practice are those which are found to be most accurate and above all capable

of use by the clinician alone. Hence I consider the indigo-carmin test the test "par excellence"; and in surgery I believe that two classes of test will prove of permanent value, namely, the dye tests (indigo and phthalein) and the blood-urea test for prostates and doubtful bilateral kidney disease.

Lastly, no surgeon ought to operate on kidneys and prostates unless he has had a good clinical training in the use of tests of renal function.

DR. R. L. MACKENZIE WALLIS.

(ABSTRACT.)

As a result of my experience I place the greatest reliance on the following tests: (1) Diastase in the urine; (2) urea in the blood; (3) ratio of blood sugar to urinary sugar; (4) urea concentration test.

(1) *The diastase content of the urine* as a test of renal efficiency has proved of the utmost value in my work. In 1915 I studied a large number of cases of nephritis occurring in France, and found the diastase content of the urine was the best individual test for both diagnosis and prognosis. Naturally my experience of this test has been extended of recent years, and both from a surgical and medical point of view I have no hesitation in recommending its use.

In any case of renal damage the normal diastase content is reduced from 10 to 20 to figures of five units or less according to the degree of damage. In toxic kidney, on the other hand, such as is met with in eclampsia, the diastase content is abnormally raised.

(2) *Urea in Blood*.—This estimation, done as a routine test on every renal case, and taken in conjunction with other tests, is undoubtedly of value. By means of a microchemical method this determination can be made upon a few drops of blood obtained by means of a finger prick. The urea content of normal healthy adults ranges between 0.02 to 0.04 gm. per cent. In elderly men this figure generally rises to 0.05 gm. per cent., so that in cases of prostatitis this latter figure may be taken as normal for the age of the patient. Figures for blood urea of 0.07 gm. per cent. should be considered as unfavourable, and should cause the surgeon to proceed with caution. Patients with a urea retention of 0.1 gm. or over should not be submitted to operation as the risks of uræmia are very much greater.

(3) *The urea concentration test* of MacLean has also been used and found to yield results in accordance with the other tests in most cases. My colleague Mr. H. E. Archer has followed the rise in blood urea following the administration of 15 gm. of urea, and found no definite relationship exists between the rise of urea in the blood either in time or extent with the urea concentration in the urine.

(4) *The relationship of the blood sugar content to renal insufficiency* is of course well known, but so far no satisfactory explanation has been discovered. It was suggested that the suprarenals were affected, and the increased activity of these glands accounted for the hyperglycæmia as well as the high blood-pressure. The presence of a definite and constant amount of sugar in normal urine has also been known for some time and various methods have been devised for the detection and estimation of this sugar. After a series of trials I have devised a method of estimating the normal urinary sugar, and find that in a normal person the percentage amount of reducing sugars is remarkably constant, and is almost identical with that in the blood. The normal blood sugar content varies from 0.08 to 0.011 gm. per cent., and that in the urine

varies from 0'07 to 0'09. If the blood sugar content is increased, as for example after a meal, then the urinary sugar is correspondingly increased. In other words the kidney acts as a "safety valve" to maintain the circulating blood sugar as near as possible at the normal level. The ratio of blood sugar to urinary sugar in a normal person is therefore unity or slightly above or below. The slightest damage to the kidney upsets the safety valve action and this is expressed by an increased ratio. The greater the renal damage the higher the blood sugar and the lower the urinary sugar will be. This ratio may be raised in spite of the absence of urea retention. There is a general correspondence between the urinary sugar and the diastase content. This test has proved of value in my hands.

No test or tests for renal function will answer the question when to operate and when not to operate. They are of value in helping the surgeon in the doubtful cases, and in such there would appear to be general agreement between the clinical signs and symptoms and the results of these tests. When in doubt the surgeon performs the operation for prostatectomy in two stages, and if he has the renal function tests done in such cases he is able to proceed with greater confidence and I am confident is not guided entirely by his clinical findings. In cases of prostatitis the tests applied are really tests for the functional efficiency of both kidneys, and we cannot at present define the type of pathological lesion, or its extent, by these tests alone.

With unilateral or bilateral disease of the kidneys, the tests are of greater value. In such cases it has been our custom to do all the tests described above. As far as possible specimens obtained by ureteral catheterization have also been obtained and these are examined for sugar, urea, and diastase content. The estimation of these three constituents can be made upon a few cubic centimetres of the urine, so that there is no difficulty with regard to quantities obtained in making these determinations. These tests have proved of value in deciding which kidney is damaged, and whether the other kidney is capable of performing its functions. Then again, by means of these tests we can differentiate between an ordinary pyelitis and a pyelonephritis. The interpretation of the results is a matter of experience, and in conclusion it may be said that no one test is of value in diagnosis of renal insufficiency. A combination of three or four tests often supplies the necessary information required, but each test must be taken on its merits and considered both individually and collectively. In this way it is found that the tests are of real value and conclusions based upon them are more frequently right than wrong.

For diagnostic purposes I regard the urea in blood, sugar in blood and urine, diastase, and urea concentration tests as the tests of choice, and if limited to only two I should select the first two.

Mr. JAMES B. MACALPINE.

(ABSTRACT.)

There can be little real parallelism between the work of the physician, which deals with the numerous non-suppurative inflammations of the kidney, and that of the surgeon, which deals, so far as total renal function is concerned, with a single primary phenomenon in renal compression and two secondary phenomena in fibrosis and suppuration. The physicians have elaborated a number of tests which are designed to meet the varying lesions which they have to investigate, and their work has to some extent a histological basis. In the surgical field the kidney function should be regarded as a unit, and in

the present state of our knowledge we do well to regard the tests for renal function as affecting the whole organ, and offering data which can be used empirically in prognosis and treatment. I propose to discuss two tests, the phenol-sulphone-phthalein and blood urea tests. Amongst bodies of which phenol-sulphone-phthalein stands as the type comes urea, and it is owing to this fact that we are interested in it. It is twin-sister to the urea concentration test. It represents the kidney's function in excreting urea and similar substances, whilst the blood urea test represents the threshold or head of blood urea which is requisite before it can be expelled through that organ. The two tests show parallel results, with one important qualification. Man is supplied with approximately three times the amount of renal tissue which bare necessity demands (Rose Bradford and others). Approximately two-thirds of the renal tissue can be removed or destroyed by disease before signs of failure set in. As a corollary it follows that in health the kidney only works at one-third its capacity, but in disease when the excess of renal power is diminished, the kidney must apply itself more systematically in order to keep the blood constitution normal, and when the two-thirds reduction point is approached its work must be uninterrupted. This is one of the causes of nocturnal frequency in a prostatic, the kidney carrying on its task breathlessly and perpetually. It also accounts for the phenomenon of "specific gravity fixation." When the safety margin has been absorbed the blood begins to show signs of retention of urea, &c. Degeneration has therefore advanced a long way before the blood urea test indicates its presence; the phenol-sulphone-phthalein test, however, gives warning at an earlier period; once the two are recording side by side I have found that their indications run parallel.

In Prostatic Disease.

In prostatic disease what are the data which may be demanded from a perfect renal test?

(1) It must supply an index before operation. If a series of cases be examined clinically there will be found to be a perfect gradation—at one end the man who is in perfect health, and at the other the moribund patient; at either extremity there is no need for artificial tests. Between these two there is an infinitely graded series of patients, and it is often difficult to appraise their true position by clinical means alone. The use of tests widens out the rungs of the uræmic ladder in such a way as to allow us to observe more clearly the position occupied by the several patients in the scale. It is possible, I believe, to take arbitrary figures indicating (i) the upper limit of the normal variation for blood urea; (ii) a line below which a one-stage operation may be safely done, and above which a two-stage prostatectomy is advisable; and (iii) a line which will lie at the entrance of the danger zone. Very varying figures are given by different observers for these lines, the differences being probably the result of differences in technique (the standardization of technique is therefore very desirable). I have assumed with MacLean that 40 mgr. per 100 c.c. represents the upper limit of blood urea in healthy people, that 50 mgr. is the limit for one-stage prostatectomies, and that 75 or 80 is the level at which the prognosis should be guarded, though many cases of greater concentration can be operated upon successfully.

(2) After the first stage these tests give valuable indications of the progress of the case, and here the urea test is much more valuable than the phenol-sulphone-phthalein. It is very difficult to gather the latter from urine obtained from a suprapubic wound, as it will be contaminated with pus and possibly

blood, and some of the urine will be lost. When phenol-sulphone-phthalein is used for this purpose it should be injected intravenously, for Braasch has shown that the tissues in this condition have the capacity of temporarily absorbing the phenol-sulphone-phthalein when injected intramuscularly, and the readings are therefore erroneous.

The blood urea test shows its highest value here, and is always used in my work to follow the progress of the case. After the first operation a period is entered upon which may be called a "negative phase," during which the renal function is further depressed and the blood urea figures naturally increase. After this preliminary depression a reaction or "positive phase" occurs, and an adequate study of the blood urea curve at this period is of the greatest importance. Nothing could be more haphazard than to submit a patient to the second stage of operation until it is known that the blood urea has fallen to normal. Further, an immediate post-operative fall in the blood urea should not tempt one to proceed with the second stage until it is certain that the fall is not due to the post-operative restriction of urea-forming bodies in the diet.

Between the stages there are two causes for slow recovery, viz., renal fibrosis and sepsis. Renal fibrosis may result from back pressure, but it may also arise from sclerotic lesions of cardiovascular origin; the fall of blood urea may be very slow when fibrosis is due to the latter cause; on one occasion I have waited as long as five months before performing a successful prostaticectomy. The progress should be carefully watched by means of observations on the blood urea. A hint of this cardiovascular lesion may be obtained before the first operation if systematic observations of blood pressure are taken.

(3) In cases in which it is desirable to avoid operative interference, systematic readings of the blood urea should be made to see to what extent, if any, the kidney is deteriorating. In such cases the dye excretion and urea concentration tests should be avoided, since they involve the use of the catheter with the attendant risk of sepsis, to which such bladders are particularly susceptible.

Separate Renal Function.

In combination with the ureteric catheter the phenol-sulphone-phthalein test has given fair results. Its chief disadvantages are:—

(1) The delicacy of the colouring matter, and ease with which blood and pus vitiate the results; blood particularly is liable to cause alterations of the colour, even in small amounts. If this occurs, I have resorted to the use of indigo-carmin on a subsequent occasion.

(2) The difficulty of obtaining a true reading on the colorimeter. I occasionally take several observations, only to find that they vary, and have seen readings by several different observers in which there has been a considerable variation.

The quantity of dye secreted by the sound kidney varies *inversely* with (a) the amount of work which is being performed by the opposite kidney, and (b) the amount of reflex depression which has been caused by disease in the opposite kidney. It will vary *directly* with the extent to which compensatory hypertrophy has taken place in the sound kidney.

These factors are very difficult to assess correctly, and for this reason an estimation of the quantity of dye eliminated in a given time has seemed to me to be less valuable than an observation of the time at which the dye comes through.

In cases of tuberculosis of the urinary tract, where the bladder irritability is so great as to preclude cystoscopy, I have occasionally used the blood-urea

test. In such cases it is necessary to prove, either as the result of clinical examination, or by means of the X-rays, that one kidney is totally disorganized. A satisfactory blood-urea test will then suggest that the opposite kidney is functioning well. Though this test cannot in any way equal a proper examination with the ureteric catheter, &c., it seems to me to give the best evidence which the circumstances allow.

Mr. R. H. JOCELYN SWAN.

(ABSTRACT.)

I propose to give my personal experience of the tests that I have found valuable, and to attempt to formulate the conclusions I have arrived at in the estimation of the efficiency of the kidneys.

From the surgical standpoint the subject must be approached from two points of view:—

(1) Where it is necessary to ascertain the general renal excretion before any operation is undertaken on the lower urinary passages, such as prostatectomy or in cases in which some major surgical operation is advisable, but in which doubt exists as to the renal efficiency.

(2) Where it is necessary to ascertain the functional efficiency of one kidney before any operation on, or possible removal of, the other is contemplated.

In the estimation of the general renal efficiency, the most valuable tests are the blood-urea test and the urea concentration test; the comparison of these figures gives an index of the renal function. These tests should be carried out in routine manner; thus the blood urea should be estimated during a period of rest on an ordinary diet, whilst the urea concentration test should be conducted after a period of abstention from food and drink for at least twelve hours. For this reason I order the 15 grm. of urea in 100 c.c. of water to be given at 7 a.m., and estimate the urea content in the urines collected at 8, 9, and 10 a.m. To comment briefly on the figures obtained in these tests: in the first place, what standard are we to consider as the approximate normal, and what divergence from the normal marks the danger zone of renal inadequacy? The normal blood-urea content has been placed as low as from 15 to 20 mgr. per 100 c.c. by some observers (Dobson, Bently Squire and Myers), but MacLean would give a higher figure as the normal. In a series of cases taken for me at the Cancer Hospital by my clinical assistant, Mr. H. W. S. Wright, I find that in a set of patients awaiting operation for conditions unconnected with the urinary organs, who had not been depleted of fluid, and in whom the ordinary urinary analysis had shown no indication of renal disease, at ages varying from 49 to 79, the average blood urea was 45 mgr. per 100 c.c., varying from 24 to 60. In this series of cases major operations were performed without any untoward symptom, including one, aged 64, who had a blood urea of 56, upon whom I did a complete abdomino-perineal resection of the pelvic colon and rectum for carcinoma. Too much reliance, therefore, must not be placed on a somewhat high blood urea, especially in patients over 50 years of age, or when operations are demanded for such conditions as malignant disease.

With regard to the urea concentration test in cases other than those of unilateral renal disease, to which I shall refer more particularly later on, the test should be carried on for three successive hours, and not only should the urea percentage be estimated and compared with that of the urine before the dose of urea was given, but the amount of urine secreted in each hour should be recorded, so that the total quantity can be estimated. The percentage urea

content will vary with diuresis, and I have found that in many cases the third hour shows a good concentration when the first two hours have been low—in fact, in my series a larger amount of urea was secreted in the third hour than in the second in 45 per cent. of cases.

In cases in which one kidney is known to be diseased, and especially if any operation is contemplated on it, the urea concentration test on the combined urines does not appear to give a satisfactory indication of the functional capacity of the other organ. If the test shows a satisfactory figure, it is strong presumptive evidence that the second kidney is fully functioning; but is the converse of this true, namely, that a low reading implies that the second kidney is not fully active? A low reading in the combined urines from both kidneys may be caused by the mixture of a low urea from the diseased side with a normal content from the other, making the combined reading low.

I therefore consider that for a true concentration test to be applied to one kidney, it should be done on the urine obtained after ureteric catheterization.

The following figures were obtained from a case of hydronephrosis in a man, aged 42: Blood urea, 38 mgr. in 100 c.c.; combined urines, urea 1·4 per cent. Concentration test: first hour, 1·7 per cent.; second hour, 1·8 per cent.; third hour, 1·8 per cent. Urine from non-affected side by ureteric catheter: urea before urea meal, 1·7 per cent. Concentration test: first hour, 2·4 per cent. in 45 c.c.; second hour, 2·8 per cent. in 37 c.c.; third hour, 3 per cent. in 60 c.c.

It is inadvisable, however, for a catheter to be retained in the ureter for this length of time, partly owing to the pain that it occasionally causes, and also on account of the hæmorrhage it may set up. For these reasons I have, in my more recent cases, given the 15 grm. of urea and passed a catheter into the renal pelvis of the side to be tested after an interval of two and a half hours, estimating the urea content from the urine obtained. This method has the advantage of being simple and is efficient. In a recent case of calculous pyonephrosis the urea concentration on the unaffected side two and a half hours after the dose of urea was 3·6 per cent., whereas on the affected side it was only 0·8 per cent. in the same time.

It is necessary to carry out all tests for renal function without an anæsthetic for ureteric catheterization, so as to eliminate artificial conditions, or should an anæsthetic be necessary, to use gas and oxygen; a spinal anæsthetic may cause such a fall in blood pressure as to affect the urinary secretion. I place no reliance on the relative amounts of urine collected from the two sides separately; I have many records in which the amount of urine from the diseased side exceeded that from the healthy. The comparison of the urea content from each side is much more valuable.

Dye-Excretion Tests.—I have used the various colour tests in the estimation of renal efficiency extensively, but am not so inclined to place reliance on the excretion of a substance foreign to our normal elaboration as on that of urea excretion. In 1910 I published the results obtained after the intramuscular injection of methylene blue, but I have discarded this method as cumbersome and too lengthy. Phenol-sulphone-phthalein has been used, but I have discarded it owing to the difficulty of determining the first trace of colour in the alkaline solution, especially in the presence of any bleeding due to instrumentation, and also because I have found the results unreliable.

I have used indigo-carmin much more extensively, both as an intramuscular and an intravenous injection. Since using an intravenous injection of the dye, I have been more impressed with the efficiency of the test, and regard with suspicion any kidney that does not secrete a blue urine within seven minutes

of the injection of the dye into a vein. The test has the great advantage of simplicity and can be readily carried out by a simple cystoscopic examination, without an anæsthetic for preference, as the elimination of the dye seems occasionally to be affected by it. The indigo-carmin elimination test is, further, a delicate one, for I have seen the coloration delayed in cases of bacilluric pyelitis, in which the blood-urea and the urea concentration tests have given results within normal limits. The following interesting case was recently under my care:—

A woman, aged 51, had had two attacks of transient, slight hæmaturia, lasting two days, during which she had had slight aching in the left loin. No renal enlargement could be detected, skiagraphy was negative for calculus, and cystoscopy showed no evidence of vesical disease; the urine showed a minute trace of albumin, no pus nor casts. Blood urea was 35 mgr. per 100 c.c., and urea concentration 1·6, 3·2 and 2·6 per cent. in three successive hours. After intravenous injection of 2 c.c. of 0·4 per cent. indigo-carmin, the urine showed marked coloration from the right kidney in six minutes, but only the slightest trace of coloration from the left side, and then only after eighteen minutes. Subsequent exploration of the kidney revealed an early hypernephroma projecting between the vessels and the ureter at the hilum of the kidney. The pelvis was not dilated and the renal tissue appeared to be normal.

The conclusions at which I have arrived as the results of my experience of these tests are the following:—

(1) For a general test of the renal function, the estimation and comparison of the blood-urea with the urea concentration test applied for three successive hours, under routine conditions, gives valuable indication of efficiency.

(2) For the estimation of the efficiency of a single kidney, the elimination of indigo-carmin within seven minutes after an intravenous injection of the dye, and the estimation of the urea in a specimen of urine collected from the kidney by ureteric catheterization two and a half hours after the injection of 15 gm. of urea in 100 c.c. of water, are the most valuable methods.

At the same time I think we are inclined to place too much stress upon the figures obtained from laboratory tests, to the exclusion of clinical observation and to the neglect of clinical signs. Do not let us look upon any one test as final, but rather base an opinion on the clinical evidence influenced perhaps by the laboratory findings, but not wholly dependent on them. The decision as to whether an operation should be performed or not, or whether it should be done in stages, must depend upon the full consideration of all the evidence rather than on one detail of it, and after all, these tests only form one link in the chain. In a large majority of cases the clinical evidence will eliminate the cases in which the renal efficiency is deficient, confirmation of which is obtained by the tests; but it is frequently just in those cases in which difficulty arises that the tests leave us in the same state of uncertainty. What has been stated by some as evidence of inefficiency has probably been given in too narrow a limit.

Mr. W. GIRLING BALL.

(ABSTRACT.)

During the last year I have been utilizing the urea concentration test, the estimation of urinary diastase and of blood urea as a means of estimating renal function; the measurement of normal urinary sugar has been of value in some cases. I have been working with Dr. Mackenzie Wallis, and we agreed to express our views, independently of each other's findings, as to the desirability or otherwise of carrying out surgical procedures, before the chemical tests and

clinical observations were placed side by side before an operation. I am satisfied from the comparatively small number of cases to which this test has been applied that the examinations mentioned above, for the most part, compare favourably with others previously adopted and reported on by other observers. I believe that tests which give estimates of the normal excretions of the kidney and the normal constituents of the blood, if they give reliable results, must necessarily have an advantage over others which require the introduction of foreign matter into the circulation.

[Mr. Girling Ball's remarks were illustrated by the demonstration of the results (shown in tables) as applied to two groups of cases: (I) Those of obstruction to the lower urinary passages, and (II) those of disease of the ureters and kidneys.] He continued: In the first group in all cases in which advanced renal disease is obvious from the clinical findings, at the operation, or at an autopsy, the tests give accurate results; at the same time, some of the records show chemical tests indicating normal renal function, whereas the clinical findings or the after results of an operation prove that this is not the case. This occasional inaccuracy constitutes a danger to the practitioner. A pair of kidneys may reasonably be expected to give normal renal function tests, whereas a surgical procedure may be quite sufficient to overtax the renal reserve, a property of the kidneys of which no tests at present in use give us any indication.

In the second group of cases the tests have been applied in two ways: (1) In estimating the whole renal function, and (2) in attempting to estimate by the same tests the function of each kidney. In every case in which one kidney only was diseased, the tests of the whole urine have given normal results. In order to estimate the urea excretion from each kidney special tests have been devised for the measurement of small quantities, and in this connexion the amount of normal sugar excreted has also proved to be of value; indications as to the value of these tests in showing which was the least damaged of two diseased kidneys have been demonstrated, but not very satisfactorily. In urinary disease, if the whole urine gives normal figures and the clinical findings show that there is only one kidney diseased, it is fair to assume that the diseased kidney can be removed. I would urge the desirability of estimating urinary sugar and urea in catheterized specimens from each kidney, with the object of proving their value. I agree with the view that the presence of a catheter in a ureter diminishes its function. Investigations in the future should be directed not towards estimating the function of the kidneys as a whole, but towards devising some means of estimating their reserve; this can only be gauged at the present time by the clinical findings and by past experience, which must always hold a high place. I think that, so far as the disease of the lower urinary passages is concerned, a double pyelography might be used as an indicator of the distension of the pelvis of the kidney, thus suggesting the degree of damage as the result of obstruction. Positive evidence of diminished renal function obtained from chemical tests must always make the operator wary, but I do not consider that normal results necessarily mean that the kidneys are capable of carrying on efficiently after surgical interference.

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Section of Urology.

President—Sir THOMAS HORDER, M.D.

Large Ureteric Calculus.

By P. MAYNARD HEATH, M.S.

THE main body of the calculus is roughly cylindrical in shape but at each extremity is a beak-like process set at an angle with the body. The length between most distant points is $3\frac{7}{8}$ in., but by making a model and unfolding the curves the real length is found to be 6 in. The greatest diameter is 1 in. and the weight 1,075 gr.

The calculus was removed from the pelvic portion of the left ureter by an extraperitoneal muscle splitting operation, and the wound in the ureter closed by suture. There was no evidence of suppuration in the kidney which was not exposed. The patient was a male, aged 30, whose only complaint was turbidity of the urine.

Ureteral Calculus (Royal College of Surgeons Museum, 3638 T).

By R. LAWFORD KNAGGS, F.R.C.S.

A LARGE trifold calculus 7 cm. ($2\frac{3}{4}$ in.) in its chief diameter which was extracted from the lower end of a dilated ureter.

History: From a man, aged 37. Twelve years before he had passed a small stone by the urethra. Before that when 17 years of age, he had voided on one occasion before micturating about a tablespoonful of some kind of discharge.

Before admission the patient was seized with acute pain in the right inguinal region lasting two hours. He had a similar attack the next day and another five days later, followed by vomiting.

On admission: The right testicle was undescended and there was occasional hernia. A hard mass could be felt bimanually from the rectum, on the right side. The skiagram showed this to be a calculus. On cystoscopy the ureteral opening—slightly patulous, and exuding mucus—was seen on the convexity of a large eminence projecting from the posterior wall of the bladder.

Median laparotomy exposed a mass rising from the pelvis. The dilated ureter containing the calculus was surrounded by consolidated fat quite an inch in thickness. The calculus was removed through a longitudinal incision. The bed in which it lay presented loculi corresponding to the prominences of the calculus and also apparently to natural gaps in the posterior bony pelvis (? great sacro-sciatic foramen). The index finger could easily be passed up the ureter

towards the kidney pelvis. The urine in the ureter was ammoniacal. The incision in the ureter was closed except that drainage tube was fixed in its lower end and brought to the surface extraperitoneally.

The patient was last seen some months later when the wound had quite healed. He suffered no inconvenience and was apparently quite well. The quantity of urine that had escaped by the drainage opening had never been large and had soon ceased. No examination of urine was made after his leaving the hospital.

Specimen of a Sarcoma of the Kidney removed from a Child, and the Patient himself.

By CECIL ROWNTREE, F.R.C.S.

MALE, aged 6. Painless tumour in the loin of several months' duration. April 18, 1920: Nephrectomy for renal sarcoma, half of which is preserved (No. 922, Cancer Hospital Museum). January 26, 1922: Patient shown.

Mr. J. D. MALCOLM recalled a case in which he removed a kidney tumour from a child under 2 years of age, in 1892,¹ the patient being now alive and well. The growth consisted of tubules of varying size, without regular direction and lined with columnar epithelium, but some were full of cells and showed no lumen. No striped muscle cells nor sarcoma cells were found. The tumour was called a "malignant adenoma" by the late Mr. Targett, but Prof. Shattock, F.R.S., had recently re-examined it, and expressed the opinion that it was innocent. Dr. Robert Abbe, of New York, published two cases² of prolonged survival of children after removal of renal growths. One specimen showed "imperfectly developed tubules similar to, but not identical with, the renal tubules. There are also irregular patches and rows of cells. In between these masses of epithelium there is a connective tissue so rich in cells of various arrangements that it might well be regarded as sarcomatous." This was at first called an "adeno-carcinoma," that structure being considered its most important feature. It obviously resembled the growth last described, and the similarity might account for the prolonged success, but a tumour developed in the other kidney nearly five years after the operation.³ In Abbe's second case the structure was that of a round and spindle celled sarcoma, associated with the striped muscle cells so common in these tumours. This patient was alive and well, and was described as "unique" in 1912.⁴ It was curious that the longest living New York case and the London case, which might be considered complete cures after at least twenty, and nearly thirty years, were operated upon within a week, respectively on November 20 and November 15, 1892. All other recorded cases of renal neoplasm in children, when operation was survived, had, so far, shown a recurrence within five years, and frequently within eighteen months.

Specimen of a Vesical Calculus enclosing a Fragment of Bone.

By CECIL ROWNTREE, F.R.C.S.

PATIENT, a male, aged 17, sustained fracture of the pelvis and rupture of the bladder and rectum, March 18, 1919. January, 1920: Vesical calculus seen in skiagram. January, 1922: Calculus removed: the bisected calculus shows a fragment of bone.

¹ *Trans. Clin. Soc. Lond.*, 1894, xxvii, p. 94; 1895, xxviii, p. 287; 1903, xxxvi, p. 233.

² *Annals of Surg.*, 1894, xix, p. 58.

³ *Ibid.*, 1897, xxv, p. 360.

⁴ *Ibid.*, 1912, lvi, p. 469.

Angeliomyoma of the Bladder removed at Operation.

By FRANK KIDD, F.R.C.S.

HISTORY: Patient, a male, aged 29. November 8, 1921: Onset two years ago, acute retention relieved by catheter. Since then several attacks of a similar nature. Four months ago severe attack, catheter passed four times, bladder infected, which resulted in great pain on passing water, increased frequency and pyuria. Slight hæmaturia once or twice after a catheter has been passed. No venereal history.

Examination: Nervous system healthy. Urethroscope shows no stricture. Radiograms show no stone.

Cystoscopy: Urine full of pus, bladder very irritable. Bladder wall very much inflamed. A smooth pedunculated tumour can be seen attached to the anterior wall of the internal meatus, projecting from there into the bladder.

Operation, November 30, 1921: The bladder was opened in the middle line. A smooth pedunculated tumour was seen arising from the anterior portion of the margin of the internal meatus. It was about the size of a date and looked like a ripe raspberry, being obviously full of blood-vessels. The pedicle was seized with a curved clamp and pulled forwards and upwards until a second clamp could be placed well behind the pedicle. The tumour was then sliced off between the two clamps. No ligature was needed. The bladder was stitched up and drained with a small Pezzer tube.

Lavage with 1 in 8,000 flavine through the tube soon put an end to the cystitis. The tube was removed on the tenth day and the fistula healed soundly within less than a fortnight, though convalescence was complicated with an attack of bronchitis. Thereafter the patient passed clear water naturally, and with no difficulty or pain, and since then has remained perfectly well.

Half the specimen is shown (it has shrunk a good deal), and also a coloured drawing. The other half has been used for microscopic investigation.

The following histological report was made by Dr. Eastes and Dr. Fletcher: "The specimen was a small ovoid polypoid tumour attached to the bladder-wall by a short narrow pedicle. A complete section has been cut, and the growth has peculiar histological characters. It is composed in the main of unstriped muscle tissue supported by a stroma of simple connective tissue, and very rich in large blood-vessels with thick muscle coats. In fact, it is the muscle coats of these vessels which apparently make up the growth. We think, therefore, it should be termed as angiomyoma. It is evidently innocent. Superficially it is inflamed, ulcerated and covered by granulation tissue."

Myoma is one of the rarest tumours met with in the bladder, and is undoubtedly benign in character. As far as I can ascertain, there are not more than thirty cases reported in the literature.

Five Cases illustrating the Value of a Radical Operation for Carcinoma of the Penis.

By FRANK KIDD, F.R.C.S.

Case I.—T. O., aged 53. History (May 17, 1920): Six months or more an ulcer around the meatus, which during the last few days has caused retention of urine and pyuria. Operation (May 26, 1920): Complete removal of crura

and most of the corpus spongiosum, the penis, both testicles and scrotum, the superficial and deep inguinal glands, and the underlying deep fascia on each side, under spinal anæsthesia. Catheter stitched into bladder. Sections showed a squamous-cell carcinoma with secondary deposits in the glands. Result (January 26, 1922): Patient can stand up and pass water with a good stream. The urine is clear. He feels as if he had a penis still. He gets nocturnal emissions with complete orgasm accompanied by a discharge of vesicular contents. No tendency to stricture of meatus.

Case II.—A. R., aged 55. History (August 4, 1921): Since 1916 has noticed an excavating ulcer on the penis. Wassermann reaction negative. There is an excavating ulcer involving the body of the penis; no enlarged glands to be felt. Operation (August 10, 1921): Spinal anæsthesia and ether. Complete removal of penis, testicles and glands and deep fascia on both sides in one piece. Catheter stitched into bladder. Sections showed carcinoma of the penis, but no deposits in the glands. Result (January 26, 1922): Patient can pass water well; no tendency to stricture.

In addition to these two cases I have operated on three other patients with carcinoma of the penis by removing the whole penis, testicles, and the glands and deep fascia on both sides at one sitting. All the operations have been under spinal anæsthesia combined with a little ether. There has been no shock and the wounds have healed well. There has been no tendency to stricture of the urethral stump. In one case I tried to make an artificial penis by leaving the urethral stump long and covering it in with a skin flap, but the skin sloughed, and my object was thus defeated.

RESULTS.

Case III.—T. E., aged 51. Operation (January 3, 1920): Reports himself as quite well two years later.

Case IV.—H. G., aged 50. Operation (October 30, 1915): Reported by himself and his doctor as quite well six years later.

Case V.—M. M., aged 70. Operation (April 5, 1913): When last seen two years later was quite well, but has since been lost sight of owing to change of address.

Carcinoma of the penis is a rare disease, so that one does not get many opportunities of operating on it. The inguinal glands, superficial and deep, are infected early—yet the course is slow, and a radical operation holds out a good prospect of cure. Barney¹ published an analysis of 100 cases, and stated that 40 per cent. were cured.

Although some good results have been noted after a partial operation, I, nevertheless, hold that in all cases a radical operation ought to be carried out to include the glands and fascia on each side, based on the principles laid down by Sampson Handley for operating on carcinoma of the breast. Success in the operation is dependent on a good knowledge of anatomy and clean dissection. The use of spinal anæsthesia minimizes the risk of shock. I do not know whether it has been pointed out before that nocturnal emissions are possible after the operation.

¹ *Annals of Surg.*, 1907, xlvii, p. 890.

Case of Bladder Diverticulum.

By F. FOWLER WARD, M.B., B.C.

PATIENT, a male, A. R., aged 41, attended out-patient clinic, Urogenital Department, at East Suffolk and Ipswich Hospital on March 10, 1920.

Previous history: Gonorrhœa twenty years ago. In 1910 had retention of urine; catheter passed, and no further trouble until September, 1919. Then he had difficulty in micturition, particularly during the day. At night, and during defæcation, urine was passed more easily.

Present condition: He now passes urine once in twenty-four hours, and micturition is assisted by pressing his hands on either side of the abdomen; no pain. Bladder very distended. Urethroscopic examination, *nil*; no definite enlargement of prostate, but fullness in middle line, extending beyond reach of finger. Vesicles normal. Hæmorrhoids present; no gastric crises. Pupils and knee-jerks normal. Wassermann reaction later proved negative. Six ounces of urine drawn off, specific gravity 1010, with considerable amount of pus.

Six ounces of urine were withdrawn for a few days, and then daily irrigation of the bladder with oxycyanide of mercury, 1 in 8,000, was carried out. By the end of May, 1920, he could pass urine three or four times a day aided by manual pressure on the abdomen. On vesical lavage as much as 40 oz. could be retained occasionally without any discomfort, the returning fluid being clear: at times, however, the discharge of this fluid was followed by that of an ounce or more of thick fœtid pus.

In June, 1920, he was admitted as an in-patient, and on June 22 cystoscopy under a general anæsthetic was attempted. After the bladder was apparently clean the cystoscope was introduced, but vision through it was immediately obstructed by thick clouds of pus, which repeated lavage failed to disperse. Rest and lavage eased his condition, and he resumed his work until September, when difficulty in micturition again occurred. The bladder was opened, and two small flat stones were found in the region of the trigone: on removal of these a small hole presented itself which barely admitted the index finger. This led to a large cavity, the outlines of which could not be reached. On withdrawal of the finger a large amount of thick fœtid pus welled up, a catheter was introduced, and copious lavage followed. The entrance to the diverticulum was centrally situated half an inch below the ureteric orifices, from which urine was observed to be passing.

In view of the septic condition it was thought advisable to defer further interference, and a large tube was inserted for drainage into the bladder. For a fortnight the diverticulum was freely irrigated with oxycyanide of mercury. Under a general anæsthetic the bladder was freely exposed, and the peritoneum stripped upwards. The upper margin of the diverticulum was found to extend upwards to within half an inch of the fundus of the bladder posteriorly, and, upon the commencement of separation of the two sacs, no difficulty occurred.

Attention was now turned to the opening of the diverticulum. As the lower limits of the sac could not be determined, an anastomosis was decided upon. The position of the ureters was defined by the passage of a probe and an incision was carried upwards in the middle line from the upper border of the opening. The cut edges of the mucous membrane of the bladder and diverticulum were sutured on either side and the opening thus formed admitted three opposed fingers: the lower limit of the diverticulum could still not be

felt. The depth of the incision of its apex was over half an inch, and it was noticed that there seemed to be a distinct line of cleavage between the two sacs. A suprapubic drainage tube was inserted, and the wound healed in a fortnight's time. Micturition was performed every three hours during the day and twice during the night—the prone position helping to empty the bladder more freely. The urine was clear, with only occasional shreds of pus.

Six weeks after the operation, November, 1920, cystoscopy was performed under a local anæsthetic. The anastomosis was widely open and healthy, and the point of the cystoscope was passed through: clouds of pus, however, obscured the view, and repeated lavage was of no avail.

The patient's condition remained satisfactory for six months, when he was admitted with acute cystitis, blood and large quantities of pus being present in the urine. After preliminary lavage it was decided to attempt to remove the diverticulum. The introduction of potassium bromide, 15 gr. to the ounce, for the purpose of radiography, caused intense pain.

On May 31, 1921, the patient was anæsthetized and placed in the Trendelenburg position. The bladder was filled and a soft catheter left *in situ* and clamped. Upon the bladder being exposed, the peritoneum was found to be adherent to the fundus, the abdominal cavity was opened intraperitoneally and the intestine packed off. The bladder and diverticulum filled the pelvis so that the hand could not be passed into Douglas's pouch. Some of the fluid in the bladder was run off, and, commencing from a median vertical incision through the peritoneum covering the fundus and back of bladder, the peritoneum was stripped off on either side. By digital manipulation the diverticulum was freed laterally as far as the attachments of the ureters to the bladder, and posteriorly from the pelvic floor. The difficulty in separating the lower end anteriorly disposed of the hope of removing the diverticulum in entirety. The bladder was then emptied, an incision made in the fundus and continued backwards and downwards to the anastomotic opening. The lower portion of the diverticulum below this opening was separated off and the two halves removed by scissors. After suture of the bladder and suprapubic wound, a large drainage tube was passed from the pelvic cavity through the coccyx, and the peritoneal toilet completed. Hæmorrhage was slight. The patient made uninterrupted, though slow progress, and after considerable shock, and the wounds were healed within a few weeks.

Carcinoma of the Bladder.
 Case 1921: He is now in good health and micturition is normal, the bladder holds 25 oz. without discomfort. The bladder is healthy. On the anterior wall is a patch of granulation tissue, where it spreads over on either side, forming two lobes, between the ureteric orifices is a ridge, most marked at the fundus, where it spreads over on either side, forming two lobes. The ureters open on either side of this ridge in a slanting position. The operation is described by Sampson Handley. There is no obstruction to the passage of the catheter and indigo-carminic is ejected freely: both ureters are patent. The use of the catheter and indigo-carminic is not known whether it has been used previously. The operation is possible after the operation.

He is now in good health and micturition is normal, the bladder holds 25 oz. without discomfort. The bladder is healthy. On the anterior wall is a patch of granulation tissue, where it spreads over on either side, forming two lobes, between the ureteric orifices is a ridge, most marked at the fundus, where it spreads over on either side, forming two lobes. The ureters open on either side of this ridge in a slanting position. The operation is described by Sampson Handley. There is no obstruction to the passage of the catheter and indigo-carminic is ejected freely: both ureters are patent. The use of the catheter and indigo-carminic is not known whether it has been used previously. The operation is possible after the operation.

appeared; this suggested that the endothelium of the bladder was loosely attached to the sides of these crypts, or alternatively, was not adherent to the base, as no movement of the crypt wall was observed at the same time. The origin of these crypts seems obscure.

Case of Ureterocele.

By JAS. B. MACALPINE, F.R.C.S.

THE specimen is a cyst removed from the vesical end of the left ureter. It was about the size of a pigeon's egg. It consists of a double layer of mucous membrane, the two layers lying back to back: the inner layer being derived from the ureteral mucosa and the external layer from the bladder mucosa. A bristle will be seen to traverse a minute aperture in the wall of the cyst, this being the ureteric orifice. It will be seen to be excentrically placed, lying not on the apex, but to one side of the cyst.

My patient was a young married lady, aged 26. She had suffered from hæmaturia at intervals for a period of six months. Sometimes the blood was absent for as long as three weeks at a time, at others it was constantly present over a period of several weeks. Clotting had been observed, but was not characteristic. Hypogastric pain had been present during the attacks and was of a short cutting character. No pain was present between the attacks of hæmaturia and there was no discomfort referable to the kidney area. Her general condition was good. External examination negative. Urine acid, specific gravity 1022; no abnormal constituents. On cystoscopic examination, a large semitranslucent cyst was seen arising from the neighbourhood of the left ureteric orifice; it appeared to be covered by healthy bladder mucous membrane on which were numerous vessels of varying size. Orifice of the ureter not visible.

Operation: I opened the bladder by the suprapubic route. The cyst was steadied by forceps and snipped off close to the bladder wall by means of a pair of curved scissors. Hæmorrhage was quite insignificant and did not require the application of any ligature. The cyst wall was very thin and consisted merely of an inner and an outer layer of mucous membrane lying in close contact. It sprang from a circular origin which enclosed the ureter and had a diameter of about $\frac{1}{2}$ in. At the line of incision the two layers of mucous membrane were in close contact and therefore the area denuded was represented by a thin circular line lying about $\frac{1}{4}$ in. distant from the edge of the ureteric orifice proper. No improvement in the condition of this linear wound could have been effected by the insertion of sutures—they were, therefore, omitted. I closed the bladder in the usual way.

Convalescence was uneventful and the patient returned home at the end of three weeks. Five months later I again cystoscoped the patient. The ureteric orifice was widely open, and healing has been clean and neat around it. A ureteric catheter was slipped in and a pyelogram of the corresponding renal pelvis was obtained. It shows a minor degree of pelvic dilatation and clubbing of the renal calyces.

The underlying factor in the production of this condition is the small size of the orifice of the ureter. The stenosis appears to affect only the mucous coat of the tube. In the majority of reported cases in which a microscopic examination has been made, there have been present only two layers of the

mucosa, with a small amount of sub-mucous areolar tissue. In a very few muscular tissue has also been present, but it appears that as a rule the mucosa is the only structure affected, and that the other coats undergo dilatation and perhaps hypertrophy. My own specimen has not been cut as yet, for I decided to show it as a whole; when it is cut it seems that the section ought to be made to pass through the opening of the ureter.

Specimen of Cystin Calculi from the Kidney of a Child.

By JAS. B. MACALPINE, F.R.C.S.

THESE six calculi require little description; they are typical of the soft greenish-yellow stones composed of cystin. One is large, and occupied the pelvis of the kidney, and thus acquired its shape and proportions. On its surface are several faceted projections which occupied the calyces of the kidney, and with which the smaller stones articulated.

The patient was a boy aged 8, who came with typical renal pain in the left kidney area. An X-ray photograph showed good shadows of the calculi: on examination of the urine, large quantities of cystin plates were observed, and the probable condition of the stones was suspected. The urine was sterile. The stones were removed by nephro-lithotomy, and the child made a rapid recovery.

As the disease of cystinuria is familial, I obtained the urines of the various members of the child's family; these were seven in all. In three cases cystin crystals were found in the urines, those of the mother, a sister, and a brother. It is notable that in all cases the urines emitted a very foul odour, which was present even in the urines which did not show crystals. A search for putrescin and cadaverin was made in all cases, but in no case were they found. The smell appeared to result from the liberation of sulphur as sulphuretted hydrogen, and was possibly due to the decomposition of the sulphur amino-group, which occurs when such urines are not obtained perfectly fresh.

Large Renal Calculus and the Kidney from which it was removed.

By SYDNEY G. MACDONALD, F.R.C.S.

PATIENT, female, aged 56. Hæmaturia once, twenty years ago. No other renal symptoms beyond a drag in the right loin. The stone could be felt in the loin. Cystoscopy and catheterization of the ureters revealed a grossly infected right kidney; the left kidney was normal. Nephrectomy. The stone weighed 13 oz. immediately after removal.

Section of Urology.

President—Sir THOMAS HORDER, M.D.

The Source of the Amyolytic Ferment of the Urine.

By P. J. CAMMIDGE, M.D.

THE presence in the blood of an amyolytic, or diastatic, ferment capable of converting starch into dextrin and sugar was described by Majendie in 1846.¹ In 1863, Cohnheim² discovered a similar enzyme in the urine. These observations were subsequently confirmed by other investigators, but, although numerous attempts were made to apply them practically, especially in the diagnosis of kidney diseases, it was not until Wohlgenuth³ published a comparatively simple and accurate method for their quantitative determination in 1908, that the estimation of the diastatic values of the blood and urine became of clinical importance. By means of his method, Wohlgenuth showed that less of the ferment is excreted by a damaged than by a healthy kidney, and Hirata,⁴ working under his direction, proved experimentally that nephritis artificially produced in animals results in a diminution of the diastatic power of the urine, with a coincident rise of the ferment in the blood. From these experiments it seemed clear that the amyolytic ferment of the urine is derived from the blood, and Wohlgenuth came to the conclusion that the ferment in the blood, in its turn, is probably derived from the digestive organs, most likely from the pancreas. Confirmation of this theory was apparently supplied by the results of experiments carried out in conjunction with Noguchi upon dogs,⁵ for they found that after resection of the pancreas the diastatic power of the blood rose, increasing in one experiment from 8 to 800 units in twenty-four hours, and to 1,600 units in forty-eight hours, with corresponding changes in the urinary amyolytic ferment. Observations on the human subject tended to support the view, since it was found that accidental injuries of the pancreas were associated with an increase in the diastatic value of the urine above the normal maximum of 32 units. Numerous workers have since confirmed Wohlgenuth's clinical results and showed that a study of the diastatic values of the blood and urine supplies valuable evidence in the diagnosis of renal and pancreatic disorders, a conclusion with which my own observations are in agreement. In this paper, therefore, I do not propose to consider the diagnostic importance of estimating the amyolytic ferment in the blood and urine, but shall confine myself to a brief account of experimental evidence I have been able to obtain throwing doubt on the now generally accepted view that the primary source of these enzymes is the pancreas.

The observations upon animals, I have carried out, in conjunction with Mr. Cairns Forsyth and Mr. Howard,⁶ have shown that the diastatic ferment of the blood of healthy dogs and rabbits averages about 2 units per cent. in the fasting condition. Following a meal it gradually rises and at the third hour reaches a maximum of 4 to 5 units: thereafter it sinks, returning to the fasting level again about the fifth or sixth hour. If the blood of a dog from

¹ *Compt. rend. Acad. d. Sci.*, 1846, xxiii, p. 189.

² *Virchow's Arch.*, 1863, xxviii, p. 241.

³ *Biochem. Zeitschr.*, 1908, ix, p. i; and 1909, xxi, p. 381.

⁴ *Biochem. Zeitschr.*, 1910, xxviii, p. 23.

⁵ *Berl. klin. Woch.*, 1912, xlix, p. 1069.

⁶ *Lancet*, 1920, August 21, p. 393; 1921, May 14, p. 1017.

which a small proportion of the pancreas (about one-third) has been removed is examined it is found that the fasting level of the amylolytic ferment is slightly raised, to about 7 units per cent., and that the ingestion of food causes a more marked rise, up to about 30 units, than in a normal animal. When still more of the gland is excised a further increase in the diastase content of the blood occurs, a fasting level of 20 units and a maximum of 45 units, three hours after a meal, being found in a dog from which two-thirds of the pancreas has been removed. So far as they show that interference with the functions of the pancreas causes an increase in the diastatic power of the blood, these results are in agreement with those obtained by Wohlgemuth and Noguchi, but the fact that the ferment value rises as the size of the gland is diminished is hardly what would be expected if their theory as to its origin is correct. When we come to consider the effects of total extirpation of the gland it is certain that their explanation cannot be true, for we found that there is a still further rise, a fasting value of 50 units and a maximum of 85 units three hours after a meal being found. As it is clearly impossible for a non-existent gland to produce an enzyme there must be some other source than the pancreas for the diastatic ferment of the blood and urine.

In the course of a series of experiments upon the relation of the liver to carbohydrate metabolism, we had occasion to investigate the effects of injecting hydrazine phosphate into animals under various conditions and found that, among other effects, a sublethal dose of the drug interfered with the normal appearance of the diastatic ferment of the blood in both dogs and rabbits, so that, although the fasting value remained constant, the usual rise and fall after a meal did not occur for several days after the injection had been given; then as the effects of the poison passed off, the usual values were gradually established, an injected animal at the end of about a week showing no appreciable difference from a healthy one on a similar diet.

When we came to investigate the effects of larger doses we found that, if a rabbit which had received a sublethal dose was given another injection twenty-four hours later, it speedily became seriously ill, refusing all food, passing liquid bile-stained motions, and eventually dying in a comatose condition two or three days later. Examinations of the blood of such an animal demonstrated that the fasting level of the diastatic ferment was not appreciably affected by the first injection, but that twenty-four hours after the second it had sunk to about half the normal value, and the next day had completely disappeared. Post mortem, the liver was found to be enlarged and of a deep yellow colour. Sections showed under the microscope vacuolization of the protoplasm with coarse granular changes, feebly staining nuclei and small localized deposits of fat which stained deeply with osmic acid. No pathological changes, either macroscopic or microscopic, could be found in the pancreas. It is evident, therefore, that a single sublethal injection of hydrazine phosphate prevents the post-prandial rise in the diastase content of the blood found in healthy animals and that a fatal dose reduces the diastase still further, eventually causing it to disappear completely. As it has been shown by Wells,¹ MacAdam² and others, that hydrazine salts have a specific toxic effect on the hepatic parenchyma but cause no pathological changes in the pancreas, a conclusion which our observations tend to confirm, it seems probable that the diminished production of diastase in the body following the

¹ *Journ. Exp. Med.*, 1908, x, p. 457.

² *Journ. Path. and Bact.*, 1913-14, xviii, p. 281.

injection of a sublethal dose of hydrazine phosphate is, in part at least, due to the liver changes. Experiments carried out by Underhill¹ have shown that injections of hydrazine, beside depleting the glycogen stores of the liver, also reduce the amount of glycogen in the muscles, which may in fact become glycogen-free; it is consequently not unlikely that the entire disappearance of the diastase from the blood we found shortly before death might be accounted for by failure of the muscular tissue, as well as of the liver, to form the ferment. While, therefore, it is probable that the diastatic ferment of the blood is derived from the liver and very likely from the muscles, there can be little doubt that the pancreas plays no important part in its formation. The experiments previously quoted have shown, however, that the amyolytic ferment of the blood is intimately related to the functional activity of the pancreas, but, as the ferment increases as the volume of the pancreas is diminished, it would seem likely that this organ in some way controls the formation of diastase by the liver, possibly through its internal secretion, which exerts a brake-like action.

If these conclusions are correct the diastase content of the urine will depend upon three factors: (1) The functional activity of the liver; (2) the integrity of the pancreatic control; and (3) the permeability of the kidneys. Consequently, estimations of the amyolytic ferment in the blood and urine may be helpful in the diagnosis of hepatic disorders as well as in the recognition of pancreatic and renal diseases. Our experience suggests that reliance cannot be placed upon the results obtained with odd specimens, however, but that serial estimations at hourly intervals, before and after a test breakfast, must be made. By comparing the figures given by the blood at the end of each hour with those given by hourly collections of the urine the excretory power of the kidneys may be estimated, and the range of the amyolytic ferment in the blood and urine will also indicate the functional activity of the pancreas and liver. As a rule the percentages of ferment in the blood and urine rise and fall together, although not necessarily to the same degree, but it sometimes happens that the percentage in the urine drops after a meal while the blood rises. Under normal conditions these variations depend upon the amount of urine excreted, for when the total amount of ferment passed is calculated for each hour it is found that it increases more rapidly than the percentage. It is therefore advisable that the urine excreted each hour should be collected separately and measured in order that the dilution due to an excessive output may be allowed for.

Glycosuria in Renal Disorders.

By G. A. HARRISON, M.B., B.Ch.

A REDUCING substance is not uncommonly found in the routine urinary examination of patients with kidney disease. Thus out of the last thirty-three cases whose renal function I have tested, eight have passed small quantities of a reducing substance. In three cases this was definitely shown to be glucose. Often such findings are of little or no importance clinically, but occasionally a knowledge of their possible significance is of the greatest value in treatment.

The object of this paper is not to bring before you any original observa-

¹ *Journ. Biol. Chem.*, 1914, xvii, p. 293.

tions, but to indicate very briefly some ways in which combined clinical and laboratory investigations may reveal the true nature of these glycosurias. The results are none the less valuable in that they frequently indicate that there is no cause for alarm.

THE NATURE OF THE REDUCING SUBSTANCE.

The patient's urine has been found to reduce cupric solutions. The first and obvious question is whether the reducing substance is a sugar, and if so whether it is glucose. The various tests required before answering this question are well known, but I would mention that a combination of the osazone, fermentation and polariscope tests is sufficient for clinical purposes. Lævulosuria, lactosuria, pentosuria, &c., might occasionally accompany renal disorders, but their presence would not influence the treatment of the kidney disease.

CLASSIFICATION.

Glycosuria may either result from, or be merely associated with, the renal disease. It may occur when the renal threshold for dextrose is lowered, or when it is raised. A lowered threshold is found in renal glycosuria (also known as renal diabetes or diabetes innocens). This condition is scarcely embraced by the title of this paper, for it is usually discovered in routine examination (for life insurance, &c.), and it must be extremely rarely that it is associated with structural kidney damage. I wish to refer to it again, however, in considering blood sugar curves. Renal disease has sometimes been proclaimed as the cause of a raised threshold for glucose. It has been suggested, however, that if due allowance be made for the hyperglycæmia of old age, and errors which may be involved in the picric acid methods of estimation, kidney lesions do not of themselves often cause hyperglycæmia. In any case it is difficult to see how renal damage could at one and at the same time be responsible for both hyperglycæmia and glycouresis, and therefore strictly speaking, we are not concerned with this controversy to-night. On the other hand, disease of the kidney and disease of another part of the body causing hyperglycæmia and glycouresis, may coexist. The most obvious example is the association of diabetes mellitus with a renal lesion.

METHODS OF INVESTIGATION.

How are we to decide as to the significance of the glycosuria?

(a) The clinical findings may rule out the necessity for special investigation: for instance, in untreated advanced diabetes mellitus. I am not particularly concerned with such cases this evening. There are, however, a number of "potential" diabetics whose condition can only be diagnosed with the aid of laboratory methods.

(b) A blood-sugar curve obtained after 50 gm. of dextrose by mouth four hours or more after the last meal, is a valuable test of the carbohydrate storing mechanism. A low or normal curve together with the urinary and clinical findings, will settle whether the case is one of renal glycosuria. A prolonged and high form of curve demonstrates hyperglycæmia, a diminished power to store carbohydrate, of which the causes are many. If a cause is demonstrable I would term the condition "glycosuria"—pancreatic, hepatic, thyroid glycosuria, &c. If no cause is found then I would term the condition "diabetes mellitus," which may exist in various grades, from the fully

developed case with typical signs and symptoms to the "potential" case with a prolonged and raised blood sugar curve but without symptoms or signs other than the presence of glucose in the urine.

(c) Other urinary findings (ketonuria, oxaluria, &c.), faecal examination and so on, all have their special bearings in testing these cases of glycosuria.

TREATMENT.

The treatment of renal glycosuria is *nil*, beyond reassuring the patient, which may be of the greatest importance.

When the "leak point" is raised, and the cause of the hyperglycaemia is recognized, the relative importance of this cause and of the kidney lesion demands consideration. Thus slight glycosuria may accompany albuminuria in some toxæmias of pregnancy, and this glycosuria is usually of little importance. Similar remarks apply to a combination of hepatic glycosuria and chronic nephritis, glycosuria and uræmia, &c. On the other hand, the albuminuria may be of small moment, for instance, in some cases of exophthalmic goitre, cirrhosis of the liver, &c.

When true diabetes mellitus is responsible for the raised threshold, treatment depends upon the severity of the diabetes and the urgency of the renal condition. The advisability of treating the diabetes first whenever possible is generally admitted, even in cases in which the diabetes is associated with surgical diseases of the kidney of a chronic nature. But when operation is urgently required, we may be faced with a very difficult problem.

ILLUSTRATIVE CASES.¹

The following cases are recorded briefly, to illustrate some of the points mentioned:—

Case I: Renal Glycosuria (M. 25).—The reducing substance was discovered during an examination for life insurance and was shown to be glucose. There was no polyphagia, no polydipsia, no polyuria, and no loss of weight. No family history of diabetes was obtained. About 1,500 c.c. of urine were passed in the twenty-four hours containing 0.1 to 1.0 per cent. of sugar. The blood-sugar curve was typical, with a lowered threshold for glucose (fig. 1). There was no evidence of renal disease, clinically, or by renal function tests. No ketonuria was demonstrated. The systolic blood-pressure was 144, and the diastolic, 108. The Wassermann reaction was negative. Treatment consisted solely in reassuring the patient.

Case II: Cirrhosis of the Liver; Intermittent Glycosuria; Albuminuria (M. 61).—A typical case of cirrhosis with slight ascites. There was a history of considerable alcoholic excesses. He was referred to King's College Hospital as a pensioner suffering from "chronic nephritis." A reducing substance was found in routine examination and demonstrated to be glucose. It varied from 0 to 3 per cent. Albuminuria 0.14 per cent. (Aufrecht); but no ketonuria. The deposit showed leucocytes, a few red blood corpuscles and epithelial cells, but no casts. The blood-sugar was 0.40 and 0.28 per cent. on two occasions when the patient was glycosuric, and 0.12 per cent. when aglycosuric. Pancreatic function tests were negative. A skiagram showed no enlargement of the pituitary fossa. The urea concentration test was 3.34 per cent., and the blood urea 26 mgr. per 100 c.c. Diastase, in plasma, 6.7 units; in urine, 4 units (1,430 c.c.) and 2 units (1,150 c.c.). The patient was able to tolerate 385 gm. of carbohydrate, 200 gm. of protein and 120 gm. of fat daily without glycosuria. Subsequently as an out-patient sugar alone was forbidden. The result was slight intermittent glycosuria, but no ketonuria. He has been under observation for eighteen

¹ These cases will be included in a future report to the Medical Research Council.

months. His cirrhosis and ascites have become slowly worse, but he has not developed any diabetic symptoms. The albuminuria is regarded as of secondary importance.

Case III: Bilateral Renal Calculus; Potential Diabetes Mellitus (M. 48).—The reducing substance was discovered during routine urinary examination before operation, and was identified as glucose. There was no polyphagia, no polydipsia, no polyuria, and no loss of weight. His father had diabetes. About 1,500 c.c. of urine containing 1 per cent. sugar was passed in the twenty-four hours. The glycosuria varied with the diet. The blood-sugar curve is shown in the diagram (fig. 2). There was difficulty in obtaining urine, but less than 1 gm. of dextrose was excreted in the three hours following 50 gm. by mouth. No cause was found for the glycosuria. There was no clinical evidence of disease in any of the ductless glands. The systolic blood-pressure was 108, and the diastolic, 85. Pancreatic function tests were negative. The renal function tests were satisfactory from the point of view of an operation. Urea concentration test, 2.22 per cent. Blood urea (two and a half hours after the dose of urea) 73 mgr. per 100 c.c. Urea concentration factor (third hour after urea) 30. The blood diastase was 8 units, and the urinary diastase 16.7 units (volume 1,420 c.c.). A moderate

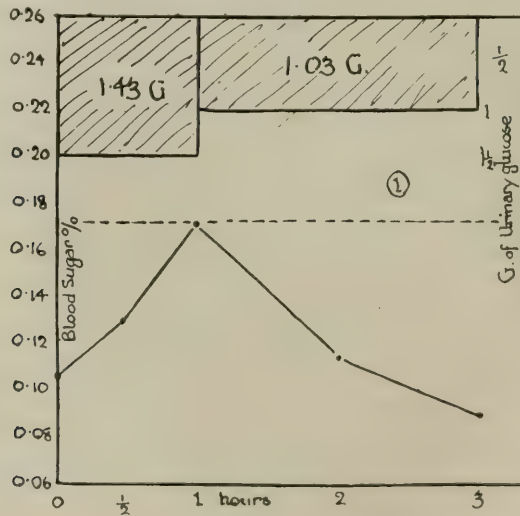


FIG. 1 (Case I).

restriction of carbohydrate, and a considerable reduction of fat was enforced for a few days before operation. The urine became free from sugar and no ketonuria was produced. Successful left nephrolithotomy was performed by Sir John Thomson Walker. Healing was slow but uneventful. Slight glycosuria but no ketonuria followed. The glycosuria was easily controlled by reducing the diet slightly. The patient was discharged sugar free and without ketonuria on a diet in which sugar was forbidden and starches were moderately reduced.

Case IV: Mild Diabetes Mellitus; slightly enlarged Prostate (M. 69).—An out-patient under Mr. Eyeridge complaining of difficulty in micturition. A slightly enlarged prostate was found. There was a history of diabetes of four years' duration. There was no polyphagia and no loss of weight, but polydipsia, polyuria and pruritus were present. No family history of diabetes was obtained. 2,000 to 2,500 c.c. of urine containing 2 per cent. glucose were passed in the twenty-four hours, on a diet from which sugar was excluded. Albuminuria, a trace. The deposit showed leucocytes and

a few epithelial cells, but no casts. Ketonuria, *nil*. The blood-sugar curve is shown on the screen (fig. 3). No cause was found for the glycosuria. All the ductless glands were normal clinically. A skiagram revealed no abnormality of the pituitary fossa. Blood-pressure: Systolic, 126; diastolic, 110. Pancreatic function tests were negative. The renal function tests were quite satisfactory. Fat was reduced to a minimum for a few days, without alteration of the protein and carbohydrate. Carbohydrate was then gradually reduced. The patient became sugar-free on a diet of carbohydrate 120 gm., protein 90 gm., and fat 40 gm., without any day of starvation, and no ketonuria resulted. He has remained free from sugar and acetone bodies for two months on a diet of 160 gm. carbohydrate, 100 gm. protein and 120 gm. fat, and his symptoms have disappeared. Prostatectomy is at present considered unnecessary, but we have considerable knowledge of his condition should the day for its performance arrive.

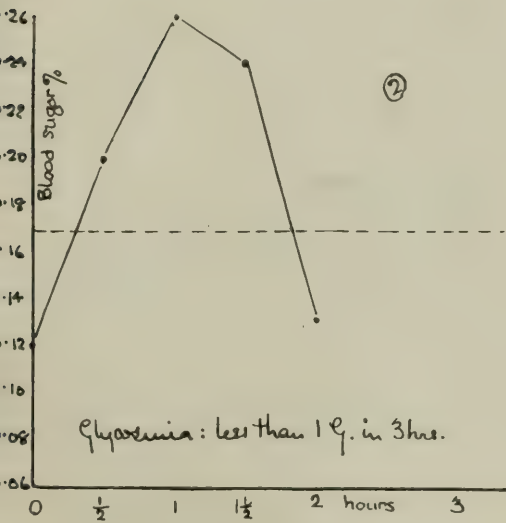


FIG. 2 (Case III).

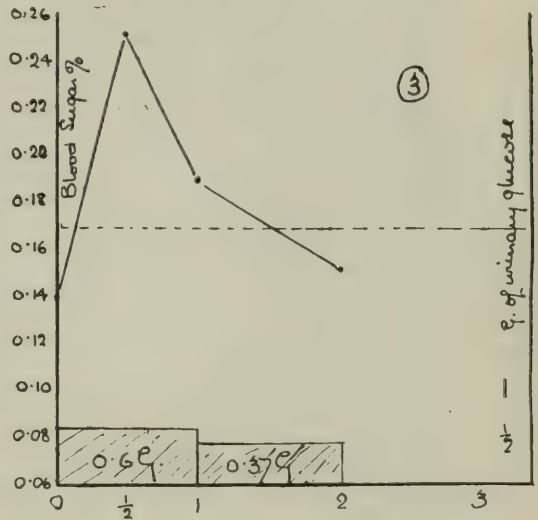


FIG. 3 (Case IV).

The table shows the main findings in the four cases for the pancreatic and kidney function tests.

Disease—	Case I Renal glycosuria	Case II Cirrhosis of liver, intermittent glycosuria, albuminuria	Case III Bilateral renal calculus, potential anaemias	Case IV Enlarged prostate, mild diabetes
Glycosuria percentage ...	0.1—1.0	0.0—3.0	1.0	2.0
Blood-sugar percentage ...	0.09—0.17	0.12—0.40	0.12—0.26	0.13—0.25
Loewi's test ...	Negative	Negative	Negative	Negative
Steatorrhœa and creatorrhœa ...	<i>Nil</i>	<i>Nil</i>	<i>Nil</i>	<i>Nil</i>
Diastase in blood ...	8	6.7	8	4
Diastase in urine ...	8	4 (1,430 c.c.)	16.7 (1,420 c.c.)	1 (2,370 c.c.)
Urea concentration test ...	2.28	3.31	2.22	2.89
Blood urea ...	—	26	73*	70*
Urea concentration factor ...	—	—	30*	41*

* In third hour after urea.

SUMMARY.

(1) The nature of the urinary reducing substance should be demonstrated. Substances other than glucose are relatively unimportant.

(2) When glycosuria is present, for which the cause is not obvious, a blood-sugar curve should be obtained. If the curve is normal or low the glycosuria is of little moment.

(3) If the curve is prolonged and raised the cause should be discovered, if possible, and the relative importance of this cause and of the renal disorder should be decided.

Dr. LEYTON said that he would refer to the chief points in the paper he had intended to read that night whilst discussing some of the questions raised by Dr. Harrison's paper. He referred first to the question as to whether cases of so-called "renal glycosuria" developed diabetes mellitus. During the last two years he had seen twenty cases of this condition, which he thought should not be referred to as renal glycosuria, but as glycosuria without hyperglycæmia. Of these, six averaged eighteen years since glycosuria had been first recognized, but none presented any of the signs or symptoms of diabetes mellitus. Glycosuria without hyperglycæmia did not protect against diabetes mellitus, and therefore it was always within the limits of possibility that an individual who happened to have that peculiarity would be attacked by diabetes mellitus, just as he might develop a new growth. The objection to the term "renal glycosuria" was that there was no definite proof that glycosuria was of renal origin. It might be due to some substances circulating in the blood, which could be altered by the normal kidney and excreted as dextrose: or it might be, as Dr. Cammidge suggested, that it was due to an altered salt concentration of the blood. The question as to when a glycosuria was negligible or was a very early diabetes mellitus was not easy to answer. He referred to a case to illustrate the difficulty, and then pointed out why it was not easy to be dogmatic: A man aged 36, attempted to insure his life but was refused. He sought the opinion of a physician to learn why he had been refused. The physician failed to find any disease in him, and as a favour was informed by the insurance company, that sugar had been found in the urine of the applicant. The man was referred to him, and was examined at 3 o'clock in the afternoon, bringing with him a specimen of urine passed at 11 a.m. That specimen was free from sugar, but the urine passed at 3 p.m. contained 0.7 per cent. of sugar, whilst the blood sugar was 0.11 per cent. The man had had thick soup and two pancakes for lunch at 1 o'clock. Obviously no conclusion could be drawn. At 9.50 a.m., he took 50 grm. of dextrose. At 10.10 a.m. his blood sugar was 0.18 per cent.; at 10.25 a.m. it had fallen to 0.14 per cent.; at 11 a.m. it was 0.1 per cent., whilst the urine collected contained 0.7 per cent. Dr. Leyton asked whether this person was a case of negligible glycosuria, or a case of early diabetes mellitus, and suggested that the fact that the father suffered from diabetes mellitus might add to the difficulty in arriving at a diagnosis. Amongst the negligible glycosurias, the glycosuria without hyperglycæmia in which sugar appeared in a recognizable quantity in the urine even during fasts was well known, but cases in which the sugar was occasionally completely absent were at any rate rare. There were apparently other types of negligible glycosuria, too; and in these not only did the quantity of sugar in the urine vary to some slight extent with the quantity of carbohydrate in the diet, but the blood sugar, too, might rise for a comparatively short period after food. That they were negligible followed from the fact that in three of the cases that had come under his observation, sugar had been discovered in the urine some twenty years previously, and in spite of the absence of treatment, no symptoms of diabetes mellitus had developed.

Section of Urology.

President—Sir THOMAS HORDER, M.D.

Ascending Infections of the Kidney.

By KENNETH M. WALKER, M.B., B.Ch., F.R.C.S.

THE changes that have occurred in our ideas concerning the pathology of urinary infections afford an excellent example of the way in which judgment tends to swing from one extreme to another, in a controversial subject. Twenty years ago many of the forms of infection of the kidney that we now know to be blood-borne were regarded as "ascending" and the spread of infection from the bladder to the kidney was considered to be a common event. However, the experimental work of Baureisen, Draper and Braasch showed that organisms could not ascend the ureter against the urinary current unless there was incompetence of the sphincter guarding the opening of the ureter into the bladder—a condition by no means common in urinary practice, and certainly not present in the majority of cases which up to that time had been cited as examples of ascending infection. As a result the pendulum of opinion swung to the opposite extreme and all renal infections were believed to be blood-borne, the existence of ascending infection being denied except as a complication of severe lesions, such as retention. This view, although nearer the truth than the preceding one, did not sufficiently recognize the possibility of infection by other routes, such as the lymphatics. I therefore, propose to discuss almost exclusively lymphatic infections of the kidney. In doing so I do not wish to suggest that in actual practice this route of infection is nearly so common as infection by the blood stream, but merely to furnish what I believe to be conclusive evidence that such a mode of infection exists, and that it must always be taken into account when the pathology of an infected kidney is being considered.

EXPERIMENTAL EVIDENCE.

Paladino-Blandini was the first to show that organisms placed in the urethra of a guinea-pig could be recovered from the upper end of the ureter at the end of twelve hours. In 1909 I repeated Blandini's experiments, employing *Bacillus prodigiosus* on account of the ease with which it could be identified in culture. The guinea-pigs were killed from twelve to eighteen hours after inoculation of the urethra and cultures were made from various portions of the urinary tract. The following table gives the results of the first four experiments:—

TABLE OF RESULTS OF FOUR INOCULATIONS OF *Bacillus prodigiosus* INTO ANTERIOR URETHRA OF GUINEA-PIGS.

	Guinea-pig	...	A	...	B	...	C	...	D†
Hours after inoculation	13	...	13	...	13	...	13
Heart's blood	S	...	S	...	S	...	S
Urine	S	...	S	...	S	...	S
Upper end of ureter	G	...	G	...	G	...	G
Kidney (stripped of capsule)	S	...	S	...	S	...	S

S = Sterile. G = Growth.

† The inoculation was also made into the posterior urethra.

With a view to obtaining some insight into the path along which organisms did reach the upper end of the ureter I tried inoculating the urethra with inorganic particles of carmine or of ferrous carbonate, searching afterwards by means of microscopic sections for their presence in the tissues. Where ferrous carbonate had been employed the sections were treated with ferrocyanide and hydrochloric acid, in order to demonstrate the iron granules. Although not conclusive, the experiments were sufficient to show that the granules

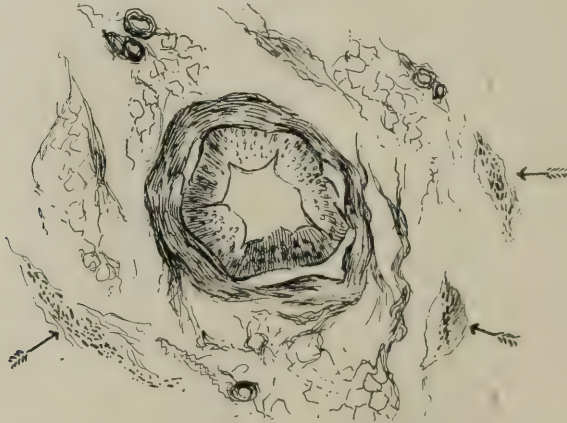


FIG. 1.—Experimental inoculation of the urethra of a guinea-pig with diphtheroids and staphylococci. Section shows numerous organisms (indicated by arrows) in peri-ureteral lymphatics twelve hours after inoculation.

reached the upper end of the ureters just as the micro-organisms had done. Moreover a valuable hint was given as to the path they had taken; whereas only a few scanty granules of iron were seen in the submucous coat of the ureter, more numerous specks were found in the lymphatics running outside the ureter. In addition to this an unexpected disposition of granules was discovered in the kidneys. Although no trace of iron could be found in the substance of the kidney unmistakable granules were seen lying in its capsule. The experiments suggested, therefore, that instead of travelling along the lumen of the ureter, organisms reached the kidney by way of the peri-ureteral lymphatics. In order to test this, the ureter of a guinea-pig was exposed on one side close to the bladder, carefully separated from surrounding lymphatics and then ligatured. It was found that ligature of the ureter without interference with the surrounding lymphatics did not prevent organisms from reaching the upper end of the ureter. Moreover a hint was taken from the

results of the previous experiments and cultures were made from the capsule of the kidney. A luxuriant growth was obtained from the capsule even when the kidney itself proved sterile. In addition to this, inoculation of the urethra with a Gram-positive organism was undertaken. Fig. 1 shows a transverse section through the ureter of a guinea-pig twelve hours after inoculation of the urethra with a mixture of diphtheroids and staphylococci. It will be seen that the peri-ureteral lymphatics are crowded with organisms. A large number of cocci and bacilli were also found lying in the capsule of the kidney.

CONCLUSIONS DERIVED FROM EXPERIMENTAL INVESTIGATIONS.

The net result of these animal experiments was, therefore, to prove the following:—

(1) That organisms placed in the lower urinary passages could reach the kidney via the peri-ureteral lymphatics.

(2) That the renal capsule formed an important link in the lymphatic chain that connected the upper and lower urinary tract, and was the first situation in which organisms were found in cases of renal infection occurring by the lymphatic route.

These results were confirmed in 1913 by Teale and Embleton working with *Bacillus mycoides*.

POST-MORTEM DEMONSTRATION OF LYMPHATIC INFECTION.

Recognizing that laboratory experiments can only reproduce to a greater or lesser extent the conditions that exist in clinical practice, I have taken every opportunity of testing my results in post-mortem cases of renal infection. As an example may be cited the case of a man aged 76, who died forty hours after enucleation of the prostate. The prostatic capsule, bladder, ureters, with surrounding tissue, and the kidneys with a certain amount of suprarenal fat, were removed for histological examination. Numerous paraffin sections were cut, stained for organisms, and carefully examined. Fig. 2 represents diagrammatically the areas submitted to examination and the situations in which organisms were found. Sections were first made of the capsule of the prostate with adherent particles of prostate. Organisms were found in this situation in such numbers that they formed dark lines clearly visible under a low power. The section somewhat resembled that of the uterus in a case of puerperal infection, the organisms streaming back from the surface to the lymphatics running to the fibrous capsule of the prostate. They were next found in the lymphatics surrounding the ureter, the appearance of a section in this situation being strongly reminiscent of the similar section prepared from a guinea-pig after urethral inoculation with staphylococcus and diphtheroids. Sections of the pedicle of the kidney, the pelvis, and a calyx all proved negative. A section through the capsule and cortex, however, showed the presence of a small round-celled infiltration just beneath the capsule, and the existence of organisms in the lymphatics of the capsule. The results of the examination were, therefore, precisely similar to those obtained by animal inoculation.

COURSE OF THE RENAL LYMPHATICS.

At first sight it may seem surprising that the lymphatics of the urinary tract should provide a path along which organisms can reach the kidney. The lymphatic route that links together the upper and lower portions of the urinary

tract is at best indirect, and at certain points the direction of the main lymphatic current is against the invading organisms. However, too much stress must not be laid on this latter point, for there is ample evidence to show that the sluggish and wayward lymphatic flow, with its countless backwaters and cross-currents, can offer but little obstruction to the progress of organisms.

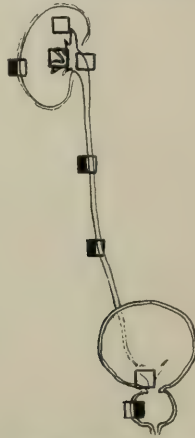


FIG. 2.—Prostatectomy case. The squares represent the portions of the urinary tract examined for organisms. The positions in which the organisms were found are marked in black, i.e., the prostatic capsule, the peri-ureteral tissues, and the renal capsule.

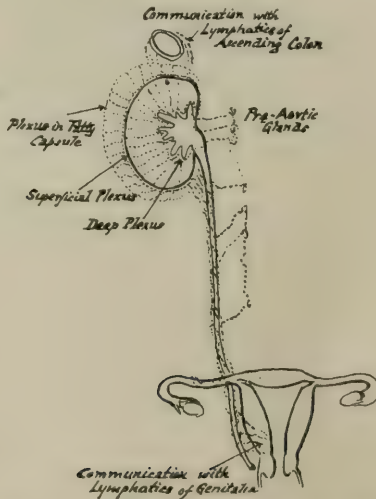


FIG. 3.—Diagrammatic representation of lymphatics of the urinary tract showing communication with those of the colon and of the genitalia.

Moreover, Sakata has shown that free communication exists between the lymphatics of the upper and middle ureter and between those of the middle ureter and the lymphatics of the bladder. Still more interesting is the fact that the urinary lymphatics freely communicate with those of the female genitalia, and on the right side with those of the ascending colon (Francke) (fig. 3).

A CASE OF RENAL INFECTION ASCENDING FROM THE GENITALIA.

The following case may be cited as an example of an ascending renal infection, resulting from a focus in the female genitalia:—

F. S., aged 32, admitted February 3, 1922, to the Royal Northern Hospital for dysmenorrhœa. On the 10th, under general anæsthesia, cervix fully dilated and endometrium curetted; large stem inserted. During dilatation of cervix the tissues were felt to give suddenly, as though a tear had occurred. On the 14th, pain and tenderness on right side of abdomen and down right leg; pain on micturition. On the 15th severe pain in right iliac fossa, with tenderness and resistance. *Per vaginam*: Uterus tender but not distinctly enlarged; thickening in right fornix suggestive of parametritis. On the 17th, rigor, vomiting, rise of temperature and pulse. Urine on examination contained pus. Leucocytosis, 21,000. Vomiting continued throughout the day with renal pain and rigor. Expectant treatment adopted, with increased fluids and alkalis. Patient transferred to my care.

Operation: On the 18th vomiting continued. General condition much the same, but only 8 oz. of urine passed in twenty-four hours. In view of pain and suppression of urine an exploratory operation was decided on, a diagnosis of lymphatic infection of the kidney having been made. On exposure of the kidney it was found to be enlarged and engorged with blood. A marked feature was œdema of the areolar tissue around the lower pole of the kidney. This œdematous tissue appeared to be continuous with œdema of the tissues surrounding the ureter. Capsule easily stripped. No pus could be seen, even on incision of cortex. The kidney was decapsulated and returned into position. Sections of capsule, perirenal areolar tissue, from lower pole of kidney, and a small fragment of the cortex were removed for histological examination. Blood-culture, negative; urine grew a coliform bacillus. On the 19th very little pain, 30 oz. of urine passed, fall of temperature and pulse.

Subsequent history: Uninterrupted recovery.

Sections: Paraffin sections stained in methylene-blue showed the following: (1) Section of renal cortex with capsule showed a round-celled infiltration below the capsule with considerable numbers of organisms lying in the capsule itself. No organisms or infiltration seen in the deeper parts of the kidney; (2) Section of capsule: Scattered round-celled infiltration, numerous organisms, both bacilli and cocci; (3) Section of perinephric tissue from lower pole of kidney: Some round-celled infiltration and numerous organisms, similar to those found in renal capsule.

Remarks: The case was evidently one of infection travelling by the retro-peritoneal lymphatics. The point of entry was presumably a tear in the cervix occurring during dilatation. The close relation of the ureter to the cervix rendered it easy for organisms to reach the peri-ureteral lymphatics, and from thence to travel to the plexus in the capsule of the kidney.

TUBERCULOUS INFECTION.

It is impossible not to refer to the bearing that lymphatic spread may have on tubercle of the genito-urinary tract. A tendency has always been shown to place the tubercle bacillus in a class apart and to allocate to it a path of infection and a method of spread different from those of other organisms. In the writer's opinion such discrimination is unjustified both on *a priori* and on *a posteriori* grounds.

EXAMINATION OF A CASE OF ADVANCED GENITO-URINARY TUBERCULOSIS.

In fig. 4 are shown transverse sections of the right and left ureters from a case of advanced genito-urinary tuberculosis. It will be noted that there is a striking difference in the location of the tuberculous infiltration on the two

sides. The mucous membrane and inner coats of the right ureter appear healthy, the tuberculous infiltration mainly affecting the peri-ureteral sheath. On the left side the converse is true, the mucous membrane and inner coats being ulcerated, whereas the outer coat and peri-ureteral sheath show only slight infiltration. These specimens were from a man aged 26, who died with advanced genito-urinary tuberculosis. Fig. 5 is a diagrammatic representation of the location of his lesions, and shows what I believe to have been the path taken by the infection. The primary lesion in the urinary tract was probably the left kidney. From the tuberculous pyelonephritis thus produced tubercle bacilli were carried by the urine to the lower urinary passages, and the bladder and prostate became infected. From thence the infection climbed up the right ureter, not by its lumen, but by means of the peri-ureteral sheath, which

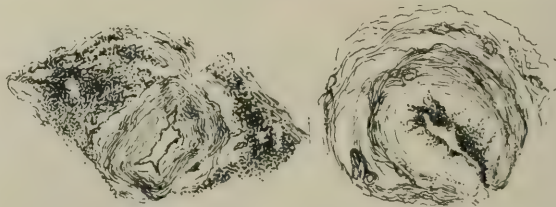


FIG. 4.—Advanced genito-urinary tuberculosis. Transverse sections through the right and left ureters showing tuberculous infiltration of the inner coat on the left side, and of the peri-ureteral tissues on the right side.



FIG. 5.—Advanced genito-urinary tuberculosis. The lesions found post mortem are shown as dark areas, and the suggested course of the disease indicated by arrows.

in the present instance was diseased to a point roughly midway between the bladder and the still healthy kidney. Had the patient lived longer, the disease would probably have reached the level of the kidney, and this organ would also have become infected.

LYMPHATIC INFECTION OF THE KIDNEY A POSSIBLE CAUSE OF CHRONIC NEPHRITIS.

Of the greatest interest is the speculation as to whether the readiness with which organisms reach the kidneys by the lymphatics may not explain the aetiology of certain types of chronic nephritis known to the physician. The

lesion that repeated lymphatic invasion of the kidney by organisms of low virulence would be likely to produce would be fibrosis of the cortical area with thickening of the capsule. The fact that no evidence of chronic irritation of the kidney will be furnished clinically by the presence of pus or organisms in the urine lends some support to the view that chronic nephritis may occasionally be dependent on such a cause.

CONCLUSIONS.

The conclusions to be drawn from the foregoing evidence may be summarized as follows:—

(1) That although the blood-stream is the commonest route by which an infection reaches the kidney, organisms may also reach it by the lymphatics surrounding the ureter and between its muscular coats.

(2) That the kidney capsule is an important link in this lymphatic chain, and is the situation in which organisms are most consistently found in cases of ascending infection.

(3) That in early cases of lymphatic infection of the kidney no organisms are found in the urine.

(4) That there are reasons to believe that a tuberculous infection of the kidney may take place along a route precisely similar to that followed by pyogenic organisms.

These conclusions are not without some bearing on the question of treatment. In the first place they lay stress on the importance of dealing with any form of infection, however trivial it may appear. In the second they raise the question as to whether decapsulation might not occasionally be beneficial as a means of dealing with ascending infections, for, as we have seen, the renal capsule is of supreme importance in all cases of lymphatic spread. Last of all, the conclusions at which we have arrived suggest the possibility that repeated infection of the renal capsule may explain the origin of certain forms of chronic nephritis, and that decapsulation might not be without effect on the progress of such a condition.

I am indebted to my colleague, Dr. T. H. G. Shore, for the specimen illustrated in figs. 4 and 5.

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Case of Horse-shoe Kidney ; Pyelo-radiogram ; Rovsing's Operation for Separation of the fused Kidneys.

By FRANK KIDD, M.Ch.

HISTORY taken on July 29, 1920. Female, aged 32, married, complains of severe aching pain in the back of the sacrum which began four years ago, increased gradually in intensity and for the last year or more has rendered her life



FIG. 1.

unbearable. When she lies on her back she feels as if something inside was pressing her, something which ought not to be there. She has to sleep on her right side to obtain any relief. She wakes up in the night with the pain. It is there every day and is made worse by walking or exertion. It is only felt over the back of the sacrum. At times she experiences a pain down the front of the left leg.

Examination : The face looks drawn and haggard. Abdominal examination reveals a movable kidney on the right side lying in the right iliac fossa. Nothing pathological was found in the pelvis.

Operation, November 8, 1920: I thought I was dealing with an ordinary case of movable kidney, and unfortunately I did not obtain a pyelo-radiogram at the time. I cut down on the right kidney through a modified Mayo's incision from the back, the patient lying on the face on an air bag. On clearing the kidney I found that an isthmus of kidney tissue led forwards and inwards from the lower pole across the aorta to fuse with the left kidney. I closed the wound, which healed uneventfully.

Progress: Fourteen days after she left the hospital, she noticed a return of the pain, which grew worse than ever.

Pyelography, January 18, 1921: I passed a catheter up each ureter and filled up each kidney with 6 c.c. of 20 per cent. sodium bromide solution. The picture shows a horse-shoe kidney (fig. 1). The left kidney is lower than the right and the upper half of the right kidney is out of the picture.

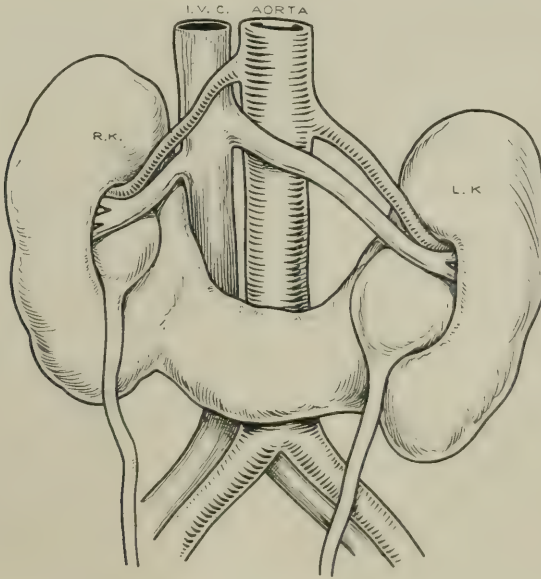


FIG. 2.

Second operation: Rovsing had pointed out (*Zeitschrift für Urologie*, 1911, p. 586) that in some cases a horse-shoe kidney may cause intense pain, presumably by pressing back on the aorta and its surrounding plexus of nerves, and had reported an operation for separation of the isthmus. Brongersma published two successful cases relieved in this manner (*Zeitschrift für Urologie*, 1914, p. 470, and Report of the Nineteenth Congress of the French Association of Urology, 1919, *Journal d'Urologie*, 1919, viii, p. 403). The second operation was carried out on February 2, 1921. The right rectus muscle was turned outwards, the intestines displaced upwards and to the left, the peritoneum divided in the middle line until the kidneys were fully exposed. The condition found is shown in the diagram (fig. 2). The kidneys were fused by a broad band of kidney tissue running across in front of the aorta and pressing back with great tension on it. Considerable force was required to drag the isthmus forward

after it had been cleared of its posterior connexions. I applied a crusher to the isthmus and then surrounded the portion to be cut across with interlocking mattress sutures of catgut driven through the kidney substance with a blunt Cullen's needle. The isthmus was cut across between the ligatures and there was no bleeding. Directly the isthmus had been cut across the right kidney flew up towards the right loin and remained there, the left kidney moved over some 2 in. outwards and a little upwards. The peritoneum and the abdominal wound were stitched up as usual. A pad was placed in the middle line to keep the kidneys from running together again, the patient being placed with head down and feet up.

Progress was uneventful for three days, but then the patient developed acute dilatation of the stomach. Despite repeated lavage and injections of strychnine and pituitrin the patient went down-hill and died on the fifth day from a dilated stomach.

Two further cases of successful operation for separation of fused kidneys appear in the *Zeitschrift für urologische Chirurgie*, March, 1922, pp. 165, 170, reported respectively by Van Houtum and de Groot, both of the Hague.

Sect. Urol. 16. 20

Section of Urology.

President—Sir THOMAS HORDER, M.D.

DISCUSSION ON THE TESTS OF RENAL FUNCTION.¹

Sir JOHN THOMSON-WALKER.

TWO criteria will govern the selection of the tests of the renal function :—

- (1) Ease of application.
- (2) Reliability.

It is impossible, of course, that ease of application will bolster up the use of an unreliable test for very long, or with any observer who is prepared to approach the subject with an open mind. But where two tests are of approximately equal value, that which is more easily applied will be more readily and widely adopted. Reliability is the final criterion by which all the tests must be judged, and it is on this that all the tests of the renal function hold their place or fall out.

Most of the speakers have concentrated their attention on two classes of test, namely, the colour tests and the urea tests. Before passing to discuss these I shall note two tests that have received less attention, namely, the electrical conductivity of the urine and blood and the diastase test.

The electrical conductivity of the urine test labours under two disabilities : A delicate electrical mechanism is required for its performance, therefore it is not easy of application ; and, secondly, it is quite unreliable. The electrical conductivity of a fluid such as the urine or blood depends upon the molecular concentration of the fluid. In addition to this method there are others by which the molecular concentration of the urine and blood is measured, such as the estimation of the freezing point (kryoscopy) and measurement of the specific gravity.

In the early days of renal function tests these methods were much in use, and I have worked with all of them. But it soon became apparent that the molecular concentration of the blood was constant under the most varying conditions, and that of the urine varied within wide limits under quite normal conditions ; so that neither examination of the urine or of the blood taken separately was of any value as indicating the renal efficiency. Nor did the combination of the two under the name of the hæmo-renal index give accurate results. So the method was abandoned as worthless. No new work in regard to this test has been produced, and a mere expression of confidence has not sufficient weight to revive it at the present time.

The diastase test has only received support from one speaker. The chief difficulty in this test is the want of reliable figures of the normal limits of variation. Until more work is done in this direction, the test must take a second place.

¹Adjourned from the meeting of November 24, 1921. See *Proceedings*, 1922, xv (Sect. Urol.), pp. 7-27.

Colour Tests.

Two colour tests have been discussed, namely, indigo-carmin and phenol-phthalein, and they represent two different principles in colour testing of the renal function.

INDIGO-CARMIN has received a large measure of support from several observers. This was one of the earliest of the colour tests used. The time of appearance and the depth of colouring of the urine with the dye are the points by which the renal function was measured. The dye is only partly excreted by the kidneys. The great advantage it possesses is that it produces a colour easily recognized on cystoscopic examination of the ureteric efflux (chromo-cystoscopy), and catheterization of the ureters may thus be avoided. By this means an occasional fallacy is excluded, namely, temporary reflex suppression on the catheterized side. Delay in the appearance of the dye is only important in my experience if it is combined with a diminished excretion. A delayed excretion followed by a deep coloration is not of any importance as an indication of renal inefficiency. If the kidneys are very seriously diseased, the indigo-carmin test will demonstrate it by delay and diminished excretion. But I have not found it reliable in demonstrating moderate degrees of renal inefficiency. For demonstrating complete absence (congenital) or advanced destruction of one kidney, the indigo-carmin test is one of the first I should choose on account of its simplicity and rapidity, and the fact that it may be used without catheterization of the ureters.

For variations between the extremes, we have no method of precise measurement or means of record. Further, the time of appearance depends upon the dilution of the dye. Any condition which produces diuresis will delay the coloration of the urine and diminish its intensity. The diuresis may be quite independent of renal disease, and it is well to remember that unilateral polyuria may be produced by such causes of irritation as a small stone in the renal pelvis or ureter without any permanent damage to the kidney.

PHENOL-PHTHALEIN is used on quite a different principle. A known quantity of dye which is only excreted by the kidney is introduced into the circulation, and the quantity excreted and the duration of excretion measured accurately by a colorimeter and expressed in percentage of the original quantity of dye introduced. This aims at a much more accurate method of testing than that to which one can aspire with the indigo-carmin test. Has this method of colour testing proved reliable? None of the speakers who have discussed phenol-phthalein have said anything in its support. One of the reasons for this is, I believe, that most of the speakers in this discussion have commenced the study of the renal function since the war, and have had to use quite inferior preparations of phenol-phthalein. After the war I found that much of the phenol-phthalein supplied for testing was worthless, and gave no colour in the urine at all. This was at first ascribed to the use of inferior glass used for the ampoules, but eventually it became clear that the English chemists could not manufacture a drug similar to that produced in Germany. But even with the best phenol-phthalein there is a margin of error. Until the manufacture of this dye is more carefully standardized, I do not see any likelihood of confidence in it being restored. It would, however, be regrettable if we abandoned altogether the attempt to measure in this accurate way the changes in the renal function and fall back on the more rough and ready methods.

But when all is said and done, the dye tests depend on an assumed

parallelism between the excretion of nitrogenous bodies in the urine and certain dyes. This parallelism is open to dispute, and I have been surprised that none of those working at the subject have thought fit to challenge it.

Urea Tests.

I will now turn from the colour tests to the group of urea tests. The old method of estimation of the percentage of urea in a twenty-four hours' specimen under known conditions of diet and rest, and repeating the observations in series or at intervals, has much to recommend it, and should always have a place in the records of a urinary case.

Urea Concentration Test.

A large dose of pre-formed urea is administered by the mouth; as a result, the blood becomes overloaded with urea, and the ability of the kidneys to excrete this urea is taken as a test of the renal function. Urea was employed for this purpose a number of years before Professor MacLean adopted it. Professor MacLean uses the percentage excretion of urea as the criterion of the renal function, and it is this form of the urea test that has been discussed by all those who have taken part in the debate.

In order that the percentage reading of the urea may have value as an index of a diseased renal function, we must assume that the two factors which influence it adversely, namely a diminished quantity of urea or an increased quantity of water, are controlled solely by disease of the kidneys. It is well known that one of the factors, namely the variations in the quantity of water, is sometimes due to normal conditions, sometimes due to disease of the kidneys and sometimes due to extrarenal disease. The difficulty arises only where there is an increase in the quantity of fluid (polyuria). Professor MacLean limits the diuresis to 120 to 130 c.c. of urine in the hour. If the diuresis is over this and the percentage of urea under 2 per cent., "the renal condition is not very satisfactory."

I saw a patient with a stone in the lower ureter; there were pronounced polyuria (80 to 100 oz. in twenty-four hours), a few tube casts and a trace of albumin in the urine. The question was raised whether renal disease was present and would contra-indicate operation. Removal of the ureteral calculus was followed by a drop in the quantity of urine to normal and the disappearance of the casts and albumin. The urea concentration tests would have confirmed the erroneous diagnosis of advanced nephritis in this case.

As a matter of fact in this type of case we could get quite as much information by the use of the estimation of the urea percentage in a twenty-four hours' specimen repeated on several occasions and without the use of the urea concentration test at all.

This question of diuresis is obviously the weak point in the percentage method and unfortunately it is just in such difficult cases that we most require its help. On the whole, we may regard this test as giving valuable assistance in estimating the renal function and from its simplicity it will undoubtedly be very widely used. But we must recognize that its claims to accuracy are seriously impaired by the reservation necessary in regard to diuresis.

Urea Elimination and the Reserve Renal Function.

There is another aspect of the urea elimination tests that I had hoped would be investigated by some of those working at these tests. It has been

a recognized fallacy all through the history of the renal function tests that the kidneys do not under ordinary conditions work up to their full value. The potential function is well over 50 per cent. of the total function. Now any method that would measure this would help us in deciding the question of operation in doubtful cases and in prognosis where we are dealing with a progressive disease.

Albarran tried to measure this reserve by noting the influence of diuretics on the curve of the renal function tests used in his day. He found that the diseased kidney had a function that remained persistently at a certain reduced standard while the normal kidney reacted readily to stimuli which increased its function.

If we take urea as the test of the excretory power of the kidney the problem we have to solve is how high we can force the elimination of urea by the normal and by the diseased kidney. Can we measure this forced urea elimination accurately by estimating the percentage in the urine? With the admitted inaccuracies of the method, I do not think we can, and we must fall back on some such modification of the method as the plotting out of a curve of forced urea excretion quite apart from the percentage composition of the urine.

The important points in such a curve are the time taken for the kidney to react to the stimulus as shown by the commencement of the curve; the rapidity with which the reserve power of the kidney is brought into action as shown by the steepness of the rise; the amount of renal reserve as shown by the height of the curve and the rapidity of return to the average function.

Experience will show whether intravenous injection will supersede administration by the mouth.

Blood Urea.

The estimation of the blood urea would, at first sight, appear to supply all the information required for an estimate of the renal function and should any further information be required, the production of a balance sheet between the urine urea and the blood urea might be expected to provide it. There are, however, two important factors that have to be reckoned with:—

- (1) The unknown reserve power of the kidney to excrete urea.
- (2) The supply of nitrogenous material added to the blood.

It is our experience in using the blood test that no increase in the blood urea is found until destruction of a large part of the renal tissue has occurred. The appearance of an increase in the blood urea marks the point at which the reserve power of the kidney has been destroyed. From this time on, the full power of the kidney to excrete can be measured by estimating the urine urea. Now this is an advanced stage of disease and as a rule the symptoms of renal failure are so pronounced at this stage as to require no confirmation.

There are however some cases in which the symptoms of uræmia at this stage are very slight or entirely absent and the case is obscure until the discovery of increased blood urea is made, and there are other cases in which symptoms are present but they are indistinguishable from other forms of disease and the discovery of a high blood urea clears up the diagnosis. In these cases an estimation of the blood urea is of the utmost value.

In a patient from whom I had removed half a horse-shoe kidney for tubercle, seven years previously, there was cystitis without other symptoms. I cystoscoped him under an anæsthetic. The examination was followed by

persistent vomiting and then the patient became comatose. The quantity of urine remained normal or slightly under and there was between 1 and 2 per cent. of urea. The blood urea was raised to four times the normal amount and an excess of urea was found also in the spinal fluid. The condition was then shown to be due to uræmia following the anæsthetic and examination.

In another case I had removed the prostate, after much hesitation on account of an old aortic aneurysm and a very flatulent bowel condition. The patient suffered from extreme lassitude after the operation but the symptoms of uræmia were very slightly developed; there was moderate thirst and disinclination for food. Vomiting occurred twice in fourteen days and the tongue was moist. An average of 40 to 50 oz. of urine were secreted with 1.5 per cent. of urea. The blood urea was 262 mgr. per 100 c.c. He died comatose three weeks after the operation without developing further symptoms of uræmia.

A high blood-urea may coincide with a high urine-urea percentage. This reduces the value that we attach to the blood-urea estimation as a measure of the renal function. It has been suggested that the excessive quantity of urea in the blood causes a leakage through the kidney. If it did so then all the value of the urea tests of the renal function would disappear, for who could say where secretion ended and leakage began? There is, however, no proof that any such leakage occurs. Such accumulation in the blood is more likely to be due to an increased supply than a diminished output.

The effect of diet on reducing the blood urea as noted by MacLean shows how very carefully the blood-urea figures should be scrutinized before being accepted as indicating renal inefficiency.

Urea Concentration Factor.

Professor MacLean presents a urea concentration factor obtained by dividing the urine-urea figures by the blood-urea figures and apparently further modifications of the factor are in contemplation.

Personally I am opposed altogether to the use of factors and formulæ. We have suffered a good deal from them in testing the renal function. In the early days we had the Koranyi formula which was a complicated formula invented to express in a single figure the kryoscopic examination of the urine and the estimation of the chlorides, corrected in many ways. Then we have had the Ambard constant dealing with the urea content of the blood and urine. Now we are threatened with the MacLean factor and others may follow. It is a legitimate question to ask, for whose use are these formulæ intended? Speaking as a surgeon I may say that it means little to me to be told that the urea concentration factor is 60 or 30. I gain much more clinical knowledge from a report that states in plain terms the amount of urea in the blood and that in the urine.

In making any test of the renal function in urinary surgery, whether for the combined function of the kidneys or for the function of one kidney, it must be remembered that a temporary depression of the renal function may be superadded to a permanent reduction of the function. Thus the lowered index of the renal function in a case of enlarged prostate with residual urine, may be partly due to the immediate effect of the back pressure at the time of the test, and partly to destruction of renal tissue. In the same way the function of the second kidney in a case of pyonephrosis is depressed by the excretion of toxins and by the reno-renal reflex exerted by the diseased kidney. The

proportion of the reduction that must be assigned to temporary depression, and that to permanent reduction, cannot be measured by the renal function tests before surgical intervention has removed the cause of temporary depression. Here surgical experience is the best guide.

All the speakers who have discussed the urea tests appear to have assumed that the urea excretory function of the kidney represents the entire renal function. The urea retention type of renal disease is the type of renal failure that we see in surgical diseases of the urinary tract and by the urea tests we hope to investigate the work of the kidneys in relation to this, admitting at the same time that uræmia is not due to urea retention but to some parallel condition.

But when we turn to another of the great renal functions, that of chloride excretion, and try to investigate chloride retention the question at once arises, what is the value of the urea tests in estimating the chloride retention type of kidney disease? In a Section in which physicians and surgeons are combined, this subject, surely, ought to have been fully discussed.

MR. SYDNEY G. MACDONALD, F.R.C.S.

I propose to state briefly the conclusions at which I have arrived after some twelve years' experience of the various functional tests. These naturally fall under two headings:—

(1) The general renal functions in operations on the lower urinary passages, taking prostatectomy as a type.

(2) The efficiency of the sound kidney in nephrectomy.

(1) *The General Renal Function in Prostatectomy.*—I have passed through the various phases from the earlier colour tests to the more recent chemical tests, viz., the urea concentration of the blood and the urine. As quantitative tests the dyes were not a success, methylene blue being too laborious and phthalein open to too many inaccuracies. As time tests I have used both phthalein and indigo-carmin, but have long since come to the conclusion that the clinical findings are more important than any dye-test, and, in fact, doubt how far any one functional test should influence us in, for example, delaying the second stage of a prostatectomy, provided the clinical condition is satisfactory. It sometimes happens, for instance, that one has no anxiety about proceeding to the second stage until it is found that the blood urea is higher than one would wish. For some time I relied on the clinical manifestations alone, and I am still of opinion that the factors which lead to successful results are (a) experience in dealing with this type of case; (b) due recognition of the signs and symptoms of renal insufficiency; (c) careful preliminary observation of the patient (daily secretion of urine, urea excretion, temperature, blood-pressure, &c.); and (d) personal, preliminary and after-treatment. These are far more important than any functional test and I have not allowed the test to veto operation against my own judgment. I think my own prostatectomy results support this conclusion; for instance, in the last fifty cases in my private practice I have had only one death and that not a renal death. In this series, eleven were two-stage operations; several were actually uræmic when I first saw them, but by careful preliminary treatment (forced hydrotherapy with a retained catheter) the patients were steered successfully through a two-stage operation. Latterly, I have made use of the urea concentration test of the urine, which on account of its simplicity and also because it corresponds

more nearly to the clinical findings, I consider the best test for routine work at the present time, especially if it is controlled by estimation of the blood-urea. I have found in some cases with the urea concentration test that the maximum urea output occurs during the third hour rather than the second; other tests being normal, does that indicate any degree of renal impairment? e.g., a patient aged 67, with blood urea of 31 mgr. per 100 c.c. :—

Urine in first hour	...	118 c.c.	...	Urea 1.0 per cent.	...	Total 1.18 gm.
,, second hour	...	60 "	...	,, 1.5 "	...	,, 0.9 "
,, third hour	...	58 "	...	,, 2.2 "	...	,, 1.27 "

With regard to the blood urea, I think this test can be taken only in conjunction with other tests, since a normal percentage of urea in the blood may have a 75 per cent. margin or no margin at all. Dr. MacLean in his contribution to the discussion¹ pointed out its numerous fallacies, especially in regard to diet. Until I have had more experience of the test, I cannot gauge the upper limit that negatives operation, nor am I satisfied from my own cases that age, *per se*, shows an increased urea percentage in the blood. I would ask the chemical pathologists whether a low blood urea is the rule in pregnancy?

(2) In the second class of case, i.e., the function of the second kidney in nephrectomy, the various tests play a more important rôle. My general line of procedure now, is to catheterize the ureters after making an intramuscular injection of indigo-carmin into the gluteal muscles. The time of appearance of the dye is first noted in each case and no further account taken of the test. Urine sufficient for microscopy and estimation of the urea percentage is then collected from each kidney and finally the blood urea is examined. If the good kidney produces the dye in seven to ten minutes, shows a good urea percentage and the blood urea is normal, I have no anxiety about the nephrectomy. I have used other tests, e.g., phloridzin, which gives fairly accurate results, but is too troublesome for ordinary use.

I cannot understand the rationale of the urea concentration test in estimating the function of one kidney, unless we know, to begin with, that one kidney is grossly diseased or practically effete. Sir John Thomson-Walker's case of ureteric stone raises an important point. We must ever be mindful of the influence of the bad kidney on its healthy neighbour. Albarran pointed out some years ago, in connexion with renal tuberculosis, that the urine from the sound kidney not infrequently contained both albumin and casts (due to a toxic nephritis) which disappeared when the diseased kidney had been removed.

Dr. J. R. MARRACK.

I have found the blood urea the most satisfactory test. The normal figures run up to about 50 mgr. to 100 c.c. In a very large number of medical cases, in whom excessive formation or intake of urea could be excluded, I have not found higher figures, except in cases in which there was definite impairment of kidney function, due either to direct damage to the kidney or secondary to some other cause, such as heart failure. A value for the blood urea above normal means that the kidneys are unable to meet their daily work, still less

¹ *Proceedings*, 1922, xv (Sect. Urol.), pp. 12, 13.

an extra strain. In men with enlarged prostates, the great majority of those whose blood urea has remained above 50 mgr. to 100 c.c., after draining have proved unsuitable for operation. Most have been rejected by the surgeon, others have either done badly or died of renal insufficiency or other causes, such as pulmonary embolism or broncho-pneumonia. Among those with normal figures, deaths from kidney insufficiency and from other causes were also much fewer than among those with high blood ureas. So that the kidney insufficiency, of which the high blood ureas form evidence, seems to predispose to death from broncho-pneumonia and embolism. The blood urea may be high in patients when they come into hospital and may drop after drainage. These cases often do well. The concentration of urea in the cerebro-spinal fluid agrees very closely with that in the blood, and nothing is gained by examining the cerebro-spinal fluid as well as the blood.

The difficulty with other tests lies in the fact that the values given by different degrees of kidney impairment overlap. If a large number of cases are taken and arranged in the order of the findings by any test, they will be found to be arranged mainly in the order of the degree of severity; but individual cases will be found far out of place; and a fallacy may arise from applying the results obtained from average cases to any individual case.

With regard to the urea concentration test, the maximum concentration of urea in the urine a normal kidney can reach, is 4 to 5 per cent.; and under a suitable stimulus, a moderately damaged kidney can excrete a urine with well over 2 per cent. of urea. With the test as used, even if water intake is cut down beforehand, a normal person with low blood urea may only receive stimulus to excrete a urine with some 2 to 2.5 per cent. of urea. Thus, the percentage reached by moderately damaged kidneys may overlap that given by normal kidneys. This actually occurs; the zone 2 to 2.5 per cent. is a particularly doubtful one, both with medical cases and patients for prostatectomy. Among medical cases, almost all who did not reach 2 per cent., in whom the water excretion was not excessive, had very definite renal damage.

With phenol-sulphone-phthalein I have had results uniformly lower than those reported by American workers, due apparently to the use of English dye. The normal figures have run as low as 40 per cent. in two hours. The overlapping of values given in different degrees of severity is well illustrated, as patients with moderate renal damage may, rarely, excrete up to 50 per cent. in two hours and overlap low normal figures, or as low as 20 per cent. overlapping the figures for severe cases. The ability to excrete phenol-sulphone-phthalein runs fairly parallel with the ability to excrete urea. High blood urea and urea concentration below 2 per cent. are rare when the phenol-sulphone-phthalein is above 30 per cent. in two hours, and become more and more frequent as one descends the scale below this.

The difficulty in estimating the colour owing to the yellow colour of urine can easily be overcome by making up the standard with an appropriate amount of urine, and blood can be removed by precipitation with zinc chloride. The practical difficulty lies in obtaining complete and accurate hourly specimens of urine.

Patients for prostatectomy who have had phenol-sulphone-phthalein excretion of over 30 per cent. in two hours, or urea concentration above 2.5 per cent., have with one exception done well. Below these figures the results with these tests have been unsatisfactory, as many patients who have come out very badly by the tests have done well and others who gave better results to the tests have done badly.

In prostatectomy cases, therefore, we get the practical result that there are two groups of cases of whom one can speak with some confidence, those whose blood urea remains over 50 mg. per 100 c.c., who are likely to stand operation badly, and those with phenol-sulphone-phthalein over 30 per cent. in two hours, or urea concentration over 2.5 per cent., who are likely to do well. Between these two groups lie many cases with normal blood ureas and low phenol-sulphone-phthalein excretion and urea concentration, of whom I think it is impossible to speak with any certainty.

I have estimated the diastatic index in urine and plasma in a large number of medical patients and do not consider it of much practical use. Diastatic indices of 3.3, or lower, in the urine, with normal or high diastatic indices in the plasma, are usual in severe cases—such cases as would be obviously unsuited for operation if they were patients with enlarged prostates. But there is not enough distinction between the values found in less severe cases and those found in normal persons.

I think it is unwise to use urea solely as the substance for testing the efficiency of the kidneys. It is apparently a comparatively harmless substance and not responsible for the symptoms of uræmia; the power to excrete the actual toxic substances does not run exactly parallel with the power to excrete urea, as some patients can continue for months with blood urea from 150 to 200 mgr. to the 100 c.c., and others run up to 300 mgr. for a time, without any sign of uræmia, while other patients develop uræmia with much lower figures for blood urea.

Mr. G. E. NELIGAN.

I strongly advocate the use of the indigo-carmin test. I have been employing this test while Dr. Marrack has been carrying out the tests of which he has just spoken, on the same patients. Those which I have found the most valuable in surgical practice and the most constantly in keeping with the clinical findings are the blood-urea and the indigo-carmin tests. I prefer the indigo-carmin test for two reasons. First, it is practically fool-proof and can be easily and speedily employed by the surgeon himself without any laboratory assistance; secondly, I have found that the indigo test is the last to recover in cases of renal inefficiency, and this being so, it gives one a greater margin of safety.

Mr. KENNETH M. WALKER.

My own practice has been to rely on the following data in arriving at a conclusion as to whether an operation such as prostatectomy is advisable or not: (1) The clinical condition; (2) the blood urea; (3) the urea concentration test; (4) the excretion of indigo-carmin. As the first three have already been dealt with I will discuss the last named only.

Sir John Thomson-Walker has drawn attention to the fact that we have no proof that any parallel exists between the excretion of dyes and the excretion of the products of nitrogenous metabolism. This is undoubtedly the case and may detract in some degree from the value of indigo-carmin as a test of the total renal efficiency of a patient. It does not, however, discount the value of the test as a means of estimating the relative value of the two kidneys from the point of view of excretion.

The chief advantages of the indigo-carmin test are its simplicity and the fact that it can be undertaken at a moment's notice during the course of a cystoscopy. The injection should certainly be given intravenously and the time of the appearance of the dye at the ureteric orifice noted. The test is a very delicate one, and I have not infrequently found delayed excretion in very minor lesions of the kidney, such as mild degrees of pyelitis and even papilloma of the pelvis. In one case of hæmaturia, cystoscoped at a time when the hæmaturia had ceased, the delayed excretion from one side was sufficient to centre interest on that kidney and by the subsequent passage of a ureteric catheter and the provocation of hæmorrhage, to allow of the diagnosis of renal papilloma being made.

It is obvious from the interesting discussion we have had that considerable difference of opinion exists as to the relative values of the various tests of renal efficiency. Most of us appear to have been let down at one time or other by our tests, and it would seem that there exists a factor which we have at present no means of estimating, when assessing the risks of an operation. I am convinced that sepsis is the factor that so frequently upsets our calculations and defies our tests. Many patients who are said to have died of renal inefficiency after prostatectomy have in my opinion died rather of sepsis superimposed on a damaged kidney. I have made a practice of examining histologically the kidneys of patients dying after a prostatectomy, and as a result am fully convinced that more patients succumb to the toxæmia of bacillary infection than to that of uræmia.

MR. R. OGIER WARD.

During the past year observations have been carried out on cases in the wards of the Surgical Unit at St. Bartholomew's Hospital: the chemical methods employed have been the urea concentration test and the estimation of the blood urea. These two tests have usually proved to be in close agreement with the clinical estimation of renal function and with operative findings, but there have been exceptions.

When Rose Bradford's work upon the kidney is remembered, it is not surprising that comparatively gross destruction of kidney tissue may have occurred, and yet the urea in the blood and the concentrating power of the kidneys may seem to be normal when tested by the methods at present available. One case of special interest may be quoted:—

A man, aged 48, was admitted suffering from right-sided renal tuberculosis with symptoms which had existed for two years. The clinical estimate, made from the general appearances of the patient, was that his poor general condition could be explained by the supposition that his renal function was impaired, or by effect upon his health of prolonged vesical irritation and attendant loss of sleep. After two days in bed on hospital half diet, his blood urea was 49 mgr. per 100 c.c. blood. On the following day his urea concentration was: First hour, 2'16 per cent. of urea in approximately 100 c.c. urine; second hour, 2'64 per cent. of urea in approximately 95 c.c. urine. Cystoscopy showed tuberculosis of the bladder with ulceration, a rigid inflamed right ureteric orifice, and a healthy one on the left side. It was considered unwise to catheterize what was believed to be the healthy ureter, and right nephrectomy was performed. He died twenty-four hours later of uræmia, having only secreted 4 oz. urine since operation, and at the post-mortem examination the cerebro-spinal fluid contained 180 mgr. of urea per 100 c.c.

In the specimen exhibited it will be seen that the right kidney is extensively tuberculous—approximately only half of it remains effective. The left kidney is free

from obvious tubercle, but is smaller than the right, the cortex is reduced, the pelvic part increased, and microscopical examination shows very extensive hyaline degeneration of tubules—many have entirely disappeared—only a very small proportion of functioning tubules remain. The degenerated tubules do not give the amyloid reaction with methyl-violet. This patient seems to have lost a very large proportion of renal tissue, approximately two-thirds, without giving evidence of this when tested by these two methods, though clinical appearances suggested the possibility of impaired renal function. This result is, as Dr. George Graham has pointed out, in close agreement with the experimental results obtained by Rose Bradford.

Indigo-carmin has been used when it was desired to ascertain the function of each kidney separately, the ureteric effluxes being watched through the cystoscope, and the time of the appearance of the dye noted. This has usually been done without general anæsthesia. In cases in which the excretion of the dye has been seen to be delayed, operation, if carried out, has always proved impaired function.

Conclusions were formed after examination of sixty surgical cases, amongst which were inflammations of the kidney, acute and chronic, renal and ureteric calculus, new growths of the kidney, enlarged prostate and urethral stricture.

These tests were useful and reliable in most cases, but the inference made from them must not be allowed precedence over the judgment passed on clinical signs.

REPORT ON THE DISCUSSION ON "THE TESTS OF RENAL FUNCTION."

DURING the Session 1921-22 two meetings of the Section of Urology of the Royal Society of Medicine, under the Presidency of Sir Thomas Horder, were held in order to discuss the value of tests designed to estimate the function of the kidneys in disease of the urinary tract. The discussion was shared by eighteen speakers, of whom eleven were surgeons and two were physicians. The remaining five speakers treated the subject primarily from the point of view of the laboratory worker. The interest of these methods of examination centred mainly round the functioning value of the kidneys in cases of enlarged prostate and in cases of renal disease in which the upper urinary tract was chiefly involved. Many of the tests were combined with others. They may be classified as follows:—

- (1) Urea concentration test.
- (2) Estimation of urea in the blood.
- (3) Estimation of urea in the urine.
- (4) Estimation of sugar in the urine and in the blood.
- (5) Estimation of diastase in the urine and in the blood.
- (6) Cryoscopic test of the urine and of the blood.
- (7) Pyelography.
- (8) Dye excretion by the kidney, after the administration by injection of some colouring matter.

Of these the *urea concentration test*, the *estimation of urea in the blood*, *pyelography*, and *dye excretion* were considered the most helpful, but absolute reliance could not be placed on any one test.

With regard to the *estimation of urea in the urine*, we feel that in view of the fact that the greater part of the urea which is ordinarily excreted comes

direct from the food, ordinary quantitative estimation of the urea seems to afford little help. In regard to the *urine concentration test* a useful standard method of performing this seems to be the following:—

(1) All fluid is withheld from the patient for six hours before the dose of urea is administered.

(2) 15 grm. of urea in 100 c.c. of water are administered. This quantity may, and perhaps should be, exceeded in certain cases.

(3) The volume of urine which is passed should be measured hourly for three hours.

The administration of the urea may be followed by polyuria. If so, allowance must be made or the test should be repeated. The presence of blood and protein in the urine does not affect the test. Urea concentration varies with the diet, therefore care should be exercised, in making the test, to take the diet into consideration.

The main conclusions reached as to the application of the test are:—

(1) If the urea in the urine at the end of the first, second, or third hours exceeds 2.5 per cent. the kidneys may be considered to be performing their function adequately.

(2) If it is less than 2.5 per cent., but more than 2 per cent., the kidneys are probably acting adequately, but an additional examination of the urea in the blood is advisable.

(3) If the urea in the urine is below 2 per cent. the kidneys are probably not doing their proper amount of work.

(4) Normal blood urea on hospital diet is 15 to 40 mgr. per cent., and if the concentration of urea in the urine is above 2 per cent. the urea in the blood would be below 40 mgr. per cent. before the urea is given. This would appear to support the statement which has been made that far too broad a standard of normality is introduced—a statement which is borne out by some of the observations on diastase in the urine, and offers a reason for the preference which clinicians have for clinical tests more or less independent of chemical examination.

Mr. Swift Joly is of opinion that not only must the percentage of urea in the second hour's urine be considered, but the total amount excreted during the second and third hours of collection should also be taken into account.

It will be perhaps convenient here to give Professor MacLean's findings in regard to this test based upon 14,000 cases, the majority of which as he states, were Army cases of nephritis. In the estimation of blood urea he finds that the quantity varies according to age—from 20 mgr. per cent. in young people to 50 mgr. per cent. in older people are normal values: 70 mgr. per cent. in an elderly patient would indicate renal insufficiency. But it is pointed out that the amount of urea in the blood bears a distinct relation to the diet and may therefore only be regarded as an index. With regard to the urea concentration test Professor MacLean thinks that:

(1) The relation of the urea in the urine to the urea in the blood should be considered.

(2) The quantity of urine collected during the second hour shows the maximum concentration. If this is over 2 per cent. the kidneys are probably efficient; if it is in the region of 1 per cent. the kidneys are not efficient.

(3) The quantity of urine passed should not be more than 130 c.c.

With particular regard to the first of these points Professor MacLean suggests a factor:—

milligrams per cent. of urea in the urine
 milligrams per cent. of urea in the blood

This factor in health varies between 60 and 80, showing a considerable possibility of error, which perhaps helps to explain the overlapping results recorded by others. If the renal condition, according to this observer, has been passed as efficient by the chemist, no death due to renal insufficiency has followed an operation. The deaths that have occurred have resulted from septicæmia, peritonitis and pneumonia. But, as the President remarked, these terminal infections are themselves frequently indicative, though indirectly, of renal insufficiency.

Dr. Geoffrey Evans and Mr. Kenneth Walker both urge strongly that in the presence of existing sepsis satisfactory renal function tests do not exclude risk to life following upon the operation.

Dr. Marrack gives general confirmation to the statements of Professor MacLean.

Both Professor MacLean and Dr. Marrack are against much value being assigned to the *diastase content in the urine* and with regard to this point some views may be given. Dr. Harrison states that the diastase in the urine gives no measure of the diastase in the blood. Mr. Girling Ball and Dr. Mackenzie Wallis attach some value to the estimation of the diastase; but, if limited to two tests, they would discard this in favour of the estimation of urea in the blood and the analysis of the relation of sugar in the blood to sugar in the urine. Dr. Mackenzie Wallis states that the diastase test was valuable in the diagnosis and prognosis of Army nephritis. It is probable that we shall have to wait for further results as regards sugar and diastase tests before forming any decision as to their value in indicating renal efficiency.

Mr. Macalpine and Mr. Ogier Ward raised the question of the amount of kidney substance compatible with life and quoted Rose Bradford's experiments in which he removed two-thirds of the total kidney substance in animals which, after this operation, showed no sign of renal inefficiency; whereas when he removed three-quarters of the total kidney substance the animals died, and it was found that they showed excess of urea in the blood. But it appears highly probable that a considerable reduction of renal substance is compatible with good renal function. In such cases we feel that the occurrence of sepsis at the time of, or following, operation may damage the remaining portion of renal substance and therefore put the patient in grave danger.

The following dye-tests were principally favoured:—

- (1) Phenol-sulphone-phthalein.
- (2) Indigo-carmin.

Methylene blue has been quite given up.

The excretion of phenol-sulphone-phthalein runs parallel with that of urea and there is, according to Marrack, a similar overlapping of results. Some observers raised objections to the use of phenol dyes in that the presence of blood may vitiate the colour test and that the urine itself is coloured; but it is proposed to get over these difficulties by precipitating the blood with zinc chloride and using urine in the standard colorimeter instead of water. Several observers object to phenol-sulphone-phthalein as it gives uncertain results. They agree that the pre-war article gave better results than that of the present day. Many observers think highly of indigo-carmin.

As regards the results of clinical observation it is felt that an admirable summary of the position is given in Mr. Cyril Nitch's words:

"When a patient has a typical 'clayey' complexion and a 'beefy' tongue, and complains of either thirst or disinclination for food, especially meat,

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I do not allow any chemical test to influence me in favour of an operation. On the other hand, if the clinical condition is good and the tests are below standard, I should pin my faith to the clinical signs."¹

We should prefer to say, as the conclusion drawn from this discussion, that chemical tests of renal inefficiency indicate a risk in operating, even when clinical signs are favourable. In other words, it would appear that warnings should be heeded, whether given by the clinical condition or by the laboratory tests.

(Signed)

THOMAS HORDER.
W. LANGDON BROWN.
GEORGE GRAHAM.
A. R. THOMPSON.

¹ *Proceedings*, 1922, xv (Sect. Urol.), p. 16.

Section of Urology.

President—Sir THOMAS HORDER, M.D.

Urinary Calculi in Animals.

By FREDERICK HOBDAY, C.M.G., F.R.C.V.S., F.R.S.E.

MY original intention was merely to show specimens—of which I have collected several hundreds, through the courtesy of brother practitioners and from my own practice—but I have decided to treat the subject in the form of a paper, because the comparative aspect of medicine and pathology has been considerably in evidence of late; and it will be agreed that by co-operation and comparison, human and veterinary medicine can learn much that is of mutual benefit, seeing that human beings and animals are liable to similar ailments and diseases.

It is with this comparative aspect that I shall deal specially to-night. I have brought a large number of specimens and skiagraphs in order to emphasize their frequency.

To begin with, it must set up a certain train of reasoning in the mind of every medical man interested in the diseases of the urinary system to learn that all domesticated animals (and wild ones too are not exempt) suffer from urinary troubles which give rise to urinary deposits—for the veterinarian in busy practice meets with these in the horse, cow, sheep, pig, dog and cat; and I have specimens from each variety of animal here to-night.

Dr. J. A. Gardner, Professor of Bio-Chemistry in the University of London, has analysed a large number of these specimens with the result that calculi from the horse have all been shown to yield calcium carbonate as the main constituent, varying from 70 to 80 per cent., with oxalates and phosphates of lime as adjuncts; of three specimens from the cow one consisted mainly of calcium carbonate, and the other two of the triple phosphate. A pig's vesical calculus yielded ammonium and magnesium phosphate, and that of a sheep mainly calcium carbonate. Whilst of a number taken from the dog—the result out of forty-one specimens—thirty-five consisted of almost pure triple phosphate, three mainly calcium oxalates, with more or less addition of phosphates; one pure cystin and one mainly guanin.

In the bladder of the horse and cat a sabulous deposit is more common than a true calculus formation; in fact in the cat, although we meet with some scores of cases of lithiasis of the bladder, I have never met with, or heard of, a case in which the deposit could be called a true calculus. It is worth noting too, that the cat patient most frequently affected is the one which has been "neutered," and is growing old and fat and lazy.

Of all our patients calculi of the urinary tract are more commonly met with in the horse or dog, possibly partly because these animals live longer

than those varieties of the domesticated animals which are used for food, and experience tells us that calculi are more commonly met with in middle life or old age. This, however, is not a rule without exception for I recently surgically removed one from the urethra of a spaniel puppy aged only 4 months, and on another occasion took one as large as a walnut from the bladder of a spaniel dog, aged only 11 months; and others have had similar experiences.

As regards diagnosis, those in the human medical world would have far greater facilities for obtaining accuracy than we have with our animal patients, foremost among these facilities being the interchange of speech, and the power of asking questions and obtaining replies, whereas veterinary practitioners have to rely largely on powers of observation and upon what experience has taught them in the way of interpreting the signs and symptoms presented. At the same time their patients do not tell them lies, so that they do not get misled in that way.

The use of the X-ray has proved an inestimable boon to us as well as to you, and for the dog and the smaller animals where expense is not necessarily a limiting factor, this aid to diagnosis is in constant demand.

In the case of food-supplying animals, except in that of the cow, calculi of the urinary tract are not often reported by the general practitioner. This discovery falls more to the lot of the meat inspector, as such things are only found post mortem: but in the horse, a utility animal, and the dog, whose constant companionship with mankind makes him almost one of ourselves, the irritation presented by the presence of calculi is soon observed and steps are taken by the owner to get relief.

The symptoms observable are those of difficulty in urination, the animal straining and making effort to pass water, and only being able to do so in dribblets or in a very small stream, and exhibiting signs of irritation or pain when so doing.

Examination per rectum in the horse, together with palpation of the abdomen in the dog, and the passing of a catheter or a flexible metal sound, will soon decide the question, or in the case of the smaller animal, if further confirmation is needed, the X-ray can be applied.

In the dog, the passage of even the smallest calculus down the urethra is complicated by the anatomical fact that there is a bone in the penis of this animal and that this is grooved. It is in this bony and unyielding groove that these small stones usually stick.

In the ram, too, we have a curious worm-like body, known as the vermiform appendix, which is situated at the end of the penis, and around the base of which urethral concretions are apt to collect. In these animals and the wether sheep, it is a generally accepted fact that these preputial concretions are especially noticeable after the animals have been receiving a dietary of mangolds for some considerable time.

In the bull, too, all is not plain sailing when urethral calculi have to be removed, as the penis is S-shaped and concretions can get lodged here in such a position that it is only with the utmost difficulty they can be removed.

Medicinal treatment in animals, when once the diagnosis is assured, is practically useless to effect a cure, and once having made sure of the presence of the calculus, its removal is merely a matter of surgical procedure, and in this we are guided by the same rules as apply to human surgery. It is unnecessary to describe the operation in detail before this audience, but, briefly stated, the skin is rendered aseptic, the patient is put under the influence of some anæsthetic, and the stone cut down upon and removed.

If in the urethra it is a comparatively simple matter, experience teaching us that it is better not to suture the wound afterwards; and if in the bladder the suprapubic method is employed with great success. Setting aside ovariectomy the operation of suprapubic cystotomy is probably the second best most successful abdominal operation the veterinary operator can be called upon to perform—and the result is invariably a successful one.

We have, however, certain minor difficulties of detail, which the human surgeon escapes, as for example those of securing our patients and persuading them to remain still during the operation. This we overcome by the use of hobbles and chloroform; equine patients being thrown in a grass field, or on a straw bed, and canine patients secured on an operating table.

In the after-treatment, too, we are placed at a great disadvantage as compared with those who treat mankind; for, although we have our male and female nurses, our patients cannot go between clean sheets, and thus be kept free from contamination.

So far as the larger varieties of animals are concerned, they have to go to a loose box bedded down with straw, as clean and sweet as circumstances will permit, and remain tied up so that the wound does not touch the dirty floor; whilst the smaller ones go into cages or kennels, with a clean cloth or blanket to lie upon, their wound being painted with iodine or otherwise dressed antiseptically every day. It is astonishing, however, how quickly these wounds heal, the proportion of deaths from septic infection being negligible. In fact, as I have already said, the surgical removal of a calculus from the bladder of a dog gives less anxiety to the operator nowadays than any other operation which necessitates the opening of an abdomen—with the exception of ovariectomy—which is done in some parts of the country every day by the empiric.

The prognosis is good, for in the large majority of cases the calculi will not return. Experience teaches us, however, not to promise this definitely upon every occasion, as some of our patients distinctly have a diathesis which predisposes to a re-formation of the calculus, and in some it will return as badly as ever within eighteen months or two years. This however is exceptional. We have again here one little advantage over the human surgeon, as our patients do not live for so many years as man, so that for a comparatively slowly-forming body like the urinary calculus the proportion of relapses is not so great.

The variations in the size, shape and external surface of the calculi are interesting to follow, and invite speculation. Perhaps you have some theory which explains the cause of their shape—why, for example, some should be perfectly smooth and others nodulated or roughened on the exterior? I myself am unable to offer any explanation, as both forms show the same chemical composition. I can only speak from the surgeon's standpoint as to which I prefer to see upon opening the bladder, as the smooth calculi are found to have done much less damage, and to have exercised less irritating effects upon the bladder wall.

In particular would I draw your attention to the fact that some of our patients are solely herbivorous, and others carnivorous feeders, whilst some eat anything and are omnivorous. Some lead very active and some very sedentary lives.

That there is a legal aspect of the case for the veterinary practitioner might not occur to you, but for us this is a serious one, as many horses and dogs cost hundreds of pounds each.

Only recently a well-known veterinary surgeon in the North of England narrowly escaped an action at law which might have cost him a large sum of money. At the request of a client he selected, and then examined for soundness and health, a valuable bitch puppy, this animal being one of a celebrated litter. His choice lay between two of the pups, and there was so little to choose between them that it was a matter of no importance from a show point which was taken. Within three months the one he had left behind was found to be the subject of urinary obstruction of such a severe character as to necessitate urgent operation, and a large vesical calculus was removed. Had this been the bitch selected it would have been a point for argument (perhaps legal) as to whether (i.e., if she had been passed "sound" three months before) the owner could sue the veterinary surgeon for giving a sound certificate to a bitch with vesical calculus. It is not the custom when examining a dog for "soundness" to examine digitally per rectum, nor indeed to palpate the abdomen, and the point comes in as to whether a litigious client could, or could not, claim damages if this procedure had not been carried out.

Summing up, then, as a veterinary surgeon addressing a body of medical men, I would draw attention to the facts that, so far as urinary calculi are concerned:—

(1) Animal patients of all kinds suffer from the presence of these concretions, and that they are presumably formed in much the same way as in human patients.

(2) Their treatment is similar—from the fact that surgical methods have to be resorted to for their removal—the main differences being those of technique, necessitated on account of the methods necessary to employ in order to get the patient still and free from pain—i.e., the securing and anæsthetizing.

(3) In the fact that these points and difficulties are common to both professions, there is a common ground for intercommunication of ideas, the opportunity for which the Royal Society of Medicine has now offered to us by encouraging the study of comparative medicine.

I hope that the specimens and skiagraphs I have brought here this evening will stimulate interest in our work, and show you that we are trying our best to do something to advance what is, after all, a common science and a common object in the fight against disease.

The Comparative Anatomy of the Accessory Sexual Glands.

By KENNETH WALKER, F.R.C.S.

(ABSTRACT.)

CONSIDERING the importance of the subject it is extraordinary how little we know concerning the physiology of reproduction and the function of the various accessory sexual glands found in the male. It is in the hope of throwing some light on the function of these glands that I am making this expedition into comparative anatomy, and amongst the accessory glands considered I am going to include the epididymis and the ampulla of the vas as well as the prostate, the seminal vesicles and Cowper's glands. Perhaps it will be considered unusual to include the epididymis and the ampulla of the vas amongst the accessory sexual glands, but I hope to justify this on grounds of comparative anatomy.

[*June* 29, 1922.

What is the function of the prostate? So little do we know of this subject that it is possible to summarize our knowledge in a few lines. The work of Kölliker, Steinach and others has shown that the prostatic glands provide a fluid in which spermatozoa retain their activity for a long period. In normal saline, spermatozoa only remain active for some two or three hours. In saline to which prostatic secretion has been added they may remain mobile for as long as twenty-two hours. The prostatic secretion, therefore, provides a medium favourable to the life of the spermatozoon, but beyond this scarcely anything is known as to the function of that gland. Of the vesiculæ seminales we know even less, but what is certain is that the name is a misnomer so far as it implies a sac in which semen is stored. The vesicle is a gland which contributes a definite secretion to the semen, and in some animals, particularly amongst the rodents, it would appear that this contribution of the vesicles to the semen is of great importance.

At one time it was suggested that the state of repletion of the prostate and of the vesicles bore a relation to sexual desire. However, Steinach's work on rats has definitely proved that the prostate and the vesicles are in no way essential to virility. Steinach excised the prostate and vesicles of a male rat. After recovery the sexual vigour of the rat was unimpaired, and he was observed, on returning to the females, to effect coitus eighty times within a period of an hour. However, although sexual vigour still remained after extirpation of the prostate and vesicles the breeding-power was impaired. The experimental work of George Walker, of Baltimore, is of great interest in this connexion. He showed that after removal of the vesicles or of the prostate a rat was still capable of breeding. After removal of both, however, he was apparently sterile. Steinach has repeated this work, and appears to have confirmed it. It would, therefore, seem that the prostatic and vesicular secretions, if not essential, are at any rate of great importance in breeding. That they are not absolutely essential would appear to be the case if Ivanoff's work is to be relied upon. Ivanoff states that he was able to fertilize female rats by means of spermatozoa taken direct from the testis and unmixed with other secretions.

In considering the comparative anatomy of the accessory sexual glands I do not intend to discuss such lowly members of the animal kingdom as insects, fish and reptiles, owing to the difficulty that exists in deciding the true analogues of the prostate and vesicles in these orders. It is interesting, however, to note in passing the existence of cloacal glands in fish. These glands after the emission of spermatozoa evacuate a thick viscous material which provides a protective envelope for the delicate sperm. Since the prostate is derived embryologically from the cloaca, we may possibly see here foreshadowed, in the action of the cloacal glands of fish, the function of the prostatic gland in higher animals.

In almost all problems of comparative anatomy, the ornithorhynchus is of great interest and in the matter of accessory sexual glands this is particularly the case. I will, therefore, start with this animal.

(Sketches of dissections of the following animals were then shown by means of the epidiascope.)

Ornithorhynchus anatinus.—In the ornithorhynchus there are neither prostate nor seminal vesicles. The most noteworthy feature is the size of the epididymis, formed by a much convoluted tube suspended on a mesentery and opening into the long urogenital canal. One is tempted to conclude that the great development of the epididymis in the ornithorhynchus is not unconnected with the absence of the prostate and vesicles. At any rate it furnishes a reservoir in which spermatozoa are stored ready for emission.

Marsupialia.—Amongst the Marsupialia we find the beginning of the prostate. In the male kangaroo the urogenital sinus leading from the bladder to the penile urethra is as long as in the ornithorhynchus, but its walls are very much thickened, and it is moreover pyriform rather than cylindrical in shape. A section through the thickened walls of this urogenital sinus reveals the existence of two layers, an internal glandular and an internal muscular. The internal glandular layer represents the prostate and the urethral glands in higher animals. Other noteworthy features in the kangaroo are the absence of seminal vesicles and the presence of a six-lobe Cowper's gland.

Wombat.—The flying wombat that I have recently dissected illustrates exceedingly well the beginning of the prostate amongst the marsupials, the urogenital sinus being shaped like a pear with its stalk towards the penile urethra.

Hedgehog (Erinaceus europaeus).—This illustrates the immense development of the seminal vesicles and the prostate amongst the rodents. In the hedgehog the seminal vesicle is divided into six lobes, and is of such a size that it overshadows the whole of the genito-urinary tract. The prostate, although less conspicuous than the vesicles, is also very well developed as a four-lobed gland. Looking at the size of the seminal vesicles it is impossible to avoid arriving at the conclusion that they must contribute some important ingredients to the semen. Marshall has failed to find any spermatozoa in the vesicles of the hedgehog even during the height of the rutting season.

Viscacha (Lagostomos trichodactylus).—In this animal the development of the vesicles surely reaches its maximum. The viscacha is a small rodent resembling a guinea-pig, that inhabits the Argentine "pampas." In it the vesicles appear as two convoluted tubes suspended on their own mesentery, and attaining a length that is three or four times the length of the whole urethra.

Rhinoceros (Rhinoceros unicornis).—A point of interest in the rhinoceros is the similarity in structure between the vesicles and the prostate. Both are multilobulated glands that are similar in macroscopic appearance and bound up into the same facial sheath. Indeed on dissection it is very difficult to find any definite line of demarcation between the vesicles and the prostate. Other features are the size of the Cowper's glands and the presence of a very well-marked uterus masculinus.

Red River Hog (Potamochoerus penicillatus).—This specimen has been shown in order to illustrate the enormous development of Cowper's glands in the pig family. On opening the gland it is seen to have a spongy structure and a central spiral reservoir in which the secretion is stored. Taking into account the enormous development of these glands it is difficult to speculate as to their function. All that we know is that the secretion of Cowper's glands is devoid of mucin, and that it has been suggested that its action is to neutralize any urine that still remains in the urethra at the moment of ejaculation. Such an explanation of the function of Cowper's glands would appear to be wholly unconvincing. I do not know the size of Bartholin's glands in the sow, but there would appear to be a relation between Cowper's glands in the male and Bartholin's in the female.

Cervus tarandus.—An interesting point in the deer family is the great development of the uterus masculinus. Not only is the uterus reproduced, but the Fallopian tubes are represented by two cornua suspended in the mesentery between the converging vasa deferentia.

Aard Vaark (Orcteropus aethiopicus).—This pig-like animal lives in South Africa, and is interesting from the fact that the uterus masculinus here reaches enormous proportions. Indeed it is so large that one is tempted to believe that it is not merely of embryological interest but actually preserves some function.

Tapir (Tapirus indicus).—I am showing this sketch in order to demonstrate how well marked are the bulb and the veru montanum in most animals. Upon the eminence of the veru montanum the ejaculatory ducts and the prostatic tubules open as in man. The function of the bulb during coitus has been described by John Hunter. During the sexual act the semen is gradually squeezed along the vas deferens by peristaltic waves, and together with secretions from the accessory sexual glands collects in the bulb. The final stage of the act consists in the expulsion of these secretions by vigorous contraction of the urethral and perineal muscles. In the initiation of the final explosive paroxysm the sensitive veru montanum with its rich supply of nerves would appear to act as a veritable trigger, and not merely as an obstacle to the passage of the semen into the bladder. The bulb may, therefore, be truly described as a receptaculum seminis, and the veru montanum as the trigger that initiates the final muscular paroxysm. It is therefore not surprising that in a certain type of impotence we should see definite changes in the appearance of the veru montanum.

Elephant (Elephas africanus).—As a contrast to what is found in the rhinoceros, the seminal vesicles in the elephant are simple in character and represented by two ear-shaped sacs with a comparatively thin wall. The prostate is not well developed. What is more interesting is the fact that the ampulla of the vas is here well marked, the vas deferens becoming distinctly convoluted as it approaches the urethra. The importance of this, however, will be seen better in the next specimen.

Zebra.—This animal illustrates well the overlapping in function that may occur between the seminal vesicles and the ampulla of the vas. In the zebra the vesicle is a thin-walled sac with a smooth lining; the section through the ampulla on the other hand shows a honeycombed structure similar to that usually found in a vesicle. Indeed the section of the ampulla so strongly resembles that generally presented by a vesicle that it is impossible to avoid the conclusion that in this animal the ampulla is discharging the function of a vesicle and contributing a secretion to the semen.

Oppel has made the interesting observation that in animals like the dog, cat and the pig, that possess no ampulla, coitus takes a long time. In those possessed of a well-marked ampulla, as in the case of the bull, the ram and the horse, coitus is rapid. The explanation of this is obvious. When no ampulla is present the secretions from the testis have to travel the whole length of the vas in order to reach the bulb, whereas when an ampulla is in existence they may be stored ready for ejaculation in that structure.

Ram (Ovis aris).—The only point of interest here is the long auricular-shaped vesicle. The prostate is relatively small.

Baboon (Cynocephalus anubis).—Here the accessory glands resemble closely those of man. The vas ends in an ampulla, which is, however, not very highly developed.

Chimpanzee (Anthropopithecus troglodytes).—It is interesting to note that in the chimpanzee the vas is straight throughout its course, there being no suggestion of an ampulla. Therefore, it would seem that man in acquiring an ampulla resembles in this respect the baboon rather than the more closely allied chimpanzee.

The following general conclusions can be drawn from a consideration of the above facts. They may be summarized as follows:—

(1) The first point worthy of note is the wide range of variations that occur in the development of the accessory sexual glands in different members of the animal kingdom.

(2) There would appear to be considerable overlapping of function, especially between the prostate and the vesicles and between the vesicles and the ampulla of the vas. This is suggested not only by their similarity in structure in many animals, but by the broad rule that where the prostate is highly developed the vesicles are often insignificant and vice versa. This observation was originally made by so great an authority as John Hunter.

(3) That Cowper's glands, although of little importance in man, would appear to discharge an important function in many animals, especially in the pig family.

(4) That the bulb rather than the vesicles must be regarded as the receptaculum seminis in which the secretions of the testis, prostate and vesicles are collected during coitus and from which they are ejaculated at the end of the act.

Specimen of a "Miner's Egg."

Shown by FRANK KIDD, M.Ch.

THIS specimen was presented to me by a patient, who obtained it from a medical friend in Chile.

It consists of a central core, a nugget of pure silver, surrounded by a shell of tallow. It measures 4 in. by $2\frac{1}{2}$ in. by $2\frac{1}{2}$ in. and is marked on the outside by mouldings of the rectal mucous membrane. The miners in Chile spend their time devising means whereby they may smuggle silver ore out of the mines. Needless to say they are searched as they come out of the mines, so that it is impossible for them to secrete silver in their clothes. They therefore steal the tallow candles provided by the management for lighting during their toils, surround nuggets worth stealing with melted tallow and insert the "eggs" so formed up their rectums. After they have emerged from the mines and reached their homes they proceed to lay eggs, with considerable profit to themselves. The occupation is not however free from hazard. When gazing on this large specimen it is hard to believe that any human being could have thrust it through his own sphincter muscle, yet the feat was actually performed. The man walked out of the mine undetected with his ill-gotten gains inside him. When he came to try and "lay his egg" he found he was quite unable to do so, and suffered not only from intestinal obstruction but from retention of urine, the occurrence of which is my excuse for presenting the specimen before this Section. He therefore betook himself to the hospital, where a kindly surgeon relieved him of his egg by open incision. He was not allowed to retain his trophy, which now rests in the museum of the Royal College of Surgeons.

There is, I understand, a similar specimen in the museum of St. Bartholomew's Hospital, which perhaps Mr. Girling Ball can describe to us.

Mr. GIRLING BALL described the specimen at St. Bartholomew's Hospital, stating that it had a piece of string attached to it. The miner forgot about the string, which he left hanging out of the rectum. A watchful manager noted this curious phenomenon, pulled on the string and delivered the egg intact, to the great chagrin of the "hen."

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WAR SECTION.

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War Section.

President—Sir JOHN GOODWIN, K.C.B., C.M.G., D.S.O., A.M.S.

PRESIDENT'S ADDRESS:

The Aftermath of War with Reference to the Medical Service of the Army.

By Sir JOHN GOODWIN, K.C.B., C.M.G., D.S.O., A.M.S.

A BRIEF review of some of the steps which have been taken since the Armistice with regard to the Medical Service of the Army, may possibly be of interest to the Section.

On the cessation of active operations in 1918-19, a period of reconstruction in the Army necessarily commenced.

As regards the R.A.M.C., many desiderata had become self-evident, and amongst the most pressing of these were the following:—

(1) The release of temporarily commissioned officers as speedily as possible in order that they might resume their careers in civil life, and that the requirements of the civil population might also be met.

(2) The closure of hospitals as beds became vacant in order to reduce expenditure, and also to enable public buildings, &c., which had been used as military hospitals during the War to resume their normal functions.

(3) To make arrangements for the organization of a satisfactory dental service for the Army.

(4) In order that the lessons of the War should not be forgotten, it was necessary while the experience was fresh in our minds to amend and bring up to date all orders, regulations, war establishments, &c., so that these might be in accordance with modern requirements.

(5) The re-institution of classes of instruction for officers and men. Schools of instruction had necessarily been closed on the outbreak of war, and it was now very evident that post-graduate and other education must be commenced as soon as possible.

The above represent only a very few of the many problems which faced us.

The following were difficulties which had to be overcome:—

(1) "War weariness." After four and a half years' war every one was tired, and many were almost worn out consequent upon the incessant work, anxiety and responsibility.

(2) Death: loss by death of able and distinguished officers whose services would now have been invaluable.

(3) Retirements: on the cessation of war many officers retired.

(4) The urgent need for economy: it very soon became evident that economy must be exercised in every possible direction, and that no measure which involved more than a minimal expenditure could be entertained.

(5) There remains one item which, though one could hardly term it a difficulty, yet one must confess was answerable for an enormous expenditure of time, energy, and mental wear and tear.

I refer to the huge number of personal interviews, letters, &c., which had to be dealt with daily, the questions, both private and parliamentary, which had to be answered, and the thousand and one matters which arose every day and which had to be dealt with forthwith.

Criticism was always with us, much, indeed the majority of it, was kindly, just and helpful, and we welcomed it and profited by it, while as regards criticism of the other description, namely, that which is destructive, ill-informed, and of no help, well, a sense of humour often comes to one's aid and enables one with a smile to give a courteous reply.

The following is a very brief review of some of the measures which were taken in hand immediately after the Armistice.

(1) *Release of Officers.*—This presented considerable difficulties; it had to be carried out as speedily as was possible without dislocation of hospital arrangements. However, the number was steadily reduced from 13,000 at the time of the Armistice to 1,400 without any breakdown.

(2) *Reduction of Hospitals.*—At the time of the Armistice our hospital beds were in round numbers distributed over the world as follows:—

United Kingdom	364,000	Malta	8,000
France	156,000	Italy	9,000
Egypt	48,000	Mesopotamia	10,000
Salonika	42,000	North Russia	650
Total nearly	638,000		

While at that time we had 578,000 patients in medical charge.

From these numbers the difficulties which faced us as to demobilization, closure of hospitals, disposal of patients, &c., may readily be deduced.

Hospitals which had been established in educational buildings, such as schools, were closed early and, following on this, other hospitals were closed all over the United Kingdom. At the time of the Armistice, as just stated, we had more than 364,000 hospital beds in the United Kingdom alone; at present we have under 8,000 so that since the Armistice, we have in the United Kingdom alone reduced our hospital beds by 356,000.

(3) *Establishment of an Army Dental Corps*, which is doing excellent work. We have now more than sixty commissioned Dental Officers in the Army, and a large number of other ranks employed as dental mechanics.

(4) *Formation of Committees* to deal with the many questions relating to the amendment of existing regulations as to establishment, strengths of units, nature and scales of hospital equipment, stores, &c.

In connexion with this question I can never be too grateful to my great friend, the late Sir William Babbie, V.C., who threw himself whole-heartedly into this work and devoted to it all his time, energy and experience. As head of the chief Committee he performed work which was invaluable. On his lamented death the work was carried on by Major-General Guise Moores, and the labours of the Committee are now completed.

(5) *The Compilation of the Official History of the War.*—This was started under the editorship of Sir William Macpherson, and a very great deal of work has been carried out, with the result that the position as regards the Medical History of the War is at present as follows: (a) The first volume of the "General History," dealing with the Medical Services in the United Kingdom, Garrisons Overseas, and the Colonial Expeditions, which were the first to come to a victorious conclusion, is now bound for issue. (b) The first volume of the "Diseases of the War" is now almost ready for issue. (c) The first volume of the "Surgery of the War" is in galley proof. (d) The second volume of the "General History," dealing with the Medical Services of France, is in course of preparation and about one-third of the volume has been written. It is hoped that it will be completed early next year. (e) The second volumes of the "Diseases of the War" and of the "Surgery of the War," are only awaiting one or two chapters in order to enable them to be sent to press, the latter is

practically complete. (f) The material for the volumes on the "Hygiene of the War" is practically complete, and it is hoped to have the first volume prepared for the press by Sir William Horrocks early next year. (g) The volume on "Pathology" is not very far advanced, so far as the material in the office is concerned, but ten of the subjects have been written up and the MS. is in the office. (h) There is no indication as yet with regard to the volume on the "Statistics of the War," and it is doubtful whether complete statistics will be eventually obtained for all periods of the War and for each area of operations.

(6) *Reinstitution of Courses of Instruction for Officers and Men.*—It became evident that this would have to be undertaken without delay, for unquestionably during the five years of war, many officers must necessarily, and through no fault of their own, have become more or less "rusty" in some, if not all, of the professional subjects.

It was therefore decided that every effort should be made to offer all possible facilities for post-graduate study and clinical experience to officers, and for education and training of other ranks of the R.A.M.C.

The following steps were taken:—

(1) The Royal Army Medical College at Millbank was re-opened as an instructional centre, the courses of instruction and examinations being considerably amplified and—in my opinion—improved.

(2) It was felt that, while officers in the vicinity of London had very considerable facilities as regards obtaining clinical work, those who were stationed in the provinces were not always so fortunately situated. Accordingly all the larger provincial infirmaries and hospitals were approached in order to ascertain whether they would offer facilities, as regards clinical work and experience, to officers of the R.A.M.C. who happened to be stationed in their vicinity. Cordial assents were happily obtained from all.

(3) Schools of hygiene, at which instruction was carried out as regards officers and men of *all* branches of the Army, were established. It was originally intended that one such school should be established in each command, but, owing to financial considerations, the number has been reduced to three.

(4) Instruction and training in *administration* and *organization* has been carried out as before the War, at Aldershot. In addition certain R.A.M.C. officers were selected to go through courses of instruction at the School of Military Administration at Chisleton. This has been an unqualified success, the officers have all passed with credit. The lectures given at the school by the R.A.M.C. officers have been received with the warmest approval. This innovation should ensure that the effect and importance of the Medical Service in War is impressed on officers of all branches of the Army.

(5) No time was lost as regards the training of the N.C.O.'s and men of the R.A.M.C., and very large numbers of N.C.O.'s and men have been through classes of instruction, no less than 400 having been trained and qualified as nursing orderlies, X-ray and laboratory attendants, dispensers, mental attendants, &c. The numbers under training are being considerably increased.

Since the war over 100 officers have passed through the post-graduate classes at the R.A.M. College, Millbank; fifty-nine officers passed as "distinguished" in various subjects. It is hoped that all these officers may in the future obtain appointments as "specialists" in the subjects in which they have specialized.

In addition, several officers have presented themselves for examination in higher degrees and with good results, the following degrees having been

obtained by various officers: M.D., 7; M.B., 2; M.R.C.P., 3; M.Ch., 3; B.S., 2; D.T.M., 2; F.R.C.S., 2; D.P.H., 27 (out of 30 officers who presented themselves for examination); Psychological Medicine, 1; total, 49.

(6) An important recent concession has consisted in our having been granted an initial sum of £220, with a subsequent annual grant of £450, for the purpose of establishing professional libraries in all our larger hospitals in order that officers may have at hand the most recent works for reference. Helped by this grant I hope that in a few years' time every military hospital at home and abroad may have an adequate and up-to-date library.

(7) Directorates in Hygiene and Pathology were established in 1918, under Colonel Beveridge and Major-General Sir William Leishman. The importance of the work carried out by these Directorates cannot be over-estimated; the work has been very extensive, embracing almost every branch of Hygiene and Pathology, and is certainly of immense value to the Army.

(8) The posts of Consultant in Medicine and Consultant in Surgery have now been established; as in the case of the directorates just mentioned, these appointments will normally be held by officers of the rank of full Colonel. Should a more junior officer be appointed he will hold the temporary rank of full Colonel with the pay and allowances of that rank.

I would draw attention to one very important point, with regard to all these appointments, viz., Directors of Hygiene and Pathology and Consultants in Medicine and Surgery, and that is, that should any officer holding one of these appointments come up in due course for promotion to the rank of Major-General, he may be so promoted as supernumerary to the establishment of Major-Generals and continue to hold his appointment. Consequently, the rank of Major-General is now open to officers who specialize in Pathology, Hygiene, Medicine or Surgery.

(9) The R.A.M.C. War Museum has been the subject of an immense amount of discussion and correspondence which at last has resulted in what, I think, is a really satisfactory solution. The Treasury has donated a sum of £7,500 to the Royal College of Surgeons of England, and that body has generously agreed, for this sum, to house, maintain, care for and complete the preparation of the entire pathological collection of war specimens, and also certain selected facial and orthopædic exhibits.

This concludes a very brief review of the work carried out since the Armistice. With any hope of ensuring an efficient service in the future, it could hardly have been less; a critic might with justice say that it might well have been more.

I had feared that post-war weariness might have had a detrimental effect, and that a lack of enthusiasm and energy might very readily—one might almost say excusably—have been manifest amongst officers who had recently been through such a strenuous, trying and exhausting five years, but happily my apprehensions were unfounded.

I wish that more could have been accomplished, but not one-tenth of what has been done could have been possible had it not been for the loyal and unwearying hard work of all the officers concerned in the work of re-construction. Therefore I cannot help expressing my admiration for the way in which, on the termination of such a terrible five years, when everyone was weary to an inexpressible degree, both officers and men set themselves whole-heartedly to the task of re-constructing the Service, of making up for lost time and of profiting by the lessons of the war.

Such a fact is an example of our steadfast national character, and is of happy augury for the future.

War Section.

President—Sir JOHN GOODWIN, K.C.B., C.M.G., D.S.O., A.M.S.

Demonstration of Drawings of War Injuries of the Fundus Oculi.

By WILLIAM WALLACE, M.D.

(ABSTRACT.)

THE demonstration was illustrated by drawings made from wounded soldiers while Mr. Wallace was serving as eye specialist in the R.A.M.C. from 1915 to 1919. They form part of the collection which he has presented to the Army Medical War Museum, and will be deposited there as soon as the necessary arrangements are completed.

The series showed the appearances of ruptures of the choroid at various stages, beginning with those in which the apparent lesion was small, and proceeding to those of grave injury to the optic nerve. Reference was made to the hæmorrhage into the vitreous, which almost invariably prevented a clear view of the fundus till a considerable time had elapsed, but one picture was shown of a rupture in which the details were seen quite sharply on the tenth day after the wound. As the appearances of these cases altered with great rapidity, a note was made of the interval that had elapsed between the receipt of the wound and the drawing of the condition. Attention was called to the fact that in "through-and-through" wounds, in which the projectile had traversed both orbits or their immediate neighbourhood, it was not invariably the case that the eye at the wound of exit was that which sustained the most severe injury, the result being dependent upon the path taken by the projectile, as often erratic as it was direct. It was also pointed out that in those cases in which some vision remained, its amount or degree did not always correspond with the extent of the lesion as seen with the ophthalmoscope, vision being found where the appearances suggested complete loss. In addition to the drawings, the speaker demonstrated, by means of the epidiascope, one of the difficulties encountered in a study of those cases in which masses of proliferation came forward into the vitreous, only one plane of which could be focussed at a time. For this purpose he had modelled these masses in relief, and with the lantern as ophthalmoscope and the model as fundus, the members of the Section were able to appreciate the relation of these masses to the part of the retina that was intact. This "glyptic method of representation," which the speaker showed for the first time at the Congress of the Ophthalmological Society of the United Kingdom, of 1920, is as far as he is aware unique, and would be valuable for teaching purposes. Among the drawings was one showing conditions which came into view only by the employment of a colour-screen, which cut off the red end of the spectrum. The conviction was expressed that the alternative and indispensable use of a redless light would revolutionize

ophthalmoscopy and reveal conditions which were invisible with the present-day illuminants.

In conclusion, the speaker said that while he had taken upon himself the task of recording these appearances of war injuries for the benefit of those ophthalmologists who had not had the honour of serving in the R.A.M.C., he ventured to hope that investigators other than ophthalmologists might in time come to find in them evidence, if not proof, of theories concerning changes in tissues not exclusively pertaining to the eye.

Supplementing the drawings of war injuries, a number of illustrations of unusual pathological or congenital conditions were shown.

Medical Organization, with Special Reference to the Transportation of Wounded, in Open Warfare.

By H. W. GRATTAN, C.B.E., D.S.O., Colonel A.M.S.

IT was my intention to deal with the transportation of wounded from the point of view of a Division and Corps, but having found it impossible to crowd so important a subject into a short paper, I propose to confine my remarks chiefly to the medical organization and administration of the medical service of a division.

This paper is based on three years' experience on the Western Front as an Ambulance Commander, A.D.M.S. of a Division and D.D.M.S. of an Army Corps, and includes service in four different armies on many fronts extending from Ypres to St. Quentin where at one period the Sixth Division, of which I was A.D.M.S., held the post of honour on the right of the British Army next the French during our advance in the autumn of 1918.

During trench warfare, which is in reality siege warfare, the transportation of wounded did not present the variety of problems for solution as in a war of movement, where, during operations, the medical situation might change every hour or half hour. I would lay stress on the importance to all administrative medical officers of developing the faculty of anticipating the nature of medical problems in the field, and taking steps to cope with them before they actually occur; with the result that either the undesirable situation does not develop or if it does materialize, it does so in a modified form, and measures to combat it being in readiness are switched on without delay so that there is no check to the evacuation of the wounded.

In laying stress on the importance of foresight I do not refer to such routine matters as an adequate supply of stretchers, blankets, waterproof sheets, dressings, and means for reinforcing bearer personnel; but I refer to a general review of an area before a battle with the aid of a map, or better still, by reconnaissance of the ground, and with a knowledge of the Divisional Commander's intentions, to visualize the most likely outstanding medical difficulties as regards the evacuation of the wounded. In one instance it was fairly obvious that the evacuation of stretcher cases would be difficult as there was no road leading in the direction of our attack, all the roads running more or less at right angles to the line of our advance. Consequently the location of advanced dressing stations in suitable situations presented some difficulty, but this was overcome by locating them in a town (Le Cateau) outside the divisional area where the majority of roads converged. On another occasion the problem

for special consideration was the evacuation of walking wounded owing to the extent of the front held, paucity of motor lorries for walking wounded and the inconvenient route followed by the light railway. The problem was successfully dealt with by utilizing the light railway up to a point nearest to the C.W.W.S. and installing a "ferry" of lorries between that point and the divisional W.W.S., a distance of four miles.

Again, when personnel, equipment, or other form of medical assistance are required in the field, they are wanted at once, there must be no delay or the evacuation of the wounded may be delayed. By this I mean, that the medical personnel should always know the location of the nearest "échelon" of reserves, which can be drawn on in the event of their own reserves becoming dangerously depleted.

(I) When an A.M.D.S. first takes over his duties he may not fully realize the importance of *co-operation* and *co-ordination* with other members of the divisional staff, and with the officers commanding certain units of divisional troops, or how success in perfecting arrangements for the succour of the wounded is largely dependent on the liaison of the medical branch with other branches at divisional headquarters. The more this principle of liaison is encouraged and developed the higher the degree of medical efficiency attained, with consequent benefit to the wounded soldier and, incidentally, a lightening of the task of the Administrative Medical Officer.

In reality, divisional and corps headquarters consist of a number of specialists who control and deal with a number of different questions nearly all of which have a direct or indirect bearing on the succour of the wounded in battle. With regard to the tactical disposition of field ambulances the General Staff are interested in the position of main dressing stations, chiefly from the point of view of the possibility of obstruction to the passage of troops, ammunition and supplies by the movements of motor ambulances. In selecting sites for advanced dressing stations in open warfare we were given a free hand, but in trench warfare no new constructional work would be undertaken without conferring with the brigadier concerned and the General Staff.

The General Staff also deals with questions relating to Army Signals and controls the issue of field telephones and their location, with special reference to the possibility of the enemy obtaining information by tapping instruments in an unduly advanced position. The divisional main dressing station was invariably on the telephone and one of the advanced dressing stations as a rule whenever field telephones were in use. The issue of maps is also the province of the General Staff, and because it was considered that the authorized allotment to field ambulances was meagre, we never rested until we obtained a more liberal supply.

The Intelligence branch is also controlled by the General Staff and arrangements were made for the medical branch to be kept informed of the location of all enemy Medical posts which might be utilized as we advanced. The source of this information was obtained from aerial photographs and from interviews with recently captured prisoners. The allotment of fighting troops as bearers in case of necessity was also arranged by the General Staff.

Misunderstandings are nearly always due to faults on both sides. Such a misunderstanding once occurred between the Medical branch and the General Staff when I was A.D.M.S. of a division. When I first took over medical charge of a division the General Staff rather regarded the medical arrangements as automatic, and did not realize how important it was for me to have

information regarding pending operations at the earliest possible moment. When once the staff comprehended my difficulties everything worked smoothly.

There is no time to explain in detail how co-operation with all the other branches helped to simplify many medical problems—but my policy was to invoke the aid of all branches, as the wounded have a claim to help and sympathy from all.

The *extent of the front held by the division* varied. In March, 1917, the division took over a front of 11,000 yards (over 6 miles) from the double-crassier in Loos, to the Hulluch-Hohenzollern sector. At this time, however, the division was reinforced by a fourth brigade. At the battle of Cambrai (November, 1917) the division attacked on a front of 2,600 yards, while at the opening of the German offensive in March, 1918, the division was in the line, N.E. of Bapaume on a front of 4,500 yards. The depth of the area and distance of Divisional Headquarters from the front line varied from $3\frac{1}{2}$ to 6 or 7 miles.

(II) *The Field Ambulance*.—As the duties of ambulance commanders are difficult, trying and dangerous, my policy as an A.D.M.S. was to try to lighten their burden, and to be able to help them at once, with something more substantial than advice when difficulties arose. Knowledge that the A.D.M.S. can help in case of urgent necessity is a source of the greatest relief to ambulance commanders, and enables them to concentrate on local difficulties and speedily overcome them. One of the special troubles was the constant unavoidable changes in personnel—especially of medical officers—to replace casualties among regimental medical officers, or in response to orders to reinforce C.C.S.

It was realized that the highest standard of efficiency was best attained by decentralizing the various duties in the unit in every possible way, in order to leave the ambulance commander free to concentrate on the weak points in his unit and to give him time to think out methods of improving the existing plans of evacuation. Our aim was to organize the ambulances in such a way that the duties were carried out as well or almost as well when the Commanding Officer was absent.

With regard to their special duties ambulance commanders are specialists in evacuation from the forward area, and the duties include the perfecting of measures for combating shock and collapse, their aim being to get the patient to the C.C.S. within six hours of being wounded, taking every possible precaution to minimize the evil effects of shock.

Special stress should be laid on the importance of encouraging and developing initiative in all ranks of the medical service.

ALLOTMENT OF DUTIES TO FIELD AMBULANCES.

Among the important duties are included (a) the collection, treatment and evacuation of lying, and walking wounded and "gassed" cases; (b) the administration of main and advanced dressing stations, walking wounded stations and collecting posts; and (c) the control of motor and horse ambulances.

The A.D.M.S. has to decide how these various duties can be most efficiently carried out, in view of the special conditions which obtain at the time, and after I have dealt with the handling of the bearer divisions, I will show in the form of a diagram five different methods of allotting duties to field ambulance commanders.

The Bearer Division.—The efficient handling of the bearer divisions, although it often entails working under most dangerous conditions, is

simpler than the management of the tent divisions. The bearer division is a mobile formation and its duties and dispositions are not altered in any way whether Corps Medical Institutions¹ are established or not.

The two principal ways of controlling the three bearer divisions were: either to detail one ambulance commander to be in charge of the evacuation from the forward area and controlling all three bearer divisions; or to divide the responsibility between two ambulance commanders, placing one in charge of the evacuation from two brigades, and making the other responsible for the remaining brigade.

In every case each bearer division was commanded by an R.A.M.C. bearer officer known as the brigade bearer officer who worked under the orders of the ambulance commander concerned. Under these arrangements there were usually two officers available, including an ambulance commander in case of necessity, to direct and supervise the evacuation of casualties from any one brigade.

In deciding whether to entrust the whole of the forward evacuation to one ambulance commander or to two, the following points would be considered: If the division was on a narrow front of about 3,000 yards, with the initiative in our hands and one main channel of evacuation—then the best and most economical method was to employ only *one* ambulance commander to control the three bearer divisions. On the other hand, if the initiative was in the hands of the enemy, or if the tactical situation from a medical point of view was a difficult one, or if the front held by the division was 3,500 yards or more, or if there were two distinct channels of evacuation—one on either flank—then the best results were obtained by dividing this duty between *two* ambulance commanders.

The work of the bearers is the most important duty of the field ambulance, and however high the standard of surgical aid is at the advanced and main dressing stations, it cannot compensate for or neutralize the evil effects which result from failure on the part of the bearers to bring the patients back at the earliest possible moment.

Efficient bearer work entails: (*a*) Adequate means for keeping in constant touch with the regimental medical detachment; (*b*) reserves of personnel and equipment so placed that they are available immediately when required.

The weakest link in the chain of the bearer organization is at the regimental aid post, where a break or interruption in the evacuation of stretcher cases is most likely to occur, and in order to maintain communication with the constantly shifting regimental aid posts, the following measures were adopted:—From four to eight R.A.M.C. bearers were placed under the control of the regimental medical officer and were attached to the regimental aid post, and acted as bearers, guides and runners. If we liken the regimental stretcher bearers to the body of a kite, these four to eight R.A.M.C. bearers form the tail of the kite—the tail being inseparable from the body.

When the regimental aid post was moved forward and established in a new position, a squad of the attached R.A.M.C. bearers would take a case back to the former regimental aid post, which in turn became a relay post, and there the case would be handed over; and other bearers would proceed forward to the new regimental aid post, being guided by one or more of the R.A.M.C. bearers attached to it. The service of these guides was essential, especially during operations in open warfare at night.

¹ Corps main dressing stations, walking wounded stations, gas treatment centres, and sick collecting stations.

In addition, the brigade bearer officer, who was also a medical liaison officer, was attached to and lived at brigade headquarters; he was not, however, under the orders of the brigade, but was under the orders and control of the field ambulance commander concerned. This arrangement worked admirably in practice. The brigade bearer and liaison officer was in direct communication with battalion headquarters, and he received information as soon as it was received by the brigade—that is, before divisional headquarters and the A.D.M.S. were informed. In addition, his duties included constant visits to the regimental aid posts; keeping a strict watch over the supply of blankets and stretchers; sending back word through the car-loading post for any requirements, and always watching for a favourable opportunity to move the car-loading post to a more advanced position, in order to lighten the work of the bearers by substituting motor transport for hand carriage or wheeled stretcher as early as possible.

The general arrangement for the supply and dispositions of reserves of stretchers, blankets and bearer personnel during a battle is best shown by diagrams. A constant stream forward of stretchers and blankets must be maintained to take the place of those in use. The supply of this equipment has to be arranged on a definite plan, so that the regimental medical officer knows where he can obtain the necessary articles without delay.

If the A.D.M.S. holds up a portion of the bearers as a reserve, it makes it difficult for the ambulance commander in charge of evacuation of stretcher cases to arrange for reliefs and much needed rest, without which the system of evacuation would collapse. The best results were obtained by handing over all the bearers to the ambulance commander concerned, and as A.D.M.S. I held, as a reserve, 100 to 150 fighting troops, who were located as a rule in the vicinity of one of the field ambulance headquarters, the site being determined according to circumstances.

(III) Before active operations the following conferences were usually held: One between the divisional commander and his staff, another between the D.D.M.S. of the Corps and A.Ds.M.S. of divisions, and another between the A.D.M.S. and field ambulance commanders and seconds in command.

At the divisional commanders' conferences before an attack on Lens in 1917, a plasticine model to scale of the field of operations was shown, and it was of the greatest assistance to those concerned to visualize the position of our objectives and the nature of the obstacles to be overcome; the various lines of trenches, the belts of wire, the contours, and the position of the roads or tracks.

In open warfare, however, it was never possible to have models of this description and we had to be content with maps. At these conferences the A.D.M.S. would learn what our objectives were; the boundaries of our area before and after the advance; where the greatest resistance would be expected, and consequently the heaviest casualties; what roads or tracks would be used by the artillery and infantry respectively; what roads it was the intention of the divisional commander to keep open as far as possible with all available labour; the situation of craters or other obstructions to transport.

At this conference the A.D.M.S. would decide with the divisional commander's approval what road would constitute the main channel of evacuation; and when this important point had been settled, the approximate sites of advanced dressing stations could be selected, and in this way the foundations of the medical arrangements laid. During the autumn of 1918 when our attacks succeeded one another at very short intervals (on some occasions without any interval at all) conferences were not always held and the A.D.M.S. would obtain the necessary information from the General Staff.

In connexion with the question of written orders the usual sequence in which orders and instructions were issued before operations was as follows:—

First, a warning order issued by the General Staff, then an R.A.M.C. or medical warning order issued by the A.D.M.S. to field ambulance commanders only, then the divisional operation order issued by the General Staff; on receipt of which the A.D.M.S. would issue the R.A.M.C. operation order to field ambulance commanders, copies being sent for information to G., Q., Brigade Headquarters, the D.D.M.S. of the corps, A.D.M.S. of divisions on either flank, the O.C. M.A.C., and when circumstances demanded to the divisional supply officer and the D.A.P.M.

Simultaneously the A.D.M.S. would draw up a concise draft of the medical arrangements for insertion in divisional administrative instructions which were issued by the A.Q.M.G.

Before discussing the scheme of medical arrangements in open warfare I must refer to two points, the importance of which must be realized by the administrative medical officer, otherwise the medical units under his command may be the means of imperilling the success of the whole military operations.

They are secrecy and the maintenance of a high standard of traffic and march discipline. Speaking as a non-combatant it appeared to me that military success in open warfare depended more on efficient traffic and march discipline than on any other factors; without them ammunition and supplies cannot reach the troops and the heavy artillery cannot advance, and incidentally, the transportation of wounded becomes a very serious problem. March discipline has been described as the ceremonial of war—interpreted in medical terms it meant, never under any circumstances to allow the field ambulance transport to be overloaded, as an overloaded wagon may break down and become the starting point of a serious block in the traffic.

As regards traffic discipline it meant not only complying with the instructions laid down for traffic on the various routes, but it also meant strict compliance with the order to keep an interval of so many yards between every twenty vehicles when the transport of a column was on the line of march.

The personnel of the field ambulances grasped the importance of these principles so well that on no single occasion did I hear any complaint on this score. The practical outcome as regards the wounded was that in open warfare we were given a free hand as regards the position of medical posts including main dressing stations, where the movements of the divisional motor ambulances and the M.A.C. might be the cause of serious trouble, unless suitable arrangements for traffic had been made beforehand, including a separate entrance and exit to the main dressing station and ample space for parking ambulances.

I may mention that I have seen, in the course of operations, a first-class road for "two-way" traffic entirely blocked by wheeled transport for a distance of over seven miles. This unfortunate incident occurred in a column of one of our allies and I understood that the road remained blocked for about twenty-four hours.

(IV) (a) We will now consider the medical arrangements in an *attack with a subsequent advance of some miles*. Experience showed that these conditions were the least difficult in which to organize satisfactory medical arrangements, in spite of the following complications: (1) The communications having become lengthened; (2) The size of the divisional area having increased considerably; (3) The blowing-up or obstruction of roads.

The reasons why these conditions were less difficult to deal with than those involved in a hostile attack or counter attack, were as follows:—

(1) The initiative being in our hands, the A.D.M.S. made his plans, and informed all concerned what those plans were, with due regard to secrecy, and he carried them out resolutely to the end.

(2) The difficulties due to lengthened communications were neutralized by the advantages accruing from motor transport, both for the evacuation of wounded and for bringing forward medical personnel and equipment, and also for dealing with the problem of obstructions and defects in roads, as R.E. personnel, material and mechanical devices for removing obstructions can be brought up with a minimum of delay.

It is most important to draw up the broad outlines of your scheme of transportation and to decide what is to be your main channel of evacuation during the advance, and after the objectives have been taken, bearing in mind that it economizes personnel to have *one* divisional route of evacuation, although under certain circumstances it may be necessary to have two.

Knowledge of the situation of this route or channel will enable those interested to grasp at once the foundation on which the scheme for rendering medical aid is based, and bearer officers and regimental medical officers can formulate and co-ordinate their plans, with the object of bringing the stretcher cases to a series of definite points, if possible in a direction towards the main channel of evacuation.

In an attack arrangements would be made for three advanced dressing stations, staffed and equipped by three tent subdivisions. The first would be open and working before the attack, the second would be in readiness to advance immediately when ordered by the A.D.M.S., while the third would be held in reserve. The administration and handling of the advanced dressing stations would be allotted to *B* ambulance commander who would also control the fifteen divisional motor ambulances, less the six Ford cars which are apportioned to the three bearer divisions under the control of *A* ambulance commander. The arrangements for dealing with the walking wounded would be controlled by the third or *C* field ambulance commander with a complete tent division, all the horsed ambulances, and half-a-dozen motor lorries usually provided by the Corps—this ambulance commander would establish a divisional walking wounded station with two tent subdivisions, his third subdivision being utilized as a collecting post, which is the counterpart of the A.D.S. for stretcher cases.

As the troops advance the collecting post is expanded into a new divisional walking wounded station and a more advanced collecting post established by one of the three tent subdivisions allotted for this purpose.

(b) *The Hostile Counter-attack.*— My conception of the medical arrangements necessary in the event of a counter-attack consisted in the closing of the most forward advanced dressing station, in the opening of another in a position 1,000 yards or so farther back, and also in the moving up of more bearers.

Additional arrangements which proved to be necessary on one occasion, involved the establishment of an entirely new route of evacuation at right angles to our original main channel and 5 miles distant from and parallel with our front. Another unforeseen complication occurred, namely, the fact that the transport of the field ambulance on the right narrowly escaped capture. We were all taken unawares as the counter-attack came in the wrong place and on the wrong day.

(c) Only brief reference can be made to the medical problems that have to be considered during an intensive and continued *hostile attack* like the German offensive in the spring of 1918. The administration of the medical services under these conditions was very difficult. From the point of view of a division and corps it is *the* situation in which a medical catastrophe is most likely to occur. The circumstances are difficult for many obvious reasons, for when the initiative is in the hands of the enemy, we do not know what his plans and objectives are. In my experience the special medical problem for consideration was the transportation of stretcher cases to the C.C.S. from the M.D.S. or A.D.S. and the calamity to be guarded against was the capture of numbers of lying cases. This we were successful in avoiding, with the exception that one advanced dressing station was captured. The paucity of information regarding the military situation and the hindrance to communication by signal or otherwise owing to acts of the enemy all added to our difficulties. In addition, motor lorries that were usually allotted for the conveyance of walking wounded were urgently required for other purposes; there was the likelihood that fighting troops as additional bearers might not be forthcoming, and the "fog" of war added to the general embarrassment. The general principle in handling the medical units of a division in these circumstances is to establish a series of advanced dressing stations and walking wounded collecting posts in échelon, and to inform ambulance commanders that they must delegate the responsibility of vacating these advanced posts, and withdrawing to the next échelon, to the officers in charge of the post.

The main dressing station must similarly be organized in échelon with one tent subdivision ready to open up and carry on in the new position while the remaining tent subdivisions close and prepare to withdraw. As the work of the bearers presents special difficulties the task of evacuation from the forward area should be handed over to two ambulance commanders. Owing to the difficulty of communications, it is of special importance for either the A.D.M.S. or the D.A.D.M.S. to make frequent visits to the various posts, in order to keep in touch with the needs of the constantly changing situation.

For want of time I am unable to discuss the rôle of an A.D.M.S. during a battle, or to deal with the organization of Corps Medical Institutions, relative to their advantages and disadvantages, and the circumstances which indicate or contra-indicate their employment.

DISCUSSION.

Mr. C. MAX PAGE said that the author had presented the details of an important subject in a clear and interesting way. He (Mr. Page) felt that the uncertain link in the chain of evacuation was always bound to lie in the sector between the Regimental Aid Post and the A.D.S. The common experience had been that the transport of the wounded man in this area was always greatly facilitated when motor ambulances could be pushed close up to the firing line. Such an arrangement also correspondingly economized the use of bearers. He felt that in any future war an effort should be made to supply splinter-proof ambulances or perhaps caterpillar trucks to assist the medical evacuation of the forward area. He asked Colonel Grattan for his opinion as to the practicability of such a proposition.

Lieutenant-Colonel P. H. HENDERSON said he agreed with the last speaker in regard to sending the ambulance cars as far forward as was reasonably possible in order to collect severely wounded during an engagement. In this connexion, up to the time he left France at the end of 1915, he was afraid there appeared to be a tendency to take too much care of the cars, and in consequence their usefulness was limited.

Cars could be more easily repaired and replaced than wounded soldiers. During the Battle of Loos unnecessary caution was disregarded in the Seventh Division and the cars were eventually sent along the Hulluch road up to and in front of the A.D.S., and thus the lives of men, who could not have been got away with reasonable expedition by any other means at the disposal of the Division, were saved. By pushing the cars forward one also saved the lives and conserved the energies of the over-worked stretcher-bearers. Colonel Grattan indicated at the beginning of his address an instance in which evacuation of wounded was rendered difficult by the fact that roads ran at right angles to the line of advance. Had aeroplanes ever been used for evacuation of wounded? He (Colonel Henderson) was of opinion that the solution of this difficulty and of the problem of getting severely wounded men and particularly abdominal cases to a surgical centre or hospital in sufficient time to save their lives lay in the use of aeroplanes. He would not suggest now the nearest point to the firing line to which an aeroplane might proceed to pick up wounded; that could only be determined on the spot and must be regulated by local conditions. It would save many lives if cases could be got to a surgical centre in two or three hours as compared with the six hours which Colonel Grattan indicated as the time arrived at as being the best that could be hoped for under our present means of transport. The address dealt entirely with conditions prevailing on the Western Front where the maximum frontage of a division was about 6 to 7 miles. In this respect and in many others the conditions differed very widely from those met with in other theatres of war—in Macedonia for instance, the frontage of some divisions was nearer 40 miles. It was suggested that the conditions under which future wars would probably be fought were more likely to approximate to those met with in the campaigns in Macedonia, Mesopotamia, Palestine and the Indian Frontier, and it was therefore unwise to base one's plans for future wars too closely on the experiences of the Western Front. For instance, in the later stages of the war in France the D.D.M.S. of the Corps selected the sites of the main dressing stations of the Divisional Field Ambulances. This procedure would be impossible on the extended fronts and with the means of communication met with, say, in Macedonia. Another point of importance for consideration was the question of the most suitable means of transport, particularly in divisional areas where there might be no roads for ambulance cars or even tracks suitable for horsed wagons. Under such conditions litters and travois had to be used and here a word of warning with reward to travois was necessary. Whilst they were a comfortable means of transport across suitable country they had the grave defect of being very apt to pick up and cut—particularly at night—the ground wires laid during an advance. Several instances of this had occurred during the war and at a critical stage of operations Divisional Headquarters had been found to be cut off from communication with Brigade Headquarters. Colonel Grattan had demonstrated the importance of rapid evacuation of casualties from the battlefield and had shown how he carried this out by means of collecting an ample supply of stretchers in the aid posts, advanced dressing stations and divisional area, and by means of establishing a system of communication between the original aid post and newly formed ones. This was all most important. The method of keeping up communication he (Colonel Henderson) had adopted in the Division in which he was A.D.M.S. had worked successfully. It was as follows: All roads and tracks from the unit aid posts to advanced dressing stations, and to divisional collecting station and main dressing stations were carefully marked by notice boards and arrows which were placed in position on the evening before the engagement started. At the commencement of the engagement the regimental sanitary detachments, which were normally idle during active fighting, were attached to the Regimental Medical Officer and each carried a small bundle of Red Cross flags made from triangular bandages. These were secured to light sticks about four feet long. As a battalion advanced and new aid posts and collecting points were formed one of these flagged sticks was stuck in the ground to mark the site and one man of the sanitary detachment then proceeded to the original aid post or to the advanced dressing station and led the field ambulance stretcher bearers to the new points where wounded were collected by the regimental bearers. This system had worked so well that it was well worth a trial by regimental medical officers.

Major-General Sir W. MACPHERSON said that Colonel Grattan had led him to understand that preparations in the way of stretchers, blankets, personnel, &c., were of minor importance, but he (Sir William Macpherson) could not imagine any greater preparations for battle on the part of the administrative medical authorities than those for replenishing stretchers, blankets and personnel. Those were the very details which should be carefully considered and prepared for in anticipation of battle. The maintenance of secrecy with regard to the arrangements was highly important, and in this respect he had found the General Staff much more anxious to let the administrative medical officer know sufficiently early the nature of the operations that were contemplated. Personally he had always received from the General Staff, from the Army Commander under whom he had served, and from the Commander-in-Chief, the very greatest help in this respect. They had trusted one to maintain secrecy; but he had not been kept so well informed by the Adjutant-General's branch; no doubt the Adjutant-General's branch had not the same faith in the maintenance of secrecy as the General Staff. Colonel Grattan had informed them that in his division 1,000 stretchers and 2,000 blankets were maintained. How had Colonel Grattan managed to carry these in the event of an advance or retreat? He seemed to have accumulated an unnecessarily large number of these articles; the procedure for replenishing stretchers and blankets which had been laid down by Corps and Army Ds.M.S. would have sufficed to keep Colonel Grattan supplied with whatever he wanted. At the battle of Loos he (Sir William Macpherson) had found some divisions doing what Colonel Grattan apparently had done, and forming large dumps of stretchers, with the result that the dumps arranged by the D.M.S. for replenishing the field ambulances were rapidly exhausted and had to be restored. Before this battle every field ambulance had been given, to begin with, fifty extra stretchers, and they were replenished by the motor ambulance cars of the motor ambulance convoy bringing up stretchers from the dumps at the casualty clearing stations to the field ambulances as required. The system had worked well, and there had been no complaint of want of stretchers. Colonel Grattan had made the remark that it was best to maintain only one route of evacuation for a division, but this he (Sir William Macpherson) thought was a mistake. Colonel Grattan had shown them the effect of having only one route of evacuation at the time of the German counter attack from Cambrai in 1917, and had said that the attack and the direction in which it was made were unexpected. He (Sir William Macpherson) would have thought, from the position of the salient on the flank of the divisional route of evacuation, that that was exactly the direction in which the attack would have been made, and in these circumstances it would have been far better, instead of depending on one divisional route of evacuation, to have arranged for collateral routes. Colonel Henderson had somewhat severely criticized the practice of not allowing motor ambulance cars to come up to regimental aid posts; but Sir William Macpherson considered that the sending of motor ambulance cars up to regimental aid posts was unjustifiable. The life of the car and the lives of the drivers and the wounded in it had to be considered, and although it might be possible to get away very quickly some severely wounded from a regimental aid post by means of motor ambulance cars, the chances were that there would be a greater loss of life and greater interference with evacuation by having cars damaged; this had occurred on more than one occasion during the battles on the Ypres salient. It was always better to bring back wounded from regimental aid posts by relays of bearers, light railways, or on wheeled stretchers to the advanced dressing stations, than to run the risk of losing motor ambulance cars by sending them too far forward. Relays of bearers, &c., could always come back over unsafe ground with less chances of being hit than could an ambulance car working on a recognized road where it was bound to be heavily shelled.

Major T. B. LAYTON thought that Colonel Grattan's paper showed the protean nature of modern warfare. It would be well to subdivide open warfare into three: (a) Semi-mobile, as instanced by the last stages of the war in France; (b) mobile, as instanced by Allenby's campaign in the autumn of 1917; and (c) very mobile, as exemplified by the leap to Aleppo in 1918. Colonel Grattan had rightly laid stress on the six-hour ideal of wound to operating table: but in all their advances in the late war he (Major Layton) doubted whether there was any improvement in this on the South

African war in a mobile or very mobile campaign. He felt confident that rapid transport could only be attained by collection of wounded by aëroplane, or by the pushing up of mobile operating units on tank transport to the field of battle on the evening of the engagement. He thought that warfare in the future would become more and more mobile, and urged that the study of the lessons of the war derived from the campaigns in Salonica, Mesopotamia, Palestine and Africa were of greater importance than the lessons derivable from France. He feared that these Near East and African campaigns were being neglected in comparison with that on the Western Front.

Colonel GRATTAN (in reply) said he considered Mr. Max Page's suggestions were practical, and he felt sure they would receive consideration by the authorities concerned. In regard to Colonel Henderson's observations, he (Colonel Grattan) explained that his paper was based on experience on the Western Front only. He must have expressed himself badly if he had conveyed the impression to Major-General Sir Wm. Macpherson that the provision of an adequate supply of stretchers and bearer personnel was of minor importance. He (Colonel Grattan) had stated that these were routine matters, nevertheless he considered them so important that he drew up a definite plan for the supply of these requirements before every battle, and had shown diagrams during the course of the evening which illustrated the methods he adopted to keep the regimental medical officer supplied with stretchers, and the brigade bearer officers with reinforcements of bearer personnel in case of necessity. The stretchers and blankets surplus to mobilization equipment were carried on lorries supplied by the D.D.M.S. of the Corps or by Divisional Headquarters. The direction of the hostile counter-attack at Cambrai should have been anticipated, and suitable arrangements made accordingly. He thought Major Layton's suggestions were most valuable, and he felt sure that they would not be lost sight of.

War Section.

President—Sir JOHN GOODWIN, K.C.B., C.M.G., D.S.O., A.M.S.

Stature in Relation to Physical Standards of Fitness.

By Brevet-Lieutenant-Colonel C. R. SYLVESTER-BRADLEY,
R.A.M.C.

IN 1920 the Ministry of National Service published their Report, vol. i, for the period 1917-19, when conscription was in force. That the Ministry of National Service had a unique opportunity for carrying out research on the national physique, no one will question; nevertheless, the report is very disappointing, and if we neglect the highly instructive parts dealing with the incidence of disease and racial deterioration from this cause, we are left with a large number of statistical tables, which appear to be of little value in estimating the normal physical development of the race. The only definite conclusion, in this respect, arrived at by the Committee is best stated in their own words:—

“In the light of these returns we may infer that a combination of height, 5 ft. 5 in., weight, 116 lb., and chest girth, 32 in., represents the average standard which the youths of the North-western and West Midland regions attain at 18 years of age. We cannot doubt that these proportions would be greatly improved by better conditions of life and environment during childhood and boyhood, and especially (as the results of Army training have demonstrated) by better food and better opportunities for physical development. There is as yet no accepted standard of physique for youths or men whatever their age, but (in addition to the observations on youths of 18 already quoted) there is already sufficient evidence to show that a combination of height 5 ft. 6 in., weight 130 lb., chest girth 34 in., will be found to be approximately the average measurements of the Grade I men of military age. It is impossible to regard either of these standards as satisfactory, or to rest content with the economic and social conditions which are responsible for such a result.

“If such measurements represent truly the physique of our manhood of military age—as there is every reason to believe they do—we cannot be surprised at the general grading results throughout the country. We may well be surprised that, with human material of such physique, it was found possible to create the armies which overthrew the Germans, and proved invincible in every theatre of war. One cannot but feel that the spirit of the race, which alone made this possible, deserves that no effort should be spared to ameliorate the conditions which have brought about such deplorable effects upon its health and physique.”

From the foregoing remarks I think it is fair to conclude that the Ministry of National Service had come to the decision that the national physique had deteriorated to such an extent, that it was surprising, “with the human material available, it was possible to create the armies which overthrew the

Germans." That the Ministry of National Service had a considerable amount of evidence which apparently supported this conclusion I do not wish to dispute; nevertheless, as the question of physique is so extremely important in connexion with recruiting for the various public services, I do not think the time will be wasted if we consider the whole question on rather broader lines than those adopted by the Ministry of National Service. Let me, however, make it quite clear that we shall be dealing solely with the question of physique, and not racial deterioration due to disease.

CAUSES OF VARIATION.

The genus man may be regarded as consisting of one species divided into a large number of races differing widely in individual characteristics. Variation exists in colour, physique, rate of growth, mental capacity, and other characteristics too numerous to mention, and the more civilized the race the greater the individual variation. Speaking broadly, variation of individuals of the same race must be looked upon as one of the chief factors in the evolutionary process, but, as Darwin [2] pointed out in his "Origin of Species," variability is always more marked in civilized beings, domesticated animals, and in plants under cultivation than in those which exist in the wild state. In the wild state, a large number of individuals may present characteristics common to the race, whilst under domestication or civilization, cross fertilization by members of the same species but of different races is common, with a mixing of racial characteristics and increased variability, variation often becoming "continuous."

When considering the effects of civilization on the human species, we find that there has been a widespread intermixing of different races, until at the present time few can be regarded other than as races of mongrels; normal racial individuality has in some instances entirely disappeared, and if we visualize the population of the British Isles, we are confronted with a motley crowd of individuals presenting every degree of "continuous" and "discontinuous" variation.

Additional factors in producing increased variability in civilized countries are the suspense or partial suspense of what Darwin and Wallace termed "natural selection," and, possibly, unfavourable changes in environment, such as occur in our large manufacturing towns and mining districts.

The initial cause of variation, apart from the intermixing of the races, is still the subject of a good deal of controversy. It has not been conclusively proved that acquired characteristics in the parent are transmitted to the offspring, and whilst it is obvious that nutrition and environment must have considerable effect in determining such a characteristic as the size of the individual, the fact remains, we must look to inherent characteristics as being the chief factor in causing individual variation.

It is affirmed by some authorities that the stunting and physical unfitness, so noticeable in our slums, is not due (or only to a small extent) to the effects of environment being transmitted by the parent to the children, but rather to the fact that the physically defective gravitate to the slums, where they intermarry, and transmit their inherent defects to their offspring, the latter being protected and preserved by our code of modern civilization to perpetuate and multiply their unfitness.

Taking it as granted that mankind is subject to the same individual and racial variations as any other species, let us examine the evidence we possess of variations in physique that have occurred in the past, and see if they present

any great change from the variations which are occurring at the present day. We will deal, first of all, with individual abnormalities or mutations. It is interesting to note how legend and history abound in the mention of giants and dwarfs. Most of us will associate our earliest recollections in the nursery with tales of giants and goblins—the ancients were of opinion that the human species had descended from giants. When, however, we come to investigate these legendary accounts of giants we find there is little, if any, evidence to show that giants were larger or more numerous in the earliest days of history than they are at the present time, and it is probable that many of the ancient legends of men of enormous stature had their origin in the remains of prehistoric monsters being mistaken for the bones of human beings. We find several mentions of giants in the Bible, notably Goliath of Gath who was slain by David, and who is stated to have measured 6 cubits and a span, which, assuming the cubit to be the cubit of a man (i.e., the length from the tip of the middle finger to the point of the elbow) would make Goliath 9 ft. and 9 in. in stature, not a very extraordinary height for a giant, even if we make no allowance for exaggeration by the narrator. Pliny quotes an Arabian giant named Gabbaras, who was 9 ft. 5½ in. high according to English measurement. The Emperor Maximinus, A.D. 235, was stated to have been between 8 and 9 ft. high, and also to have been of great bulk, but we do not possess any absolutely authentic statement of the measurements of giants until we approach more recent history, when we have the following definite records: (1) John Middleton, of Hale, 1578, measured 9 ft. 3 in. (2) Patrick Cottler, an Irishman, died at Clifton in 1802, and measured 8 ft. 7 in. (3) O'Brien, the famous Irish giant, whose skeleton is preserved in the Museum of the Royal College of Surgeons, was 8 ft. 4 in. (4) Joseph Winkelmaier, an Austrian, who was exhibited in London in 1887, was 8 ft. 9 in. (5) Machnow, a Russian born at Charkow, measured 9 ft. 3 in. at 23 years old, and weighed 360 lb.

Stories of individual dwarfs have not been preserved in ancient history to the same extent as those of giants. Dwarfs, from the earliest times, have attracted the attention of kings, as, for instance, the tiny Akkas at the courts of the Pharaohs; and it is interesting to note that the Romans practised artificial dwarfing, and one of the most efficacious recipes, according to report, was to anoint the backbone of the child with the grease of moles, bats and dormice. The earliest authentic English dwarf is recorded as being page to Queen Mary I, and to have measured 24 in. There is also the celebrated Jeffery Hudson, 1619-82, the son of a Rutlandshire butcher; he only measured 18 in. at 9 years of age, and history relates how he was served up in a cold pie at a dinner given by the Duke of Buckingham to Charles I. Unfortunately, he started to grow when he was 30 years old, and eventually became 45 in. tall: he died at the age of 63. Another celebrated European dwarf was Richebourg, who died in Paris in 1858, at the age of 90; he only measured 23 in. high.

From the above records it would appear that abnormalities of stature are just as frequent at the present time as they were in the past, also that the extremes of individual variation have remained fairly constant during the passage of some thousands of years.

Turning to the question of racial variations in stature, whilst we find distinct evidence of pygmy races having existed at the time of the earliest Egyptian dynasties and tradition, that races of very small men existed in prehistoric times, there is no such evidence of the existence of races of giants.

True, races of men well above the average stature have existed, and do exist still, but their height is not so great as to entitle them to be called "giants." Magellan, when he first discovered Patagonia, described the natives as averaging 7 to 8 ft. high, but his first impressions were found to be totally inaccurate and although the Patagonians still rank as one of the tallest races in the world, their average height does not in all probability exceed 70 in.

Some interesting calculations have been made as to the average height of prehistoric man. Professor Keith estimates the height of the fossil man of Java at 66 in. The Neanderthal man stood about 64 in., and De Quatrefages puts the Cro-Magnon race down as averaging 70 in.

Modern races present considerable variation as to the average stature of their males. Weisbach estimated the average stature of the Bosjesmans as nearly 54 in. The Akkas or Tikki Tikkis, a pygmy race of Central Africa, are stated to have an average height of 55 in., whilst some of the Polynesian races have (or rather had before they became degraded from contact with European civilization), an average stature of over 70 in. and are reputed to have possessed the finest physique of any race in the world.

Keith quotes the following figures for the British Isles: commonalty, 67 in.; middle class, 68 in.: Oxford students, 68'5 in.

The comparative stature of soldiers of different nations is also instructive. Average stature of soldiers: Italians, 63'8 in.; Germans, 66'7 in.; English, 66'9 in.; Irish, 67'2 in.; Scotch, 67'4 in.; American, 68'3 in.

The average height of recruits for twenty years before 1914 was 66'1 in. The average height of recruits, 1920-1: infantry recruits, 65'4 in.; all recruits, 66'3 in.

Average height of professional football players (Wolverhampton Wanderers and Sheffield United), 66'7 in.

A critical examination of the foregoing figures reveals the following facts:—

(1) That different races and individuals of the same race normally show considerable variation as to their stature.

(2) We may assume that 66 in. is approximately the average height for males of the general population in these Isles.

(3) There does not appear to be sufficient ground for stating that the national physique, as regards stature, has greatly diminished in recent years, nor that the stature of man in general shows any tendency to increase or decrease.

Professor Keith [5] sums up the whole question as follows: "The evidence so far leads us to believe that our present stature and size of body are part of an ancient inheritance, one which has not been altered by the passage of hundreds of thousands of years."

I do not wish to labour this point of individual variations in stature. At the same time I want to emphasize the fact that any body of men of the same age will normally differ very considerably as to their height, and further that if the average height of this body of men is shown in relation to their age, it does not follow that the average height of a second or third body of men of the same age will conform to the average height of the first. In other words the practice of trying to correlate physical characteristics to age is valueless from a statistical point of view and the results arrived at by such a method are totally misleading.

An illustration from some of the statistical tables in the Report of the Ministry of National Service will make this point clear. In Table VIII the average heights of 146,128 men are tabulated according to their ages, and in Table IX their average weights are given in relation to their age.

TABLE VIII.—AVERAGE HEIGHTS BY AGES.*

Period—May 1 to October 31, 1918.

	Aged 18	Aged 19	Aged 20	Aged 21
Birmingham ...	64 $\frac{3}{4}$ in.	65 $\frac{1}{2}$ in.	66 in.	65 $\frac{1}{2}$ in.
Burslem ...	66 $\frac{3}{4}$ "	65 $\frac{3}{4}$ "	66 $\frac{1}{2}$ "	66 "
Worcester ...	64 "	65 $\frac{1}{2}$ "	66 "	66 "
Coventry ...	66 $\frac{1}{2}$ "	66 "	66 $\frac{1}{2}$ "	66 $\frac{1}{4}$ "
Shrewsbury ...	65 $\frac{3}{4}$ "	65 $\frac{1}{4}$ "	66 $\frac{1}{2}$ "	66 "
Hereford ...	66 $\frac{1}{2}$ "	63 $\frac{3}{4}$ "	64 $\frac{3}{4}$ "	65 $\frac{3}{4}$ "
Leamington ...	66 "	66 $\frac{1}{4}$ "	66 $\frac{3}{4}$ "	66 $\frac{1}{4}$ "
Walsall ...	65 $\frac{1}{2}$ "	66 "	66 "	66 "
Wolverhampton ...	64 "	65 $\frac{1}{4}$ "	66 $\frac{1}{4}$ "	66 "

TABLE IX.—AVERAGE WEIGHTS BY AGES.*

Period—May 1 to October 31, 1918.

	Aged 18	Aged 19	Aged 20	Aged 21
Birmingham ...	121 $\frac{1}{4}$ lb.	118 lb.	118 lb.	120 lb.
Burslem ...	118 "	120 $\frac{1}{2}$ "	125 $\frac{1}{4}$ "	122 "
Worcester ...	115 $\frac{1}{2}$ "	118 $\frac{1}{2}$ "	120 $\frac{1}{2}$ "	125 "
Coventry ...	124 $\frac{1}{4}$ "	120 $\frac{1}{4}$ "	122 $\frac{1}{2}$ "	121 "
Shrewsbury ...	120 "	123 $\frac{1}{2}$ "	126 $\frac{1}{2}$ "	128 $\frac{1}{4}$ "
Hereford ...	120 $\frac{1}{2}$ "	128 $\frac{1}{2}$ "	122 $\frac{1}{2}$ "	123 "
Leamington ...	118 $\frac{1}{2}$ "	116 "	123 $\frac{1}{2}$ "	125 $\frac{1}{4}$ "
Walsall ...	120 $\frac{1}{2}$ "	120 "	121 $\frac{1}{4}$ "	122 $\frac{1}{4}$ "
Wolverhampton ...	114 $\frac{1}{2}$ "	113 $\frac{1}{4}$ "	127 $\frac{1}{4}$ "	123 $\frac{1}{2}$ "

* Extracted from Report by Ministry of National Service.

The average height of Herefordshire youths of 18 is given as 66 $\frac{1}{2}$ in.; at 19 years, the average height of youths from the same area is only 63 $\frac{1}{2}$ in., whilst at the age of 29 they are only 62 $\frac{1}{4}$ in.

Similarly with regard to the weights, the average weight of Coventry youths of 18 is 124 $\frac{1}{4}$ lb.; at 19 years, youths from the same area are only 120 $\frac{1}{4}$ lb.; and at 21 years they are only 121 lb. Such results can only be explained by recognizing the fact that the method of tabulating the statistics is at fault.

The only scientific method of recording the physical characteristics of man is to correlate them to some fixed factor such as the length of the trunk or the stature. A general increase in size and weight takes place as a natural sequence to growth and advance in years but such increase is not in direct ratio to the age increment.

This matter is important when we bear in mind that most of our anthropometrists in the past have used age as the factor on which to base their statistics, and may account for the wide discrepancy which is so common in the tabulated results by different investigators.

Recently Professor Dreyer [4] has published a most valuable book "The Assessment of Physical Fitness," in which he gives tables showing the weight of the body, the normal chest circumference and the vital capacity, in what he considers to be their normal relation to the trunk height irrespective of age, his results being calculated to the eighth part of an inch and decimals of pounds.

Whilst admitting the general truth of Professor Dreyer's conclusions, I find it difficult to believe that it is possible to show individual characteristics actually correlated in such exact proportion as he asserts them to be, more especially as he makes no allowance for the physiological increase in weight after growth in stature has finished, nor does he allow for the fact that weight is normally variable in a healthy individual.

Professor Dreyer has discarded standing height or stature as a factor for his calculations on the assumption that sitting or "trunk and neck height" is more reliable, owing to individual variations in the length of the lower limbs. This assumption is not borne out completely, however, when we examine the measurements of the white and coloured soldiers on demobilization in America.

It will be noticed that whilst the average stature of white and coloured soldiers is practically the same, 68 in., the weight of the coloured soldier is 149.5 lb. to 144.6 lb. for the white. The arms and legs in the coloured soldier are approximately 1 in. longer than the white. The sitting height of the "whites" is some 2 in. longer than the "coloured," but this apparently greater trunk length in the whites is to some extent negated by the fact that the head and neck is approximately 1 in. longer in the "whites" than the "coloured," as can be noted by comparing the height of the sternal notch and the sitting height of the two races.

Taking these facts into consideration I do not think it is justifiable to condemn the standing height as a useless factor in the assessment of physical fitness, even if we agree that the trunk height may be less liable to variation.

NORMAL WEIGHT.

Given a minimum height-standard, weight is one of the most reliable factors in estimating physical efficiency; but before dealing with the highly important question of the normality of weight, and the correlation of weight to height, we must discuss briefly the variability of weight in the individual.

The maintenance of the normal weight in a healthy individual can only be brought about by "intake" balancing "excretion," and it follows that because "intake" and "excretion" are not accurately synchronized, variations of weight must occur in a perfectly healthy person.

Pfaundler and Schlossmann [6] found that the average excess of the evening weight over the morning weight (the evening weight being taken the last thing at night when retiring to bed and the morning weight on getting up and before the bladder was emptied) to be 1,000 grm. (approximately 2.2 lb.) but experiments conducted in this country on an adult weighing 160 lb. failed to make the average loss of weight during the night more than 1.5 lb., although on one occasion the loss was as high as 2.5 lb. It has also been proved that a weekly variation of from 3 to 4 lb. is quite compatible with health. Seasonal variations in weight are well recognized. Loss of weight due to sickness is clearly illustrated by a reference to the fortnightly weight charts of recruits under training. Admission to hospital for even minor ailments is nearly always followed by a drop in weight, and it is interesting to note that "absence without leave" has very much the same effect.

These facts being borne in mind, it follows that when we try to estimate normality of weight with reference to height we must be content if we arrive at a figure which will allow of a margin of 7 or 8 lb. for error.

For the purpose of comparison weights must, of course, be taken without clothes, and should all be taken at approximately the same time of the day, that is to say after or before a meal. Any individuals who have recently been suffering from any severe illness should be excluded.

Investigations have been carried out during the past year to ascertain if there is any evidence to prove that a true correlation exists between the weight and the *standing* height. Large numbers of boys and youths of the same

height have been weighed and the averages tabulated. From the results, a hypothetical table has been constructed showing a definite progressive increase in weight for each inch of height for youths of the general population up to the age of 21 years. After 21 years, or when a youth has finished growing in stature, he continues to put on weight physiologically, and by the time he has reached 30 years of age he may have added 30 lb. to the normal weight for his height at 21 years; but any excess over or above this figure must be considered as bordering upon the pathological.

Recruits for enlistment provide practically an unlimited supply of material for estimating the average weight in relation to the height from 60 in. upwards, and it will be seen that these figures conform very closely to the hypothetical figures.

Further investigations have been carried out to ascertain what is the relation of weight to the stature of recruits undergoing their training, and the results are interesting when contrasted with the figures on enlistment and the recommendations with regard to weight as shown in Army Recruiting Regulations.

CHEST MEASUREMENT.

The measurement of the expanded chest at the nipple line has been regarded for many years, especially in the Army, as a most reliable index of physical fitness; but when we come to inquire into the reasons for laying so much stress on this measurement, one is forced to the conclusion that no scientific reason *does* exist, and it would appear to be probable that the insistence on an abnormally large thoracic expansion is a relic of the days when physical efficiency was measured by the ability of a body of men on parade to hold their chest fixed in a position of forced inspiration.

It has already been stated that it should be possible to show all proportions of the body in close relationship to some fixed factor such as the trunk length or the stature, and if we could be certain of making accurately corresponding chest measurements, we should expect to find such measurements increasing in direct proportion to any increase of height and weight and thus providing useful data in estimating physical development.

Unfortunately we are handicapped in making accurately corresponding measurements of the chest owing to its expansibility. If, therefore, we adopt a chest measurement simply as a test of physique, better results would be obtained if the measurement of the chest at rest were taken; that is to say, whilst the subject is breathing naturally, with arms hanging loosely at the sides, and whilst he is engaged in conversation.

On the other hand if the chest girth when fully expanded, and the degree of expansion, are intended as an indication of "vital capacity," the margin of error becomes so great as to render this measurement useless. Lung expansion in the male is normally abdominal, complete thoracic expansion only being brought into action during violent exercise.

The degree of voluntary thoracic expansion is largely a matter of practice in the use of the inspiratory muscles and depends upon the angle of attachment of the ribs to the vertical lines of the spine and sternum respectively. If there is acute angulation, the chest may be small and capable of a high degree of thoracic expansion. If on the other hand the angulation approximates a right angle, the chest is near its maximum size and thoracic expansion is small. That individual peculiarities in the amount of chest expansion do occur and have little reference to physical efficiency may be illustrated by a

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comparison of the chest measurement of such fine athletes as Carpentier and Dempsey. Whilst Dempsey has a normal chest measurement of 42 in. with an expansion of 4 in., Carpentier has a normal chest measurement of 41 in. with only $2\frac{1}{2}$ in. expansion.

HYPOTHETICAL TABLE BASED ON OVER 5,000 OBSERVATIONS SHOWING VARIOUS STANDARDS OF NORMALITIES FOR MALES OF THE GENERAL POPULATION UNDER 21 YEARS OF AGE, CALCULATED FROM THE STATURE IN INCHES.

Stature	Sitting height	Weight without clothes	Chest measurement at rest	Food requirements in Calories	Approximate age
37 in. ...	21.74 in. ...	31.4 lb. ...	19.622 in. ...	1,156 C. ...	3 years
38 " ...	22.112 " ...	32.91 " ...	20.015 " ...	1,225 " ...	
39 " ...	22.486 " ...	34.53 " ...	20.409 " ...	1,296 " ...	4 "
40 " ...	22.862 " ...	36.26 " ...	20.804 " ...	1,369 " ...	
41 " ...	23.24 " ...	38.10 " ...	21.1 " ...	1,444 " ...	5 "
42 " ...	23.62 " ...	40.05 " ...	21.498 " ...	1,521 " ...	
43 " ...	24.002 " ...	42.11 " ...	21.886 " ...	1,600 " ...	6 "
44 " ...	24.386 " ...	44.28 " ...	22.285 " ...	1,681 " ...	
45 " ...	24.772 " ...	46.56 " ...	22.685 " ...	1,764 " ...	7 "
46 " ...	25.16 " ...	48.95 " ...	23.086 " ...	1,849 " ...	
47 " ...	25.55 " ...	51.45 " ...	23.488 " ...	1,936 " ...	8 "
48 " ...	25.942 " ...	54.06 " ...	23.891 " ...	2,025 " ...	
49 " ...	26.336 " ...	56.78 " ...	24.295 " ...	2,116 " ...	9 "
50 " ...	26.732 " ...	59.61 " ...	24.7 " ...	2,209 " ...	
51 " ...	27.13 " ...	62.55 " ...	25.106 " ...	2,304 " ...	10 "
52 " ...	27.53 " ...	65.60 " ...	25.513 " ...	2,401 " ...	
53 " ...	27.932 " ...	68.75 " ...	25.921 " ...	2,500 " ...	11 "
54 " ...	28.336 " ...	72.03 " ...	26.33 " ...	2,601 " ...	
55 " ...	28.742 " ...	75.41 " ...	26.72 " ...	2,704 " ...	12 "
56 " ...	29.15 " ...	78.9 " ...	27.131 " ...	2,809 " ...	
57 " ...	29.55 " ...	82.5 " ...	27.543 " ...	2,916 " ...	13 "
58 " ...	29.972 " ...	86.21 " ...	27.956 " ...	3,025 " ...	
59 " ...	30.386 " ...	90.03 " ...	28.37 " ...	3,136 " ...	14 "
60 " ...	30.802 " ...	93.96 " ...	28.785 " ...	3,249 " ...	
61 " ...	31.22 " ...	98.0 " ...	29.201 " ...	3,364 " ...	15 "
62 " ...	31.64 " ...	102.15 " ...	29.618 " ...	3,481 " ...	
63 " ...	32.062 " ...	106.41 " ...	30.036 " ...	3,600 " ...	16 "
64 " ...	32.486 " ...	110.78 " ...	30.455 " ...	3,721 " ...	
65 " ...	32.912 " ...	115.26 " ...	30.875 " ...	3,844 " ...	17 "
66 " ...	33.34 " ...	119.85 " ...	31.296 " ...	3,969 " ...	
67 " ...	33.77 " ...	124.55 " ...	31.718 " ...	4,096 " ...	18 "
68 " ...	34.202 " ...	129.36 " ...	32.141 " ...	4,225 " ...	
69 " ...	34.636 " ...	134.28 " ...	32.565 " ...	4,256 " ...	19 "
70 " ...	35.072 " ...	139.31 " ...	32.99 " ...	4,389 " ...	
71 " ...	35.51 " ...	144.45 " ...	33.416 " ...	4,524 " ...	20 "
72 " ...	36.05 " ...	149.7 " ...	33.843 " ...	4,661 " ...	
73 " ...	36.492 " ...	155.06 " ...	34.271 " ...	4,800 " ...	21 "

* Formula for estimating the daily food requirements of the average youth expending an average amount of energy ($H - 3E = C$ when "H" equals the stature in inches and "C" = total food requirement in large Calories.

CONCLUSION.

We have dealt at some length with a few selected standards of physique, and I would ask for your careful consideration of the following points:—

- (1) That the common practice of using age as the basis for tabulating anthropometrical statistics is fundamentally wrong.
- (2) That standards of physique may be very closely correlated to the trunk length or stature in the normal individual up to the age of 21 years.
- (3) That our physical standards of fitness for the enlistment of recruits into the various Services require considerable revision.

In conclusion, I would remind you that our standards of physique have been arrived at empirically, that is to say, on the false assumption that men

of equal physique should be capable of performing an equal amount of work in a given time; but physique is of no value to a soldier or sailor unless he possesses the vigour and mental capacity to carry out all the duties he may be asked to perform in actual warfare.

To obtain soldiers, sailors or airmen best fitted for the warfare of the future, the recruiting medical officer must be something more than the hide-bound automaton he has been in the past. He must be intimately acquainted with all the characteristics of normal man. To quote from Professor Karl Pearson in his Presidential Address to Section H of the British Association for the Advancement of Science, 1920: " ' Psycho-physical and psycho-physiological ' characters are of far greater weight in the struggle of nations to-day than the superficial measurements of man's body. Physique in the fullest sense, counts for something still, but it is physique as measured by health, not by stature or eye-colour. But character, strength of will, mental quickness count more, and if anthropometry is to be useful to the State it must turn from these rusty old weapons, these measurements of stature and records of eye-colour, to more certain appreciation of bodily health and mental aptitude, to what we term vigorimetry and to psychometry."

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DISCUSSION.

Colonel BERNARD MYERS stated that measurements of the circumference and lateral and longitudinal diameters of the head, the chest and the abdomen, and the intercrystal diameter, together with the standing and sitting heights, were made in all children at the Children's Clinic, Marylebone Road. The weights were also taken. He believed that gain in weight indicated a healthy state of nutrition in children, providing it were not due to œdema or such-like. He believed that weight should be always stated with height.

Lieutenant-Colonel W. SALISBURY SHARPE remarked (a) that the Ministry of National Service, which commenced operations as such late in 1917, was dealing with a residuum of the population from which all the best elements had already been drawn, and which had been "combed" over more than once. As its candidates, therefore, were men who had previously either been rejected or placed in very low categories, the low standard which the Ministry found was due to that fact and not necessarily to any marked deterioration of the race. (b) That the poor physique of youths arriving at the age of 18 shortly after the Armistice was largely due to their insufficient nourishment as growing lads during the rationing period.

Mr. R. TIMBERG asked: (1) Whether a genuine increase in the stature of women had not taken place during the last generation? His own impression was that this was the case, at least amongst the more favourably situated classes in this and other countries owing to the greater amount of exercise taken by women of the present day, their more healthy mode of living and the greater freedom which they were generally enjoying. (2) Whether any information could be given as to the variation in height in the same individual in the morning and evening? Lieutenant-Colonel Sylvester-Bradley had made the interesting statement that weight decreased by a pound or two

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during the night; but had he not found an opposite effect on the height, i.e., that this decreased towards the end of the day, obviously as the result of changes in the spinal column, the intervertebral discs becoming more compressed and the physiological curves of the spine increasing from the upright position assumed during the day?

Lieutenant-Colonel C. R. SYLVESTER-BRADLEY (in reply) stated that he had no statistics as to the stature of women, but he was prepared to admit that it was possible that a proportion of the women of the present generation had increased in stature, owing to the influence of selection and the more healthy mode of living adopted by the women of the upper and middle classes. He did not, however, consider that the average stature of women as a whole had materially increased. With regard to variation in height in the same individual in the morning and evening, this was a well-recognized fact due to the conditions suggested by Mr. Timberg, but the variation was so small that he did not think his omission of the statement of this fact would affect the validity of the subject matter of his paper.

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